THE SCIENTIFIC PAPERS OF THE HONOURABLE HENRY CAVENDISH, F.R.S.

THE ELECTRICAL, CHEMICAL, AND DYNAMICAL RESEARCHES

Edited from the Published Papers, and the Cavendish Manuscripts in the possession of
His GRACE THE DUKE OF DEVONSHIRE, K.G., F.R.S.
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PREFACE

In pursuance of the policy of the Cambridge University Press to render available in collected form the occasional writings of men eminent in science and learning who have been associated with the University, a complete edition of the works of Henry Cavendish has long been contemplated. This project was naturally suggested by the position of Cavendish as one of the greatest of British men of science, and by the fact that his manuscripts were known to contain very important contributions to knowledge which had never been published to the world: in justice to his fame, and in the interests of the history of science, it was desirable that they should see the light. It was, moreover, an obligation which had very special claims on Cambridge on account of the intimate connection of the House of Cavendish with the University, maintained now through many generations.

As regards what was considered the more pressing part, the Electrical Researches, this duty was discharged with signal efficiency by the late Professor Clerk Maxwell, in a volume published by the University Press in 1879, which now, in revised form, constitutes Vol. I of this complete edition.

With the concurrence of the Duke of Devonshire, who again allowed the use of Henry Cavendish's manuscripts preserved at Chatsworth, the task of preparing for press the researches, published and unpublished, other than electrical, was begun some years ago; but the printing has been greatly delayed owing to circumstances arising out of the war. This portion of the work now forms Vol. II of the complete edition. It is largely, although by no means exclusively, concerned with chemical subjects. In the history of science it has been the custom to regard Cavendish mainly as a chemist. It is true that his chemical discoveries are among the greatest of his scientific achievements. But, as these volumes abundantly prove, to consider him simply as a chemist is to take a very partial and incomplete view of his scientific activities. In truth he was a Natural Philosopher of the broadest possible type, who occupied himself in turn with every branch of physical science known in his time, and who impressed the marks of his genius and the extraordinary penetrative force of his intellect on them all.

In its general plan the present volume is substantially similar to the volume of Electrical Researches edited by Professor Clerk Maxwell. It gives a brief account of Cavendish's personal history and characteristics, followed by a short commentary on his investigations, as published in part in the *Philosophical Transactions* of the Royal Society, where, as a matter of fact, everything that he chose to print is to be found. This commentary attempts to show the relation of his work to the knowledge of the time, to point out wherein it marks a new departure and how it permanently influenced the progress and development of science. Such advances as the recognition of the individuality of the gases, the discovery of the real nature of atmospheric air, and of the compound nature of water and its quantitative composition, were, it need hardly be stated, epoch-making. The twenty-two years that Cavendish devoted to chemical inquiry constitute indeed one of the most brilliant periods in the history of that science. In his work on thermometry, on cryoscopy, and on terrestrial magnetism he often breaks new ground and anticipates discoveries which have been attributed to much later observers.

This section is followed by a reprint of Cavendish's papers, other than those relating to electricity, which, as already stated, form the subject of Vol. I, with reproductions of the original illustrations. The papers are arranged in chronological order, and certain obvious errors and necessary typographical corrections have been indicated.

The last section deals with the unpublished manuscripts other than those already dealt with by Professor Clerk Maxwell, and is based upon the material in the possession of the Duke of Devonshire. Some small portion of these has already been made known. The Rev. William Vernon Harcourt examined the papers in connection with the famous Water Controversy, which he made the main subject of his Presidential Address to the British Association in 1839; he eloquently and convincingly advocated the just claims of Cavendish as the first and true discoverer of the compound nature of water, and printed in *facsimile* some excerpts from the manuscripts. Mr Harcourt appears to have gone through the whole of the papers and to have arranged them according to their subject matter. He was so impressed with their character and with the light they threw on the nature and range of Cavendish's intellectual activities that, in the course of his address, he strongly urged that they should be critically examined and arranged for publication. The present edition of Cavendish's works may be regarded as a compliance, however belated, with a desire that has often been urged.

The unpublished papers relating to chemistry show that Cavendish had anticipated Scheele in the discovery of arsenic acid, which he prepared by the method now in use. They also appear to indicate that if he did not actually anticipate Scheele in the isolation of tartaric acid, he was an independent discoverer of the true nature of "tartar," and of the relation of cream of tartar to "soluble tartar" or normal potassium tartrate. He seems to have been familiar with certain of the general principles underlying the phenomena of gaseous diffusion and to have experimentally verified them. He was perhaps the first to attempt to investigate quantitatively the phenomena of gaseous explosions. His work on Heat was original and independent; and had it been published at about the time it was performed, it would have placed him on a par with Black in regard to the historical development of the subject and have established his priority to Irvine, Crawford, Wilcke and Charles. His long and patient work on the tension of aqueous vapour was remarkably accurate and compares favourably even with that of Regnault. It was thus far superior to the measurements of Dalton, made nearly 30 years subsequently, and long regarded, despite the criticisms of Biot, as authoritative, especially in this country.

With respect to his other physical investigations it can hardly be matter for surprise to those who have studied the Electrical Researches as edited and expounded by Maxwell, that their author should have possessed views on these subjects far in advance of his age. The manuscripts reveal him as an indefatigable student of physical science of every kind, remarkable both for originality of ideas and for skill in their scrutiny and development by the rough direct methods of Newtonian fluxional calculation. There can be no doubt that Cavendish was the directing force in the project to determine the mean density of the Earth, whether from observations of the attraction of a mountain, or, twenty years later, by the vibrational method which he had inherited from his friend Michell. A preliminary discussion, which must date from some earlier time, of the results of the French geodetic expedition to Peru, had this object mainly in view: it dealt in a practical way with all the questions of geodetic compensation and isostasy which emerged later in more precise fashion in the great Indian Survey, and are now so prominent. The error in observations due to change of refraction on a heated slope was not overlooked, and was discussed (p. 393) in a masterly manner. The dynamical variation of latitude, now an important correction to refined observation, which had only been mentioned in passing by Euler, is fully grasped, and concisely expounded, though its actual amount was of course then unknown.

But perhaps most striking of all were his reasoned views on the Conservation of Energy, including a precise introduction of the idea of Potential Energy, and the recital of the causes of the degradation of Energy into the form of Heat. To obtain a parallel to the lines of his argument, cramped in expression as it naturally was at that time, we have to come down to Helmholtz's famous Essay of 1847. An effort has been made by Sir Joseph Larmor, in brief footnotes, to correlate his views with the pronouncements of Newton and Daniel Bernoulli, and other writers of the eighteenth century on mathematical physics, to whom Helmholtz has recorded his obligations in connection with the doctrine of Energy. Especially acute is Cavendish's expression of his conviction that the degradation of any likely amounts of kinetic and potential energies of moving particles, as then known, would prove insufficient to provide the large amounts of heat that are found to be involved in chemical changes; the next word on this subject belongs to Faraday and Maxwell and the Electric Molecular Theory.

He knew, as the Astronomer Royal points out, about the tidal retardation of the diurnal rotation of the Earth, and understood (p. 437) the principles of rotational torque and exchange of energy that are involved, apparently in a more direct and complete way than did Kant, recalling in fact the modern developments by Lord Kelvin and Sir George Darwin. The amount of deviation of a ray of light passing near the Sun, on the Newtonian corpuscular theory as extended by Michell to include gravitation of the corpuscles, did not escape (p. 437) his universal scrutiny of physical Nature. In most of his work the trend of thought seems to have been straight towards the course of the subsequent progress of science.

From the very outset of his career as a scientific observer, Cavendish seems to have occupied himself with the problems of terrestrial magnetism; and his interest in that subject continued unabated so long as he lived. Much of the material preserved at Chatsworth, and which is incidentally referred to in a postscript to Clerk Maxwell's Introduction to Vol. I, relates to this subject. Although it did not fall within the scope of the work on which he was engaged, Maxwell gave a brief summary of the character of the magnetic papers, and concluded that "they may supply important materials for the magnetic history of the Earth, and are in all respects excellent specimens of Cavendish's scientific procedure."

These papers have now been carefully examined by Dr Chree, the superintendent of the Kew Observatory, who has furnished a very complete account of their contents. He shows how Cavendish anticipated the ideas and procedure of subsequent observers in determining, for example, the best form of dip needles, in tracing the influence of "bending" of the needle on the observed value of its inclination, and as regards other sources of error.

Cavendish continued to make observations on magnetic dip and declination for nearly 40 years, and accumulated a considerable mass of material. This has been thoroughly sifted and discussed by Dr Chree; thus his summary of the results constitutes an important contribution to the magnetic history of the Earth, and its known close relation to the solar cycles, during the last third of the eighteenth century for which our information is so scanty and uncertain.

Acknowledgments are due to Sir Joseph Larmor for the active interest he has taken in the production of the work, and in particular for his contribution concerning the mathematical and dynamical papers and memoranda to be found among the Chatsworth manuscripts, and for certain general remarks on Cavendish's merits as an original thinker and observer which have been incorporated in the Preface. Thanks also are due to Sir Archibald Geikie, to the Astronomer Royal, and to Dr Chree for their

several communications on such of Cavendish's unpublished papers as come within their special scope and interests.

Sir Horace Darwin was good enough to afford a critical estimate of Cavendish's published paper on his manner of dividing Astronomical Instruments; and Dr Burnett, of the British Museum, gave a similar estimate of the value of Cavendish's paper in the *Philosophical Transactions* on the Civil Year of the Hindoos.

Finally it is a pleasure to acknowledge the very efficient assistance rendered in every way by the staff of the Cambridge University Press. It cannot be doubted that the enterprise of the University Press, in now at length giving to the world an adequate account of the researches of one of the greatest of the intellectual sons of Cambridge, will meet at the hands of all men of science with the appreciation it deserves.

T. E. THORPE.

July 1920.

PREFACE

THE University of Cambridge has a deep interest both in the Author and in the Editor of the electrical investigations now presented in final form in this volume.

Henry Cavendish matriculated in the University on 18 Dec. 1749, from Hackney School. According to records preserved at Peterhouse he commenced residence there on 24 Nov. 1749, and resided very regularly and constantly as a Fellow-Commoner until 23 Feb. 1753, when he left without proceeding to a degree. Two others of the Cavendish family were at Peterhouse at the same time; Henry's younger brother Frederick who was entered ro April, 1751, and also left without taking his degree; and his cousin Lord John Cavendish, fourth son of the Duke of Devonshire, afterwards Chancellor of the Exchequer, who was entered 21 Feb. 1750 and became M.A. in 1753. Among his contemporaries at Peterhouse, then as now a small society, were the Earl of Euston, afterwards as Duke of Grafton prominent in the writings of Junius, Gray the poet and also Mason*, and the Greek critic Markland, who was Senior Fellow of the College at the time and always in residence.

These details arc taken from a statement contributed in 1851 to Dr G. Wilson's *Life of Cavendish* by Prof. F. Fuller, then Fellow and Tutor of Peterhouse.

James Clerk Maxwell graduated at Cambridge in Jan. 1854. After two years' resident activity at Cambridge, including election to a Fellowship at Trinity College, he was appointed in April, 1856 to the Chair of Natural Philosophy in Marischal College, Aberdeen. There he worked until 1860, when on the fusion of the two Aberdeen Universities he became Professor of Natural Philosophy in King's College, London. He resigned that Chair at Easter, 1865, left London the following year and settled down at his inherited home at Glenlair near Dalbeattie in Galloway. To quote his own words of Feb. 1866 *(Life,* 1882, by L. Campbell and W. Garnett, p. 344): "I have now my time fully occupied with experiments and speculations of a physical kind, which I could not undertake so long as I had public duties." The result has contributed, more than any other cause, to the modem revolution in the ideas and methods of physical science.

In October, 1870 William Cavendish, Duke of Devonshire, who had graduated as second wrangler and first Smith's Prizeman in Mathematics and a first class in Classics in 1829, and was elected in 1861 Chancellor of

* Mason was tutor to Lord John Cavendish, but was of Pembroke College, having previously been of St John's.

t Cf. also ***The Cavendish Family*** by F. Bickley (r9n), pp. 197-208.

the University, signified his desire to build and furnish a Physical Laboratory for Cambridge: and in prospect of that gift the Cavendish Professorship of Experimental Physics was founded by Grace of the Senate in Feb. 1871. In response to appeals from the prominent Cambridge men of the time, including Stokes, W. Thomson, and Rayleigh, Clerk Maxwell was persuaded to offer himself for the Chair. He was formally elected the following month, after six years of retired study and investigation which doubtless had matured and consolidated the intellectual interests of his life, including the preparation of the *Electricity and Magnetism* (Feb. 1873) and the *Theory of Heat* (1870). The Laboratory was planned and furnished under Maxwell's direction, and formally handed over to the University in the spring of 1874. It was not however until 1877 that the Chancellor had completed his gift "by furnishing the Cavendish Laboratory with apparatus suitable to the present state of science"; an equipment to which Maxwell afterwards contributed many additions. Later, Lord Rayleigh, when Chancellor of the University, devoted the proceeds of the award of a Nobel Prize to provide for an urgent expansion of the Laboratory of which he had himself been Director.

The *Electricity and Magnetism* shows many marks of hurried final consolidation with a view to immediate publication. It was said that the pressing need of a textbook in the University was a paramount consideration: there was no treatise of comparable depth and grasp at that time in any language. And certainly under Maxwell's influence Cambridge was the focus in which the new electrical ideas, inherited in outline from Faraday, were developed and propagated, years before they were taken up in other countries and thus became everywhere the mainspring of progress in physical science. Maxwell's own personal investigations during the Cambridge period, in addition to a series of brilliant articles, now classical, written for the *Encyclopaedia Britannica*, were concerned mainly with the development of the other equally fundamental, but analytically much more complex, subjects centring round the molecular theory of gases; in this domain also he had previously (1860) been the originator of the modern exact analysis, based on application of the mathematical principles of statistics to the fortuitous dance of the innumerable molecules.

According to Maxwell's biographers his chief continuous literary occupation, for the five years from 1874 to his death in October, 1879, was the editing of the *Electrical Researches of Henry Cavendish.* It had been well known that Cavendish's papers, preserved in the possession of his kinsman the Duke of Devonshire, contained a very remarkable and even mysterious record of progress in electrical as well as chemical science, effected a hundred years previously by a solitary investigator, of which only fragments had been revealed by various men of science who had seen the manuscripts. The publication of an adequate account of the researches of Cavendish was a task obviously incumbent on British science, for its own sake; but the difficulty and labour of the undertaking, and the learning and historical research that it involved, had hitherto warned off the men most competent to discharge it. The zeal of Maxwell for his new Cavendish foundation was not thus to be deterred. Already in July, 1874, we find him writing from Glenlair to Mr Garnett (*Life*, p. 389):

In the MS. he appears to be familiar with the theory of divided currents and also of conductors in series, but some reference to his printed paper [on the Torpedo] is required to throw light on what he says. He made a most extensive series of experiments on the conductivity of saline solutions in tubes, compared with wires of different metals, and it seems as if more marks were wanted for him if he cut out G. S. Ohm long before constant currents were invented. His measures of capacity will give us some work at the Cavendish Laboratory, before we work up to the point where he left it. His only defect is not having Thomson's electrometer. He found out inductive capacity of glass, resin, wax, etc.

According to Mr Garnett (*Life*, p. 555) who was in a position to be intimately acquainted with the facts:

The amount of labour which Professor Maxwell bestowed on this work during the last five years of his life can only be known to those who were constantly in his company. Nearly all the MSS. he transcribed with his own hand, the greater part being copied after midnight....Every obscure passage or alteration was the subject of a long and searching investigation: and many were the letters written to the Librarian of the Royal Society and to scientific and literary friends in different parts of the country, to gain information respecting the meaning of obsolete words and symbols, or the history of individuals. And besides this, and a comparison of Cavendish's results with those obtained by subsequent investigators, Maxwell repeated many of Cavendish's experiments almost in their original form, only employing modern instruments for the purposes of measurement.

The result of five years of continual application to the subject was the volume published in October, 1879 by the Cambridge University Press, a few weeks before the death of its Editor, and now reprinted in different form. The introductory sketch prepared by Maxwell, probably at the end of his task, gives a clear and most interesting summary of the electrical work of Cavendish: the postscript dated 14 June, 1879, describing some manuscripts on magnetism that had just come to hand, coincides with the beginning of his final illness.

There is perhaps no instance in the history of science in which the unpublished records left by an investigator have been arranged and elucidated with such minute fidelity. Careless though Cavendish was of scientific reputation, intent on pressing on to new solitary achievement, to the neglect of publication, due as it would seem as much to the habit of continual postponement of final preparations for the press as to the fascination of exercising his powers of discovery—and even, as it has proved, as a consequence of his recluse and self-centred life—there are perhaps few investigators of the first rank of whose work and aims and procedure we have now more complete knowledge than of his.

The additions appended by Maxwell, in the form of thirty-five notes of elucidation and commentary, on modern lines, relating to Cavendish's results and methods, constitute an example of powerful and elegant relevant original investigation such as could hardly have been carried through by anyone else.

Advantage has been taken of the present reprint of the Electrical Researches, as constituting Volume I of a definitive edition of the *Scientific Writings* of Henry Cavendish, to add a few brief annotations and references such as were needed to bring Clerk Maxwell's commentary up to date. These notes, where appended to Cavendish's text, are enclosed in curved brackets to distinguish them from Maxwell's own. As examples, reference may be made to pp. 374, 413, 422. The printing of the original edition had probably proceeded at intervals, and the final consolidation must have gone on during Prof. Maxwell's last illness in the summer of 1879. Thus it has now been possible to improve the headings of the chapters and sections, and the headlines of the pages, so as to convey a clearer and more rapid view of the nature and content of the text. The index and table of contents have been improved.

Apart from his permanent contributions to experimental laws, it is possible to maintain that the theoretical views of Cavendish should now command on historical grounds even more interest than they could excite in 1878, when the Electrical Researches were made public in complete form by Clerk Maxwell. At that time attention was largely concentrated on the elucidation of the electric field, and the mode of transmission of electrical influence from one body to another. The formal settlement of that range of problems on the lines of the Faraday-Maxwell theory has now transferred investigation to the sources of electric influence; and problems of the distribution of electrons in conducting and insulating bodies, their relation to the electrically polarisable molecules of matter, their function in conduction and in radiation, even the exploration of crystals in atomic detail by radiations of molecular wave-length, are now opening out. These problems all involve interaction in a binary medium, electrons and molecules controlling activities in an aether; it is now an affair of relations of the field of transmission with electrically polar or polarisable molecules which are its sources; and though this is very different from Cavendish's idea of a uniform electric fluid pervading and interacting with material substances by mere attraction, yet the degree of success that had been attained by the earlier and simpler mode of representation can become again by contrast a subject of historical scientific interest. The title of one of Lord Kelvin's best-known memoirs, "Æpinus atomized," is evidence for this view.

Numerous biographies of Cavendish have been published. He was one of the select circle of foreign associates of the Institute of France; and French interest in his work, stimulated by its close relations with that of Lavoisier, was reflected in memoirs by Cuvier in the *Éloges Historiques de l'Académie*, vol. I and by Biot in the *Biographie Universelle*, vol. VII. These and other biographies are drawn upon by Dr George Wilson in the very thorough *Life of Cavendish* (pp. 478), including an analysis of his chemical work, which was undertaken under the auspices of the Cavendish Society, founded in 1846, and appeared in 1851 as one of the early volumes of their publications.

The biographical sketch contributed by Dr Thomas Young to the supplement of the *Encyclopaedia Britannica* about 1820 has been reprinted here as an Appendix. Young must have been personally well acquainted with Cavendish, and no one was better qualified to form a contemporary judgment on his career.

The portrait prefixed to the present volume is said to have been constructed from surreptitious sketches made by the artist W. Alexander at a dinner of the Royal Society Club. The original is in the print room of the British Museum, where it was re-discovered by Charles Tomlinson, F.R.S. who had an engraving made from it. This engraving has been reproduced as a frontispiece to Wilson's *Life*, and many times since. The present photographic impression has been taken from the original picture, by permission of the authorities of the British Museum.

It has been the custom, even among Cavendish's admirers, to brand him as misanthropic. But there is surely another side to this judgment. The cultivation of the highest domains of physical science is rarely consistent with dispersal of interest in other directions. The tracking out of great discoveries which will be a possession to the human race for all time has indeed to be its own supreme intellectual satisfaction; and once an investigator has realized, in however modest a way, his capacity for such achievement, he can feel that he is serving humanity in the most perfect manner open to him by concentrating upon that work. Yet the temptation to continual postponement of ordinary social intercourse inevitably involves increasing isolation, and growing habits of solitude. As already noted, there is no evidence that Cavendish's researches aimed at his personal gratification alone: if they had not been adequately recorded by him they could not have been recovered so completely: and it is easy to understand how the driving force of his curiosity and conscious power would impel him to the exploration of new fields, in temporary preference to the final polishing of work already achieved. If he spent his life in compelling the phenomena of physical nature to submit to exact measure and weight, it was not from a special passion for such work, for its own sake, but as the one means of assuring an adequate foundation for sciences then being born: in all directions he was opening up and securing brilliant vistas into the philosophical explication of nature. Though standing so far beyond most of his contemporaries in intellect and vision, there is abundant evidence in these volumes that in the cooperative tasks which united the scientific men of the time, largely conducted under the auspices of the Royal Society, he was always ready to take unsparing pains, and to devote himself without limit to the assistance of his colleagues. The operations and discussions preparatory to the gravity survey of Schehallion, summarised in Vol. II, are an example.

The two volumes now published may be regarded as the final garnering of the work of one of the greatest of scientific discoverers. The acknowledgments of the intellectual world will doubtless be accorded to the Cambridge University Press for their courage in facing the great expense involved in a complete edition of the writings of Cavendish, in a form adequate to the subject, which was projected in the less exacting times before the Great War. The Editors desire to record their thanks to the staff of the Press for very efficient cooperation on the technical side of the undertaking.

J. L.

CAMBRIDGE,
February 1921.

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* The two flat conductors between which the plate of air lies, or, in modern language, the electrodes.

Here ends Cavendish's Index

* Sea salt.

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[JAMES CLERK MAXWELL, 1879]

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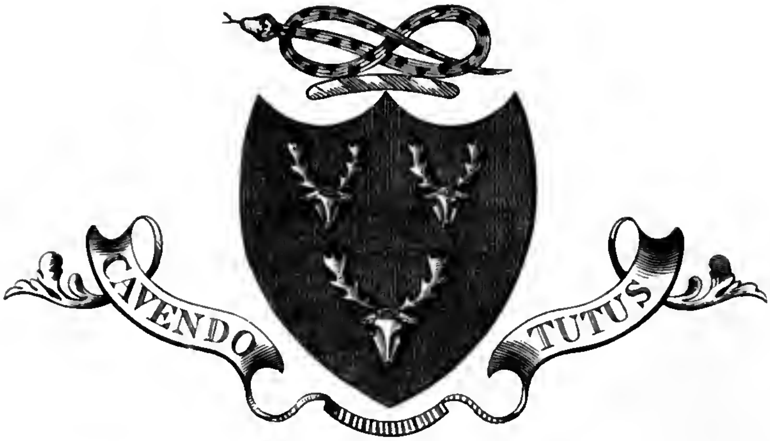
In the late Dr George Wilson's collection of Cavendish MSS. there is a drawing of which the following page is a reduced copy. The words "buried at Derby" are written in pencil on the margin.

Henry Cavendish was buried in the Devonshire Vault, All Saints' Church, Derby, but Mr J. Cooling, Jun., Churchwarden of All Saints, informs me that there is no slab or monument of any kind erected in memory of him there. [See introduction to Vol. II of this edition.]



HENRY CAVENDISH ESQ^{R}

Eldest Son of the Right Honorable

LORD CHARLES CAVENDISH,

Third Son of William,

2ND *DUKE* OF *DEVONSHIRE.*

Fellow of the Royal Society, and of the Society

OF ANTIQUARIES OF LONDON.

Trustee of the British Museum

and Foreign Associate in the First Class

of the INSTITUTE at Paris.

BORN October 10th 1731 | DIED February 24th 1810

INTRODUCTION*

So little is known of the details of the life of Henry Cavendish, and so fully have the few known facts been given in the *Life of Cavendish* by Dr George Wilson†, that it is unnecessary here to repeat them except in so far as they bear on the history of his electrical researches.

He was born at Nice on the 10th October, 1731; he became a Fellow of the Royal Society in 1760, and was an active member of that body during the rest of his life. He died at Clapham on the 24th February, 1810.

His father was Lord Charles Cavendish, third son of William, second Duke of Devonshire, who married Lady Anne Grey, fourth daughter of the Duke of Kent. Henry was their eldest son. He had one brother, Frederick, who died 23rd February, 1812.

Of Lord Charles Cavendish we have the following notice by Dr Franklin‡. After describing an experiment of his on the passage of electricity through glass when heated to 400° F., he says, "It were to be wished that this noble philosopher would communicate more of his experiments to the world, as he makes many, and with great accuracy."

Lord Charles Cavendish has also recorded a very accurate series of observations§ on the depression of mercury in glass tubes, and these have furnished the basis not only for the correction of the reading of barometers, &c., but for the verification of the theory of capillary action by Young, Laplace, Poisson and Ivory.

I think it right to notice the scientific work of Lord Charles Cavendish, because Henry seems to have been living with him during the whole period of his electrical researches. Some of the jottings of his electrical calculations are on torn backs of letters, one of which is addressed,

[The Ho]n^{ble} M^{r} Cavendish
at the R^{t} Hon^{ble}
The L^{d} Charles
Cavendish's
Marlborough Street.

* By the Editor [James Clerk Maxwell].

† Published in 1851 as the first volume of the Works of the Cavendish Society.

‡ *Franklin's Works*, edited by Jared Sparks, Boston, 1856, vol. v. p. 383. See also Note 26 at the end of this book.

§ *Phil. Trans.* 1776, p. 382.

These calculations relate to the equivalent values of his trial plates when drawn out to different numbers of divisions. There is no date nor any part of the original letter.

The memoranda of some experiments similar to those in Art. 588, on the time of discharge of electricity through different bodies, are on the back of the usual Notice of the election of the Council and Officers of the Royal Society on the Thirtieth of November, 1774 (being St Andrew's Day) at Ten o'Clock in the Forenoon at the House of the Royal Society in Crane Court, Fleet Street. The address on the back of this letter is

To

The Hon Henry Cavendish

Gr[t] Marlborough Street.

Dr Thomas Thomson, who was acquainted with Cavendish, says in his interesting sketch of him*,

> During his father's life-time he was kept in rather narrow circumstances, being allowed an annuity of £500 only, while his apartments were a set of stables, fitted up for his accommodation. It was during this period that he acquired those habits of economy and those singular oddities of character which he exhibited ever after in so striking a manner.

The whole of the electric researches of which we are to give an account were made before the death of Lord Charles Cavendish, which took place in 1783. We must therefore suppose that they were made in Great Marlborough Street, and probably in the set of stables mentioned by Dr Thomson. He speaks of a "fore room and a back room" in Art. 469, and in Art. 335 he compares the size of the room in which he worked to that of a sphere 16 feet in diameter. The dimensions of his laboratory are of some importance in determining the electric capacity of bodies hung up in it, and by the foot-note to Art. 335 it would appear that the room was probably 14 feet high, which is somewhat lofty for "a set of stables," but I believe not much more than the height of some of the rooms in the dwelling-houses in Great Marlborough Street.

Let us then suppose that we have been admitted by Cavendish into his laboratory in Great Marlborough Street, as it was arranged for his electrical experiments in 1773, and let us make the best of an opportunity rarely, if ever, accorded to any scientific man of his own time, and examine the apparatus by which the electric fluid, instead of startling us with the brilliant phenomena, new instances of which were then every day being discovered, was made to submit itself, like everything else which entered that house, to be measured.

The largest piece of apparatus was the "machine for trying simple bodies" of which we have a description and sketch in Art. 241, and plans

* *History of Chemistry*, vol. 1. p. 336, quoted in Wilson's *Life of Cavendish*, p. 159.

at Arts. 265 and 273. The framework of the machine is not represented in these figures.

We learn, however, from Dr Davy*, that

Cavendish seemed to have in view, in construction, efficiency merely, without attention to appearance. Hard woods were never used, excepting when required. Fir-wood (common deal) was that commonly employed.

The bodies to be "tried" and the wires and vials for trying them were either supported on glass rods as shown in the sketch at Art. 239, or else hung by silk strings from a horizontal bar 7 feet $3\frac{1}{2}$ inches from the floor as mentioned in Art. 466. The electrical connexions were made and broken at the proper times by means of silk strings passing over pullies attached to the horizontal bar.

One of the bodies, the charges of which Cavendish compared by means of this apparatus, was a globe 12·1 inches in diameter covered with tinfoil. This globe has historical interest as it was not only the standard of capacity with which Cavendish compared that of all other bodies, but it formed part of the apparatus by which he established that the electric repulsion varies inversely as the square of the distance.

There was also a set of circles of tin plate, one of 36 inches diameter, one of 18·5 and two of 9·3; and also square and oblong tin plates, and squared pieces of stone and slate, and a collection of cylinders and wires of different sizes.

There was another "machine," represented, with its framework, in Fig. 20, Art. 295, "for trying Leyden vials."

The "Leyden vials" were most of them flat plates of glass with circular coatings of tinfoil, one on each side. They were made in sets of three, any one of each set being nearly equal in capacity to the three of the former set taken together. Cavendish had thus a complete set of condensers of known capacity by means of which he measured the capacity of every piece of his apparatus, from the little wire which he used to connect his coated plates, and which he found to contain ·28 "inches of electricity," up to his battery of 49 jars, which contained 321,000 "inches of electricity †."

These "inches of electricity" can be directly compared with our modern measurements of electrostatic capacity. Indeed the only difference is that Cavendish's "inches of electricity" express the diameter of the sphere of equivalent capacity, while the modern measurements express the capacity by stating the radius of the same sphere in centimetres.

Of each of these plates of glass Cavendish has given a most minute description, so that each, if it were found, could be identified. Mr Cottrell, of the Royal Institution, has been kind enough to examine the catalogue of apparatus there, which contains Cavendish's Eudiometer and Registering

* Wilson's *Life*, p. 178.

† About half a microfarad.

Thermometer. No trace, however, of a set of glass plates could be found. It is possible, however, that if the plates were neatly packed up, their small bulk and their apparent uselessness may have enabled them to survive the periodical overhaulings of some less celebrated repository, and that they may yet gain an honourable place in the museum of historical instruments.

But we need not expect ever to discover a piece of apparatus of still greater historical interest—that by which Cavendish proved that the law of electric repulsion could not differ from that of the inverse square by more than $\frac{1}{50}$. It consisted of a pair of somewhat rickety wooden frames, to which two hemispheres of pasteboard were fastened by means of sticks of glass. By pulling a string these frames were made to open like a book, showing within the hemispheres the memorable globe of 12·1 inches diameter, supported on a glass stick as an axis. By pulling the string still more, the hemispheres were drawn quite away from the globe, and a pith ball electrometer was drawn up to the globe to test its "degree of electrification." A machine so bulky, so brittle, and so inelegant was not likely to last long, even in a lumber room. A facsimile of Cavendish's sketch of it is given at page 118. His own account of the experiment, in Arts. 217–234, is one of the most perfect examples of scientific exposition.

We might also notice the different electrometers, most of them consisting of a pair of cork or pith balls, mounted on straws or on linen threads, and some of them capable of having their weight altered by means of wires run into the straws; but though Cavendish had a wonderful power of making correct observations and getting accurate results with these somewhat clumsy instruments, we must confess that in these, the most vital organs of electric research, Cavendish showed less inventive genius than some of his contemporaries. When Lane and Henly brought out their respective electrometers, Cavendish compared their indications, and by stating in every case the distance at which Lane's electrometer discharged, he has enabled us to calculate in modern units every degree of electrification that he made use of. What was really needed for Cavendish's experiments was a sensitive electrometer. Cavendish did the best with the electrometers he found in existence, but he did not invent a better one.

It was not till 1785 that Coulomb began to publish the wonderful series of experiments, in which he got such good results with the torsion electrometer, an instrument constructed on the same principle as that with which Cavendish afterwards measured the attraction of gravitation; and it was not till 1787 that Bennett described in the *Philosophical Transactions* the gold leaf electrometer, by means of which Volta afterwards demonstrated the different electrification of the different metals.

The electrical machine, by Nairne, was one with a glass globe.

We should also notice the dividing engine, by Bird, for determining the thickness of the glass plates, and other small distances.

An attendant*, "Richard," appears occasionally, to help in turning the electrical machine, or in pulling the strings which made or broke the electrical connexions; and sometimes he is even asked his opinion as to the comparative strength of two electric shocks†. But there is no record of any other person having been admitted into the laboratory during the series of experiments to which we now refer.

The authority of Cavendish in electrical science was of course established by his paper of 1771, and accordingly we find him nominated by the Royal Society as one of a committee appointed in 1772 "to consider of a method for securing the powder magazine at Purfleet‡."

A powder mill at Brescia having blown up in consequence of being struck by lightning, the Board of Ordnance applied to Mr Benjamin Wilson, F.R.S., who held the contract for the house-painting under the Board§, and who had some reputation as an electrician, for a method to prevent a like accident to their magazines at Purfleet. Mr Wilson having advised a blunt conductor, and it being understood that Dr Franklin's opinion, formed upon the spot, was for a pointed one, the matter was referred, in 1772, to the Royal Society, and by them as usual to a committee, who after consultation presented a method conformable to Dr Franklin's theory‖.

The Committee consisted of Cavendish, Dr, afterwards Sir William Watson, Dr Franklin, Mr J. Robertson (Clerk and Librarian to the Royal Society), Mr Wilson and Mr Delaval.

Dr Franklin read to the Committee a paper which is printed in his works, vol. v. p. 435, but is not referred to in the report of the Committee, though the report is entirely in conformity with it¶.

The Committee went down to Purfleet and examined all the buildings together, but I cannot trace any evidence that Cavendish did anything to modify the report, and Franklin does not mention him in any part of his writings, as one of the remarkable men with whom he was brought in contact.

The most noteworthy incident of the Committee was the dissent** of Mr Wilson, to which Mr Delaval adhered as regards that part of the report which recommended the conductors to be pointed. Mr Wilson followed up his dissent by a paper††, in which he gave his reasons for preferring

* Arts. 242, 560, 565. † Art. 511.

‡ See *Franklin's Works*, vol. v. p. 430, note.

§ He also painted portraits of Franklin and of Gowin Knight, as well as of Garrick in various characters.

‖ *Phil. Trans.* 1772, p. 42.

¶ The report is printed in *Franklin's Works*, vol. v. p. 430, and is there stated to be "Drawn up by Benjamin Franklin, August 21, 1772." The paper on the utility of long, pointed rods is stated to have been read on August 27th, 1772. [On the general subject of atmospheric electric discharges see C. T. R. Wilson, *Phil. Trans.* 1920.]

** *Phil. Trans.* 1772, p. 48. †† *Ib.* p. 49.

blunt conductors; but the other four members of the Committee, Messrs Cavendish, Franklin, Watson, and Robertson, having heard and considered these objections, found no reason to change their opinion or vary from their Report*.

But on the 15th May 1777, the Board House at Purfleet was struck by lightning, and some of the brickwork damaged. This being communicated by the Board of Ordnance to the Royal Society†, a Committee was appointed to examine the effects of the lightning and to report.

The Committee consisted of Mr Henly, Mr Lane, Mr Nairne and Mr Planta, Secretary of the Royal Society. They reported in favour of making a channel all round the parapet and filling it with lead, and connecting this in four places with the main conductor on the roof of the building.

Mr Wilson, however, dissented from this Report, and communicated to the Royal Society an account of a most elaborate and indeed magnificent set of experiments conducted in the Pantheon, in which a cylinder 155 feet long, composed of 120 drums, and connected with a wire 4800 feet long, suspended on silk strings, was electrified, and the discharge made to strike a model of the Board House at Purfleet. The experiments were witnessed by King George III, and seem to have been very brilliant. The picture of the experiment, probably drawn by Mr Wilson, is, as a work of art, considerably above the average of the plates in the *Philosophical Transactions*.

The subject was referred to a larger Committee, consisting of Sir John Pringle, President of the Royal Society, Dr Watson, Henry Cavendish, W. Henly, Bishop Horsley, T. Lane, Lord Mahon, Edw. Nairne, and Dr Priestley.

They reported‡ in favour of having an additional number of conductors ten feet high, terminated with pieces of copper eighteen inches long, and as finely tapered and acutely pointed as possible.

We give these directions (they conclude), being persuaded, that elevated rods are preferable to low conductors terminated in rounded ends, knobs, or balls of metal; and conceiving, that the experiments and reasons made and alledged to the contrary by Mr Wilson, are inconclusive.

I have stated this incident at some length because it does not appear to have been noticed by Cavendish's biographers, and because it shows him cooperating with Franklin and others in an electrical investigation undertaken in the interest of the nation.

Cavendish's researches on the electric current have been hitherto very imperfectly known, as they are only alluded to in his celebrated paper on the Torpedo. The private investigations of Cavendish are contained in this volume, but the external events which were more or less connected with them, were as follows:

* *Phil. Trans.* 1772, p. 66. † *Ib.* 1778, p. 232. ‡ *Ib.* p. 313.

On July 1, 1773, Mr Walsh communicated to the Royal Society his paper "Of the Electric Property of the Torpedo. In a Letter from John Walsh, Esq., F.R.S., to Benjamin Franklin, Esq., LL.D., F.R.S., Ac. R. Par. Soc. Ext., &c."

The following extracts will indicate the chief points of electrical interest.

The vigour of the fresh taken Torpedos at the Isle of Ré was not able to force the torpedinal fluid across the minutest tract of air; not from one link of a small chain, suspended freely, to another; not through an almost invisible separation, made by the edge of a pen-knife in a slip of tinfoil pasted on sealing-wax.

The effect produced by the Torpedo when in air appeared, on many repeated experiments to be about four times as strong as when in water.

The Torpedo, on this occasion, dispensed only the distinct instantaneous stroke, so well known by the name of the electric shock. That protracted but lighter sensation, that Torpor or Numbness which he at times induces, and from which he takes his name, was not then experienced from the animal; but it was imitated with artificial electricity, and shewn to be producible by a quick succession of minute shocks. This in the Torpedo may perhaps be effected by the successive discharge of his numerous cylinders, in the nature of a running fire of musketry; the strong single shock may be his general volley. In the continued effect, as well as in the instantaneous, his eyes, usually prominent, are withdrawn into their sockets.

Walsh shows that these phenomena "are in no ways repugnant to the laws of electricity," for "the same quantity of electric matter, according as it is used in a dense or rare state, will produce the different consequences."

Let me here remark that the sagacity of Mr Cavendish in devising and his address in executing electrical experiments, led him the first to experience with artificial electricity, that a shock could be received from a charge which was unable to force a passage through the least space of air.

Walsh concludes his letter to Franklin in the following terms:

I rejoice in addressing these communications to You. He, who predicted and shewed that electricity wings the formidable bolt of the atmosphere, will hear with attention, that in the deep it speeds an humbler bolt, silent and invisible: He, who analysed the electrified Phial, will hear with pleasure that its laws prevail in animate Phials: He, who by Reason became an electrician, will hear with reverence of an instinctive electrician, gifted in his birth with a wonderful apparatus, and with the skill to use it*.

* That the electrical fishes still possess the power of exciting the imagination as well as the nerves of those who have felt their power may be seen from the following passage with which Prof. Du Bois Reymond begins his account of experiments on a living Malapterurus in the *Monatsberichte d. k. Acad. Berlin*, 28 Jan. 1858.

"Fast möchte man es, im Sinne Newton's, eine Anwandlung der Natur nennen, dass es ihr gefallen hat, aus der Unzahl der Geschöpfe drei Fische, und zwar der

However I may respect your talents as an electrician, it is certainly for knowledge of more general import that I am impressed with that high esteem, with which I remain,

Dear Sir,

Your affectionate

And obedient servant,

JOHN WALSH.

This paper is followed in the *Philosophical Transactions* by "Anatomical Observations on the Torpedo," by John Hunter, F.R.S., in which the great anatomist describes the structure of the electric organs, in specimens of the fish furnished by Mr Walsh.

Considerable interest seems to have been excited by this account of the Torpedo, and several papers on the Torpedo and the Gymnotus are in the *Philosophical Transactions* for 1775, none of them, however, so valuable as the original one by Walsh.

The practical electricians, however, were by no means satisfied that the effects of these fishes were really produced by electricity.

Mr Ronayne has made a curious remark upon the supposed electricity of the torpedo: he says, "if *that* could be proved, he does not see why we might not have storms of thunder and lightning in the depths of the ocean. Indeed, I must say, that when a Gentleman can so far give up his reason as to believe the possibility of an accumulation of electricity *among conductors* sufficient to produce the effects ascribed to the Torpedo, he need not hesitate a moment to embrace *as truths* the grossest contradictions that can be laid before him*."

I am aware of only two occasions on which Cavendish, after he had settled his own opinion on any subject, thought it worth his while to set other people right who differed from him. One of these occasions was in

verschiedensten Art, nach Willkür herauszugreifen, um sie mit elektromotorischen Vorrichtungen von furchtbarer Gewalt als eine Waffe auszustatten, neben welcher der Giftzahn der Klapperschlange, ja die nordamericanische Drehpistole, als eine plumpe und armselige Erfindung erscheint; eine Waffe die, ohne ihren Träger der Gefahr blosszustellen, lautlos und mit Blitzesschnelle in die Entfernung reicht, und minutenlang eine secundendicht gedrängte Reihe von Geschossen schleudert, deren keines fehlen kann, weil alle auf allen Punkten des Raumes gleichzeitig vorhanden sind."

In the *Journal of Anatomy and Physiology* for April, 1879, is a *Note on a Curious Habit of the Malapterurus Electricus*, by A. B. Stirling. The author attempted to feed Joe (the Malapterurus) with fresh worms, but he would not look at them. Another fish, however, called Dick (Clarias), swallowed them. When Joe considered that Dick had enjoyed his breakfast long enough, he swam up to him and gave him such a shock that the whole was disgorged, whereupon Joe swallowed it himself. When Dick at last succumbed to this treatment, Joe could no longer get his food prepared for him, and gave up eating altogether.

* Extract from MS. letter of W. Henly, dated 21 May, 1775, in the Canton Papers in the Royal Society's Library. (Communicated to the editor by H. B. Wheatley, Esq.)

1778, when his experiments on the formation of nitric acid by the electric spark from phlogisticated and dephlogisticated air (nitrogen and oxygen) had been repeated without success by Van Marum with the great Teylerian electrical machine, and by Lavoisier and Monge, and when Cavendish "thought it right to take some measures to authenticate the truth of it." For this purpose he requested Mr Gilpin, clerk to the Royal Society, to repeat the experiment, and desired some of the gentlemen most conversant with these subjects to be present at putting the materials together, and at the examination of the produce*.

The other occasion, with which alone we are now concerned, is the only one in which the presence of visitors to Cavendish's laboratory is recorded. There can be no doubt that Cavendish had completely satisfied not only Mr Walsh, but what was more to the purpose, himself, that the electric phenomena of the torpedo are such as might arise from the discharge of a large quantity of electricity at a very feeble degree of electrification. It must therefore have been to satisfy other persons on this point that he took the trouble to construct an artificial torpedo of wood covered with leather, a rude model of the figure given by Walsh, with electric organs of pewter supplied with electricity from a battery of Leyden jars, by wires protected by glass tubes.

The accessories of this machine were equally unlike the kind of apparatus which Cavendish made when working for himself. The torpedo had a trough of salt water, the saltness of which was carefully adjusted, so as to be equal to that of the sea. It had also a basket to lie in, and a bed of sand to be buried in, and there were pieces of sole-leather, well soaked in salt water, which Cavendish placed between the torpedo and his hands, so that he might form some idea of what would happen if a traveller with wet shoes were to tread on a live torpedo half buried in wet sand.

It was on Saturday, 27th May, 1775, that Cavendish tried the effect of his Torpedo on a select company of men of science. We find in the Journal (Art. 601), the names of John Hunter, the great anatomist, Dr Joseph Priestley, chemist, electrician and expounder of human knowledge in general, Mr Thomas Ronayne, from Cork, the disbeliever in the electrical character of the torpedo, Mr Timothy Lane, apothecary and electrician, and Mr Edward Nairne, the eminent maker of philosophical instruments.

They got shocks from the torpedo to their complete satisfaction, and probably learnt a good deal about electricity, but it was neither to satisfy them nor to communicate to them his electrical discoveries, that Cavendish admitted them into his laboratory on this memorable occasion, but simply to obtain the testimony of these eminent men to the fact, that the shocks of the artificial torpedo agreed in a sufficient manner with Walsh's de-

* *Phil. Trans.* 1788.

scription of the effects of the live fish, to warrant the hypothesis that the shock of the real torpedo may also be an electrical phenomenon.

I have now related all that I have been able to ascertain of the external history of Cavendish, in so far as it bears on his electrical researches. We must in the next place consider the record of these researches—the two papers in the *Philosophical Transactions*, which are here reprinted, and the manuscripts now first published.

CAVENDISH'S WRITINGS ON ELECTRICITY

In the *Philosophical Transactions* for 1771 there is a paper entitled "An attempt to explain some of the principal Phænomena of Electricity by Means of an Elastic Fluid: By the Honourable Henry Cavendish, F.R.S." [Read Dec. 19, 1771, and Jan. 9, 1772, pp. 584–677.] This paper and that on the Torpedo (*Phil. Trans.* 1776) are the only publications of Cavendish relating to electricity.

Dr George Wilson, however, in his *Life of Cavendish** says,

Besides his two published papers on electricity, Cavendish has left behind him some twenty packets of manuscript essays, more or less complete, on Mathematical and Experimental Electricity. These papers are at present in the hands of Sir W. Snow Harris, who most kindly sent me an abstract of them, with a commentary of great value on their contents. It will I trust be made public.

Sir W. states that Cavendish had really anticipated all those great facts in common electricity which were subsequently made known to the scientific world through the investigations and writings of the celebrated Coulomb and other philosophers, and had also obtained the more immediate results of experiments of a refined kind instituted in our own day.

Sir William Thomson, to whom Sir William Snow Harris showed some of Cavendish's results, thus speaks of them in a note dated Plymouth, Monday, July 2, 1849.

Sir William Snow Harris has been showing me Cavendish's unpublished MSS., put in his hands by Lord Burlington, and his work upon them; a most valuable mine of results. I find already that the capacity of a disc (circular) was determined experimentally by Cavendish as $\frac{1}{1\cdot 57}$ of that of a sphere of same radius. Now we have capacity of disc $= \frac{2}{\pi} a = \frac{a}{1\cdot 571}$!

It is much to be desired that those manuscripts of Cavendish should be published complete; or, at all events, that their safe keeping and accessibility should be secured to the world†.

* *Works of the Cavendish Society*, vol. 1. *Life of Cavendish*, by George Wilson, M.D., F.R.S.E., London, 1851, p. 469.

† Reprint of *Papers on Electrostatics and Magnetism*, § 235, foot-note.

The Cavendish Society, for whom Dr Wilson prepared his *Life of Cavendish*, with an account of his chemical researches, did not consider that it came within their design to publish his electrical researches.

Sir W. Harris, in whose hands the manuscripts were placed by the Earl of Burlington, died in 1867. He makes several references to them in his work on Frictional Electricity, edited after his death by Charles Tomlinson, F.R.S., and published in 1867*, but he did not live to edit the manuscripts themselves. Under these circumstances it was thought desirable by Sir W. Thomson, Mr Tomlinson, and other men of science, that something should be done to render the researches of Cavendish accessible.

They accordingly represented the state of the case to the Duke of Devonshire, to whom the manuscripts belong, and in 1874 he placed them in my hands.

I could find no trace of Sir W. Harris' commentary referred to by Dr Wilson, except that Dr Wilson mentions having returned it to Sir W. Harris.

On the inside of the lid of the box which contained the manuscripts was pasted a paper in the handwriting of Sir W. Harris, of which the following is a copy.

The several parcels of manuscript papers by the late Mr Cavendish, which the Earl of Burlington did me the honor to place in my hands with a view to an examination and report on their contents may be taken at 24 in number. Twenty of these contain sundry Philosophical papers on Mathematical and Experimental Electricity, and Four sundry other Papers relating to Meteorology.

All these Papers are more or less confused as to systematic arrangement, and require some considerable attention in decyphering. They are in many instances rather notes of experiments and rough drafts intended as a basis for more perfect productions than finished Philosophical Papers.

They are nevertheless extremely valuable and most interesting as evidence of Mr Cavendish's great Philosophical †   , and clearly prove that he had anticipated nearly all those great facts in common electricity which at a later period were made known to the scientific world through the writings of Coulomb and the French philosophers.

Papers on Electricity.

Of the 20 parcels of papers on electricity 18 belong to the years 1771, 1772 & 1773, and have never yet appeared in print; the two remaining parcels are dated 1775 and 1776, and are evidently connected with the author's celebrated paper on the Torpedo published in the *Royal Society's Transactions* for 1776.

* P. 23 (straw electrometer), p. 45 (globe and hemispheres), p. 58 (specific inductive capacity), p. 121 (measures of electricity), p. 208 (law of force), p. 223 (induction at a great distance).

† So in MS.

The papers belonging to the years 1771, 1772 & 1773 consist of six papers on Mathematical Electricity, nine experimental papers, one of Diagrams and Figures, the remainder are of a miscellaneous character, and contain some interesting Notes and Remarks and Thoughts on Electricity.

On examining the 20 parcels of manuscripts I found their contents to be as follows:

No. 1. MS. pp. 1–10. Apparently an early form of the "Preliminary Propositions."

No. 2. MS. pp. 1–31. Draft of "Preliminary Propositions" as far as Prop. XXVII.

No. 3. MS. L. 3 to L. 23. Contains the same propositions in a less complete form and not numbered, also two drafts of the propositions on coated plates, each 12 pp., and 38 loose pages of drafts of propositions, and jottings of algebraical calculations.

No. 4. MS. pp. 1–48. The fair copy of the "Preliminary Propositions." Props. XXIX. to XXXVII. Refers to figs. 1 to 10 of No. 15. See Arts. 140–174.

No. 5. MS. pp. 1–20. "Appendix." Refers to fig. 11. See Arts. 175–194.

No. 6. "Computations for explanation of experiments."
MS. pp. 1–15. Drafts of the propositions.
16 pages of computations. "B. 17." Charge of a sphere within a concentric sphere. [This is placed here as a note to Art. 335.]
"Attractions of elect. bodies more accurate," pp. 1–4.

No. 7. MS. D. 1 to D. 13. Fair copy of First and Second Experiments. Refers to Figs. 12, 13. See Arts. 217–235.
Draft of ditto marked "DIA."

No. 8. MS. pp. 1–7. Refers to Fig. 31. See Arts. 386–394.

No. 9. MS. pp. 1–51. Continuation of Experiments. See Arts. 236–294.

No. 10. MS. pp. 52–132. Part* of Experiments. See Arts. 295–385.

No. 11. MS. pp. 1–8. 1A. p. 10 A. 8, 9. p. 29 A. p. 32 A. 1 and 2. pp. 57–64. pp. 85, 86. pp. 91–96. pp. 103–108. pp. 119–126. pp. 133–138. pp. 141, 142. pp. 156–166.
All drafts of portions of the Account of Experiments.

No. 12. "Experiments 1771," MS. pp. 1–24. See Arts. 438–465. Also 14 loose sheets of calculations and measurements.

No. 13. "Experiments 1772," MS. pp. 1–29. See Arts. 466–493.
M. 1 to M. 13. Measurements of glasses, &c. See Arts. 592–595.

No. 14. Experiment 1773, MS. pp. 1–135. See Arts. 494–580.
Index to elect. exper. 1773, pp. 1–8. See Contents.
Dimensions of trial plates, 4 pages.

No. 15. Figures and Diagrams.

1 to 10 refer to	Preliminary propositions	No. 4
11	Appendix	No. 3
12 ,, 13 ,, ,,	Exp. 1	No. 7
14 ,, 19 ,, ,,	Experiments, Part 1	No. 9
20 ,, 27 ,, ,,	,, Part 2	No. 10
30 ,, ,,	Electrometer	No. 9
31 ,, ,,	Repulsion	No. 8

* So in MS.

No. 16. "Result." MS. pp. 1–21. See Arts. 647–683.

No. 17. "Notes." 4 pp. notes to "Thoughts concerning Electricity." These are inserted in their proper places, Arts. 195–216.

MS. pp. 1–15. Drafts of propositions for the paper of 1771, but founded on the theory stated in the "Thoughts." They are given in Note 18.

No. 18. "Thoughts concerning Electricity," MS. pp. 1–16. See Arts. 195–216.

No. 19. Resistance to Electricity, MS. pp. 1–23. See Arts. 616–631. "Res." Results of ditto, pp. 1–4. See Arts. 684–696. Resistance of Copper wire, pp. 1–38. See Arts. 636–646. Calibration of Tubes. See Arts. 632–635.

No. 20. Experiments with the artificial Torpedo, pp. 1–26. See Arts. 596–615. M. 1 to M. 42. Measurement of Leyden jars and batteries and of thickness of plates. See Arts. 581–591. "Extract from Dr Williamson's exper. on elect. Eel made in july 1773" pp. 1 to 14 + 4 pp. (See *Phil. Trans.* 1775, p. 94.)

In Art. 349, p. 175 of this book, Cavendish uses the expression "when I wrote the second part* of this work." It appears from this that he meant it for a book, not a paper to be communicated to the Royal Society. Several portions of this book are contained in the manuscripts, but the order in which they were intended to be placed can be discovered only by help of the figures and diagrams, which are numbered from 1 to 31.

From these it appears that we must begin with No. 4 and No. 5, the Preliminary Propositions† and the Appendix‡. The Preliminary Propositions refer to the printed paper of 1771. The last proposition in that paper is numbered XXVII., and the first in the MS. is XXIX., so that one proposition appears to be missing, but as there are several drafts, in all of which the first proposition is numbered XXIX., it is probable either that Prop. XXVIII. is not lost, but must be sought for among the enunciations in the second part of the printed paper, or else that Cavendish made a mistake in numbering his propositions.

The Lemmas, however, are numbered consecutively, the last in the printed paper being Lemma XI. and the first in the MS. Lemma XII.

The other mathematical manuscripts are either drafts of these propositions or jottings of calculations not intended for publication.

The paper entitled "Thoughts concerning electricity"§ (No. 18) is placed next. It forms a suitable introduction to the account of the experiments, as it indicates the leading ideas of Cavendish's researches. The paper has no date, but its contents show that it is an earlier form of the theory of electricity, which Cavendish had already abandoned before he wrote the paper of 1771. The propositions in No. 17 belong to this form of the theory, and are given in Note 18.

* This seems to refer to the second part of the paper in the *Phil. Trans.* 1771, p. 670, or [p. 66] of this edition, and shows that this paper was intended to form the first part of the "Work."

† Arts. 140 to 174. ‡ Arts. 175 to 194.

§ Arts. 195 to 216.

We have next the account of the experiments, the order of which is

No. 7	Figs. 12 to 13	Exp. I. and II.	Arts. 217 to 235
No. 9	Figs. 14 „ 19	Exps. III. to VIII.	Arts. 236 „ 294
No. 10	Figs. 20 „ 30		Arts. 295 „ 385
No. 8	Fig. 31		Arts. 386 „ 394

The style in which these papers are written leaves no doubt that they were intended to form a book, and to be published. They are given here without any alteration except in the case of a few abbreviations the meaning of which is either obvious or is explained in some other part of the MS. I have also divided them into articles for the sake of more convenient reference. All additions to the MS. are enclosed in square brackets.

After this I have placed the paper on the Torpedo from the *Philosophical Transactions* for 1776. This, I think, is the whole of the "work" which is extant, but it is by no means a complete account of Cavendish's electrical researches. There are three forms in which Cavendish recorded the results of his experiments:

1st. A Journal containing notes of every observation as it was made, with the particulars of the experiments, and measurements of the apparatus.

2nd. "Results," containing a comparison of the different measures of quantities as recorded in the Journal, and a deduction of the most probable result. See Arts. 647–696.

3rd. An account of the experiments written for publication.

I have reproduced the journals for 1771* and 1772† entire, because they form a good example of Cavendish's method of work, and because they contain all the data of some of the most important electrical measurements.

The journal for 1773‡ is much larger than the others, and gives an account of many interesting and important researches.

Many pages of this journal, however, are filled with the details of the experiments for the comparison of the coated plates which Cavendish used as standards of capacity. These experiments differ in no respect from those in the former journals, and all the conclusions which Cavendish deduced from them are stated by himself in the "Results." I have therefore thought it best to omit them from the journal, but to retain Cavendish's heading of each experiment and its date when known, and to make the numbers of the omitted articles run on continuously with those retained.

Many of the entries in the journals give the day of the week and of the month, but very few of them give the year. I have therefore ascertained in what years the stated days of the week and month coincided, and have inserted the most probable year within square brackets. It thus appears that the journal entitled "Experiments in 1773" begins with

* Arts. 438 to 465. † Arts. 466 to 493. ‡ Arts. 494 to 580.

experiments made in October, 1772. Cavendish appears, however, to have got wrong in his reckoning for a good many days together during that month. See Art. 502.

It is somewhat difficult to account for the fact, that though Cavendish had prepared a complete description of his experiments on the charges of bodies, and had even taken the trouble to write out a fair copy, and though all this seems to have been done before 1774, and he continued to make experiments in electricity till 1781, and lived on till 1810, he kept his manuscript by him and never published it. It was not till 1784 that he communicated to the Royal Society those "Experiments on Air," including the production of water and of nitric acid, the absorbing interest of which might perhaps account for some neglect of his electrical writings.

Cavendish cared more for investigation than for publication. He would undertake the most laborious researches in order to clear up a difficulty which no one but himself could appreciate, or was even aware of, and we cannot doubt that the result of his enquiries, when successful, gave him a certain degree of satisfaction. But it did not excite in him that desire to communicate the discovery to others which, in the case of ordinary men of science, generally ensures the publication of their results. How completely these researches of Cavendish remained unknown to other men of science is shown by the external history of electricity.

Viscount Mahon, afterwards Lord Stanhope, a man of great ingenuity and fertility in invention, a pupil of Le Sage of Geneva, and the inventor of the printing press which bears his name, published in 1779 his *Principles of Electricity*. The theory developed in this book is that

A positively electrified body surrounded by air will deposit upon all the particles of that Air which shall come successively into contact with it, a proportional part of its *superabundant* Electricity, By which means, the *Air* surrounding that body will also become *positively* electrified: that is to say, it will form round that positive body, an electrical atmosphere, which will likewise be positive. (p. 7.)

That the electrical *Density* of all such Atmospheres decreases, when the distance from the charged Body is increased. (p. 14.)

He then proceeds to determine the law of the density of the electrical atmosphere, as it depends on the distance from the charged body. He assumes that if a cylinder with hemispherical ends is placed in the electrical atmosphere of a charged body, the density of the electricity at any part of the cylinder will depend on the density of the electrical atmosphere in contact with it.

He also shows by experiment that if the cylinder is insulated, and originally without charge, it does not become charged as a whole by being immersed in the electrical atmosphere of a charged body. Hence, when the electricity of the cylinder is disturbed, the whole positive charge on one portion of the surface is numerically equal to the whole negative charge on the other portion.

Now if the density (on the cylinder) were inversely as the distance from the charged body, a transverse section of the cylinder whose distance from the charged body is the geometric mean of the distances of the ends, would divide the charge into two equal parts (both of course of the same kind of electricity), but if the density were inversely as the square of the distance, the distance of the section which would bisect the charge would be the harmonic mean of the distance of the ends. In all this he tacitly confounds the point of bisection of the charge with the neutral point.

He then shows by experiment that the actual position of the neutral point agrees sufficiently well with the harmonic mean, but not with the geometric mean, and from this he concludes (p. 65),

Consequently, it evidently appears, from what was said above, that the Density of the Electricity, of the electrical Atmosphere (in which the said Body *A*, *B* was immersed) was in the inverse Ratio of the square of the Distance.

It is evident from this that Lord Mahon was entirely ignorant of everything which Cavendish had done.

About the close of the century Dr Thomas Young, whose acquaintance with all branches of science was as remarkable for its extent as for its profundity, says of this neutral point§:

It was from the situation of this point that Lord Stanhope first inferred the true law of the electric attractions and repulsions, although Mr Cavendish had before suggested the same law as the most probable supposition.

The same writer, in his "Life of Cavendish," in the Supplement to the *Encyclopædia Britannica**, gives the following account of the first paper on electricity.

3. *An Attempt to explain some of the principal Phenomena of Electricity by means of an Elastic Fluid.* (*Phil. Trans.* 1771, p. 584.) Our author's theory of electricity agrees with that which had been published a few years before by Æpinus, but he has entered more minutely into the details of calculation, showing the manner in which the supposed fluid must be distributed in a variety of cases, and explaining the phenomena of electrified and charged substances as they are actually observed. There is some degree of unnecessary complication from the great generality of the determinations: the law of electric attraction and repulsion not having been at that time fully ascertained, although Mr Cavendish inclines to the true supposition, of forces varying inversely as the square of the distance: this deficiency he proposes to supply by future experiments, and leaves it to more skilful mathematicians to render some other parts of the theory still more complete. He probably found that the necessity of the experiments, which he intended to pursue, was afterwards superseded by those of Lord Stanhope and

[* Reprinted, at the end of this volume, from Young's *Miscellaneous Works*, vol. II.]

§ *Lectures on Natural Philosophy*, London 1807, vol. I. Lecture liii, p. 664.

M. Coulomb; but he had carried the mathematical investigation somewhat further at a later period of his life, though he did not publish his papers; an omission, however, which is the less to be regretted, as M. Poisson, assisted by all the improvements of modern analysis, has lately treated the same subject in a very masterly manner. The acknowledged imperfections, in some parts of Mr Cavendish's demonstrative reasoning, have served to display the strength of a judgment and sagacity still more admirable than the plodding labours of an automatical calculator. One of the corollaries* seems at first sight to lead to a mode of distinguishing positive from negative electricity, which is not justified by experiment; but the fallacy appears to be referable to the very comprehensive character of the author's hypothesis, which requires some little modification to accommodate it to the actual circumstances of the electric fluid, as it must be supposed to exist in nature.

No man was better able than Dr Young to appreciate the scientific merits of Cavendish, and it is evident that he spared no pains in obtaining the data from which he wrote this sketch of his life, yet this account of his electrical researches shows a complete ignorance of Cavendish's unpublished work, and this ignorance must have been shared by the whole scientific world.

Dr Young, as it appears from the above extract, was aware of the existence of unpublished papers by Cavendish relating to electricity, but he supposed that these papers were entirely mathematical, and that "he probably found that the necessity of the experiments, which he intended to pursue, was afterwards superseded by those of Lord Stanhope and M. Coulomb."

We now know that the unpublished mathematical papers were entirely subsidiary to the experimental ones, and it is plain from Art. 95 that Cavendish had actually made some of his experiments before the paper of 1771, and that all those on electrostatics were completed before the end of 1773.

The favourable reception which Lord Stanhope's very interesting and popular experiments met with may have influenced Cavendish not to publish his own, but his estimate of their value as a foundation for a theory of electricity may be gathered from the fact, that in his "Thoughts concerning Electricity," which appears to be his earliest writing on the subject, he devotes two pages (Arts. 195–198) to the refutation of the very theory of electric atmospheres which is the basis of Lord Stanhope's reasoning; whereas in the paper of 1771, which contains his more matured views, he does not even allude to that theory.

It was not till 1785 that the first of the seven electrical memoirs of M. Coulomb was published. The experiments recorded in these memoirs furnished the data on which the mathematical theory of electricity, as we now have it, was actually founded by Poisson, and it is impossible to

* Art. 49 and Note 1.

overestimate the delicacy and ingenuity of his apparatus, the accuracy of his observations, and the sound scientific method of his researches; but it is remarkable, that not one of his experiments coincides with any of those made by Cavendish. The method by which Coulomb made direct measurements of the electric force at different distances, and that by which he compared the density of the surface-charge on different parts of conductors, are entirely his own, and were not anticipated by Cavendish. On the other hand, the very idea of the capacity of a conductor as a subject of investigation is entirely due to Cavendish, and nothing equivalent to it is to be found in the memoirs of Coulomb.

The leading idea which distinguishes the electrical researches of Cavendish from those of his predecessors and contemporaries, is the introduction of the phrase "degree of electrification" with a clear scientific definition, which shows that it is precisely equivalent to what we now call potential.

In his first published paper (1771), he begins at Art. 101 by giving a precise sense to the terms "positively and negatively electrified," which up to that time had been in common use, but were often confounded with the terms "over and under charged," and in Art. 102 he defines what is meant by the "degree of electrification."

We find the same idea, however, in the much earlier draft of his theory in the "Thoughts concerning Electricity," Art. 201, where the degree of electrification is boldly, if somewhat prematurely, explained in a physical sense, as the *compression*, or as we should now say, the *pressure*, of the electric fluid.

We can trace this leading idea through the whole course of the electrical researches.

He shows that when two charged conductors are connected by a wire they must be electrified in the same degree, and he devotes the greater part of his experimental work to the comparison of the charges of the two bodies when equally electrified.

He ascertained by a well-arranged series of experiments the ratios of the charges of a great number of bodies to that of a sphere 12·1 inches in diameter, and as he had already proved that the charges of similar bodies are in the ratio of their linear dimenions, he expressed the charge of any given body in terms of the diameter of the sphere, which, when equally electrified, would have an equal charge, so that when in his private journals he speaks of the charge of a body as being so many "globular inches," or more briefly, so many "inches of electricity," he means that the capacity of the body is equal to that of a sphere whose diameter is that number of inches.

In the present state of electrical science, the capacity of a body is defined as its charge when its potential is unity, and the capacity of a sphere as thus defined is numerically equal to its radius. Hence, when Cavendish says that a certain conductor contains n inches of electricity, we may express his result in modern language by saying that its electric capacity is $\frac{1}{2}n$ inches.

In his early experiments he seems to have endeavoured to obtain a number of conductors as different as possible in form, of which the capacities should be nearly equal. Thus we find him comparing a pasteboard circle of 19·4 inches in diameter with his globe of 12·1 inches in diameter, but finding the charge of the circle greater than that of the globe, he ever after uses a circle of tin plate, 18·5 inches in diameter, the capacity of which he found more nearly equal to that of the globe.

In like manner the first wire that he used was 96 inches long and 0·185 diameter, but afterwards he always used a wire of the same diameter, but 72 inches long, the capacity of which was more nearly equal to that of the globe.

He also provided himself with a set of glass plates coated with circles of tin-foil on both sides. These plates formed three sets of three of equal capacity, the capacities of the three sets being as 1, 3 and 9, with a tenth coated plate whose capacity was as 27.

Besides these he had "double" plates of very small capacity made of two plates of glass stuck together, and also other plates of wax and rosin, the inductive capacity of these substances being, as he had already found, less than that of glass; and jars of larger capacity, ranging up to his great battery of 49 jars, whose capacity was 321,000 "inches of electricity." In estimating the capacity of his battery, he used the method of repeated touching with a body of small capacity. (Arts. 412, 441, 582.) This method is the same as that used by MM. Weber and Kohlrausch in their classical investigation of the ratio of the electric units*.

Thus the method of experimental research which Cavendish adhered to was the comparison of capacities, and the formation of a graduated series of condensers, such as is now recognised as the most important apparatus in electrostatic measurements.

We have next to consider the steps by which he established the accuracy of his theory, and the discoveries he made respecting the electrical properties of different substances.

Cavendish himself, in his description of his experiments, has shown us the order in which he wishes us to consider them. The first experiment† is that of the globe within two hemispheres, from which he proves that the electric force varies inversely as the square of the distance, or at least cannot differ from that ratio by more than a fiftieth part. The degree of accuracy of all the experiments was limited by the sensitiveness of the pith ball electrometer which he used. Bennett's gold leaf electrometer, which is much more sensitive, was not introduced till 1787, but in repeating the experiment we can now use Thomson's Quadrant electrometer, and

* *Elektrodynamische Maasbestimmungen*, Abh. IV. p. 235.

† Arts. 217 to 235.

thereby detect a deviation from the law of the inverse square not exceeding one in 72,000. See Note 19.

The second experiment, Art. 135, is a repetition of the first with bodies of different shape.

The third experiment, Art. 265, shows that in comparing the charges of bodies, the place where the connecting wire touches the body, and the form of the connecting wire itself, are matters of indifference.

The fourth experiment, Art. 269, shows that the charges of bodies of the same shape and size, but of different substances, are equal.

The fifth, Art. 273, compares the charge of a large circle with that of two of half the diameter. According to the theory the charge of the large circle should be equal to that of the two small ones if they are at a great distance from each other, and equal to twice that of the small ones if they are close together. Cavendish tried them at three different distances and compared the results with his calculations.

The sixth experiment, Art. 279, compares one long wire with two of half the length and half the diameter, placed at different distances.

The seventh, Art. 281, compares the charges of a globe, a circle, a square, an oblong and three different cylinders, and the eighth, Art. 288, shows that the charge of the middle plate of three parallel plates is small compared with that of the two outer ones.

Cavendish next describes his experiments for comparison of the charges of coated plates of glass and other substances, but begins by examining the sources of error in measurements of this kind.

The first of these which he investigates is the spreading of electricity on the surface of the plates beyond the coatings of tinfoil. He distinguishes two kinds of this spreading, one a gradual creeping of the electricity over the surface of the glass, Art. 300, and the other instantaneous, Art. 307.

He attempted to check the first kind by varnishing the glass plates and by enclosing their edges in a thick frame of cement, but he found very little advantage in this method, and finally adopted the plan of performing all the operations of the experiment as quickly as possible, so as to allow very little time for the gradual spreading of the electricity.

He next investigated the instantaneous spreading of electricity on the glass near the edge of the coating. He noticed that at the instant of charging the plate in the dark, a faint light could be seen all round the edges. He also observed that after charging and discharging a coated plate of glass many times without cleaning it, a narrow fringed ring of dirt could be traced all round the coating, the space between this ring and the coating being clean, and in general about $\frac{1}{10}$ inch broad.

He also observed that the flash of light was stronger the first or second times of charging a plate than afterwards.

To determine how much the capacity of a coated plate was increased by this spreading of the electricity, he compared the capacity of a plate with a circular coating with that of the same plate with a new coating of nearly the same area, but cut into strips, so that its perimeter was very much greater than that of the circular coating.

In this way he found that if we suppose a strip of uniform breadth added to the coating all round its boundary, the capacity of this coating, supposing the electricity not to spread, will be equal to that of the actual coating as increased by the spreading of the electricity. The most probable breadth of this strip he found to be 0·07 inch for thick glass and 0·09 for thin.

When this correction was applied to the areas of the coatings of the different coated plates, the computed charges of plates made of the same kind of glass were found to be very nearly in the same ratio as their observed charges.

But the observed charges of coated plates were found to be always several times greater than the charges computed from their thickness and the area of their coatings, the ratio of the observed charge to the computed charge being for plate glass about 8·2, for crown glass about 8·5, for shellac about 4·47, and for bees' wax about 3·5. Thus Cavendish not only anticipated Faraday's discovery of the Specific Inductive Capacity of different substances, but measured its numerical value in several substances.

The values of the specific inductive capacity of various substances as determined by different modern observers are compared with those found by Cavendish in the table in Note 27.

To make it certain, however, that the difference between the observed and calculated capacities of coated plates really arose from the nature of the plate and not from some error in the theory, Cavendish determined the capacity of a "plate of air," that is to say a condenser consisting of two circles of tinfoil on glass with air between them. The capacity of a plate of air was found to be much less than that of a plate of glass or of wax of the same dimensions, but it seemed to be about $\frac{1}{11}$ in excess of the calculated value. This discrepancy will be discussed in Note 17.

These may be considered the principal results of the investigations with coated plates, but the following list of collateral experimental researches will show how thoroughly Cavendish went to work.

A question of fundamental importance in the theory of dielectrics is whether the electric induction is strictly proportional to the electromotive force which produces it, or in other words, is the capacity of a condenser made of glass or any other dielectric the same for high and for low potentials?

The form in which Cavendish stated this question was as follows*:—
"Whether the charge of a coated plate bears the same proportion to that of a simple conductor, whether the electrification is strong or weak."

* Art. 526.

Cavendish, who explained the fact that the capacity of a glass plate is greater than that of an air plate, by supposing that the electricity is free to move within certain portions of the glass, supposed that when the plate was more strongly electrified the electricity would be able to penetrate further into the glass, and that therefore its charge would be greater in proportion to that of a simple conductor or a plate of air the stronger the degree of electrification.

But according to the experiments he made to answer this question* a coated plate and a simple conductor whose charges were equal for the usual degree of electrification remained sensibly equal for higher and lower degrees, and if, as appeared probable from the experiments on the spreading of electricity at the edge of the coating, this spreading extended further for high degrees of electrification than for low, it would be necessary to admit that the charge of a glass plate became less in proportion to that of a simple conductor as the degree of electrification increased. Cavendish, however, concluded that the experiments were hardly accurate enough to warrant the deduction from them of so improbable a conclusion.

He also found that the result of the comparison of a coated plate and a simple conductor was the same whether they were charged positively or negatively.

He tried whether the capacity of a plate of rosin altered with the temperature, but he could not find that it did†. In glass he found that the capacity increased as the temperature rose, but the most decided increase did not occur till the glass began to conduct somewhat freely. Cavendish therefore does not consider the experiment quite decisive‡.

He found that the apparent capacity of a Florence flask§ was greater when it continued charged a good while than when it was charged and discharged immediately, and he found that the same was the case with a coated globe of glass. This phenomenon, which Faraday called "electric absorption," has recently been carefully studied in different kinds of glass by Dr Hopkinson‖. It is connected with the long-known phenomenon of the "residual charge," and the existence of such phenomena in many dielectrics renders it difficult to obtain consistent values of their inductive capacities; for the more rapidly the charging and discharging is effected the lower is the apparent value of the capacity. It is for this reason that condensers of glass cannot be used as standards of capacity when accurate measurements are desired.

Franklin had shown¶ that the charge of a glass condenser resides in the glass and not in the coatings, for when the coatings were removed they were found to be without charge, and when new coatings were put in their place the condenser thus reconstructed was found to be charged.

* Arts. 355–365. † Art. 523. ‡ Art. 366.
§ Art. 523. ‖ *Phil. Trans.* 1877, p. 599.
¶ *Franklin's Works*, ed. Sparks, vol. v. p. 201.

Cavendish tried whether this was the case with a charged plate of air, by lifting one of the electrodes and changing the air between them and then replacing the electrode. He found that the charge was not altered during these operations, and concluded that the charge resides, not in the air, but in the metal plates.

In Arts. 336 to 339 we find a most ingenious method of determining by experiment the effect of the floor, walls and ceiling of a room, and of other surrounding objects, in increasing the apparent capacity of a conductor placed in a given position in the room. The method consists in measuring the capacities of two conductors of the same shape but of different dimensions, the centre of each being at the given point in the room. If the experiment had been made with the conductors at an infinite distance from all other bodies their capacities would have been in the ratio of their corresponding dimensions, but the effect of surrounding objects is to make their capacities vary in a higher ratio than that of their dimensions, and from the measured ratio of the two capacities, the correction for the effect of surrounding objects on the capacity of any small body may be calculated.

Cavendish also verified by experiment what he had already proved theoretically, that the capacity of two condensers is not sensibly altered when they are placed near to each other or far apart.

But besides this series of experiments on electric capacity, another course of experiments on electric resistance was going on between 1773 and 1781, the knowledge of which seems never to have been communicated to the world.

In his paper on the Torpedo in the *Philosophical Transactions* for 1776 (Art. 398) he alludes to "some experiments of which I propose shortly to lay an account before this Society," but he never followed up this proposal by divulging the method by which he obtained the results which he proceeds to state—"that iron wire conducts about 400 million times better than rain or distilled water*," and that "sea water, or a solution of one part of salt in 30 of water conducts 100 times, and a saturated solution of sea-salt about 720 times better than rain water."

Such was the reputation of Cavendish for scientific accuracy, that these bare statements seem to have been accepted at once, and soon found their way into the general stock of scientific information, although no one, as far as I can make out, has ever conjectured by what method Cavendish actually obtained them, more than forty years before the invention of

* This is equivalent to saying that iron wire conducts 555,555 times better than saturated solution of sea salt. A comparison of the experiments of Matthiessen on iron with those of Kohlrausch on solutions of sodium chloride at 18° C., would make the ratio 451,390. The resistance of iron increases and that of the solution diminishes as the temperature rises, and at a temperature of about 11° C. the ratio of the resistances would agree with that given by Cavendish.

the galvanometer, the only instrument by which any one else has ever been able to compare electric resistances.

We learn from the manuscripts now first published, that Cavendish was his own galvanometer. In order to compare the intensity of currents he caused them to pass through his own body, and by comparing the intensity of the sensations he felt in his wrist and elbows, he estimated which of the two shocks was the more powerful.

As Cavendish does not appear to have prepared an account of these experiments in the manner in which he usually wrote out what he intended to publish, it may be well to describe them here, as we collect them from different parts of his Journals.

The conductors to be compared were for the most part solutions of common salt of known strength or of other substances. These solutions were placed in glass tubes, more than a yard long, bent near one end. The tubes had been previously calibrated by means of mercury.

Two wires were run into the tube, probably through holes in corks at each end, to serve as electrodes. The length of the effective column of the liquid could be altered by sliding the wire in the straight part of the tube.

In order to send electric discharges of equal quantity and equal electromotive force through two different tubes Cavendish chose six jars of nearly equal capacity from "Nairne's last battery." The two tubes to be compared were placed so that the wires run into their bent ends communicated with the outside of this battery of six jars. The wires run into the straight ends of the tubes were fastened to two separately insulated pieces of tinfoil. The six jars were then all charged at once by the same conductor till the gauge electrometer indicated the proper degree of electrification. The conductor was then removed, so that the six jars remained with their inside coatings insulated from each other and equally charged.

Cavendish then taking two pieces of metal, one in each hand, touched with one the tinfoil belonging to one of the tubes to be compared, and then with the other touched the knob of jar No. 1, so as to receive a shock, the charge passing through his body and the first tube.

He next laid one of the metals on the tinfoil of the second tube, and then touching with the other the knob of jar No. 2, he received a second shock, the discharge passing through his body and the second tube.

In this way he took six shocks, making them pass alternately through the first and the second tube, and proceeded to record his impression whether the intensity of the shock through the second tube was greater or less than that of the shock through the first, and concluded that the tube which gave the greater shock had the smaller resistance.

He then adjusted the wire in one of the tubes so as to make the resistance more nearly equal to that of the other, and repeated the experiment, always recording his impression of the result, till he found that one adjustment made the shock of the second tube sensibly greater than that of the first, and that another adjustment made it sensibly less.

From the result of the whole series of experiments he judged what adjustment would make the two shocks exactly equal.

Instead of using six jars only, he seems latterly to have used the whole battery, electrifying one row to a given degree and then communicating this charge to the whole battery, and taking the discharge of one row at a time through the tubes alternately. He seems to have found some advantage in thus using a discharge of greater quantity and smaller electromotive force.

The accuracy which Cavendish attained in the discrimination of the intensity of shocks is truly marvellous, whether we judge by the consistency of his results with each other, or whether we compare them with the latest results obtained with the aid of the galvanometer, and with all the precautions which experience has shown to be necessary in measuring the resistance of electrolytes.

One of the most important investigations which Cavendish made in this way was to find, as he expressed it, "what power of the velocity the resistance is proportional to*."

Cavendish means by "resistance" the whole force which resists the current, and by "velocity" the strength of the current through unit of area of the section of the conductor.

(In modern language the word resistance is used in a different sense, and is measured by the force which resists a current of unit strength.)

By four different series of experiments on the same solution in wide and in narrow tubes, Cavendish found that the resistance (in his sense) varied as the

$$1{\cdot}08,\ 1{\cdot}03,\ 0{\cdot}976,\ \text{and}\ 1{\cdot}00$$

power of the velocity.

This is the same as saying that the resistance (in the modern sense) varies as the

$$0{\cdot}08,\ 0{\cdot}03,\ -0{\cdot}024$$

power of the strength of the current in the first three sets of experiments, and in the fourth set that it does not vary at all.

This result, obtained by Cavendish in January, 1781, is an anticipation of the law of electric resistance discovered independently by Ohm and published by him in 1827. It was not till long after the latter date that the importance of Ohm's law was fully appreciated, and that the measurement of electric resistance became a recognised branch of research. The exactness of the proportionality between the electromotive force and the current in the same conductor seems, however, to have been admitted, rather because nothing else could account for the consistency of the

* Arts. 574, 575, 629, 686.

measurements of resistance obtained by different methods, than on the evidence of any direct experiments.

Some doubts, however, having been suggested with respect to the mathematical accuracy of Ohm's law, the subject was taken up by the British Association in 1874, and the experiments of Professor Chrystal, by which the exactness of the law, as it relates to metallic conductors, was tested by currents of every degree of intensity, are contained in the Report of the British Association for 1876.

The laws of the strength of currents in multiple and divided circuits are accurately stated by Cavendish in Arts. 417, 597, 598.

Cavendish applied the same method of experiment to compare the resistance of the same liquid at different temperatures*, and he found that "salt in 69 [of water] conducts 1·97 times better in heat of 105 than in that of 58½." He also found that "the proportion of the resistance of saturated solution and salt in 999 to each other seems not much altered by varying heat from 50 to 95."

Kohlrausch, who has made a most extensive series of experiments on the resistance of electrolytes, gives results from which it appears that the ratio of the resistances of salt in 69 at 105° F. and at 58½° F. would be 1·59. He also finds that the temperature coefficient for solutions of salt alters very little with the strength. See Note 33.

Cavendish also tested the resistance of solutions of salt of strengths varying from saturation to one in 20,000 of distilled water, and arrived at the result, which Kohlrausch has shown to be nearly accurate, that for weak solutions the product of the resistance into the percentage of salt is nearly constant.

Of all substances, that for which different observers have given the most different measures of resistance is pure water.

It has been found indeed that the presence of the minutest trace of impurity in water diminishes its resistance enormously. Thus Kohlrausch found that it was necessary to use water quite freshly distilled in platinum vessels, for if placed in a glass vessel it rapidly diminished in resistance by dissolving a minute quantity of the glass, and a few minutes exposure to the air of the laboratory, by impregnating the water with a trace of tobacco smoke, was found sufficient to spoil it for a determination of resistance. Kohlrausch indeed estimates that the electric conductivity which he observed in the purest water he could obtain might be accounted for by the presence of no more than one ten millionth part of hydrochloric acid, a quantity which no chemical analysis could detect. Hence the hypothesis that water is a non-conductor of electricity, if not true, cannot be disproved.

Some of these remarkable properties of water were detected by Cavendish. He found that the resistance of pump water was 4⅙ times less

* Art. 691.

than that of rain water, and that of rain water was 2·4 times less than that of distilled water*.

In January 1777, he found that salt in 2999 conducted about 70 or 90 times better than some water distilled in the preceding summer but only about 25 or 50 times better than the distilled water used in the year 1776†, and that the conductivity of distilled water increased by standing two or three hours in a glass tube‡.

He also found that in order to make the conducting powers of his weakest solutions of salt agree with the hypothesis that they are as the quantity of salt in them, it would be necessary only to suppose that his distilled water contained one part of salt in 120,000§.

It was found that distilled water impregnated with fixed air from oil of vitriol and marble conducted $2\frac{1}{2}$ times better than the same water deprived of its air by boiling‖, and that the presence of absorbed air in a weak solution of salt seemed to increase its conductivity¶.

In order to find whether electricity is resisted in passing out of one medium into another in perfect contact with it, Cavendish prepared a tube containing 8 columns of saturated solution of sea salt enclosed between columns of mercury. He found that the shock was diminished in passing through a mixed column in which the length of salt water was 21·8 inches as much as in passing through a single column of the same size whose length was 22·94 inches**.

The difference would have been far greater if the comparison had been made with an ordinary galvanometer and continued currents which rapidly produce polarization, but with the small quantities of electricity which Cavendish used, the effect of polarization would hardly be sensible.

He also made a compound conductor consisting of 40 bits of tin soldered together. The shock through this appeared to be of the same strength as through a single piece of the same size. This experiment however is not of much value, as the resistance of the conductor was far too small compared with that of Cavendish's body to give good results††.

We now come to a very remarkable set of experiments which Cavendish made on a series of salts and acids in order to determine their relative electric resistance. They are recorded in Arts. 626, 627 and 694, and are dated Jan. 13 and 15, 1777.

The strength of the different solutions was such, as Cavendish tells us, "that the quantity of acid in each should be equivalent to that in a solution of salt in 29 of water."

* Art. 525. † Art. 690. ‡ Art. 621.

§ Art. 630. ‖ Arts. 625, 693. ¶ Art. 692. ** Art. 578.

†† Art. 579. The resistance of a man's body, from one hand to the other, varies from about 1000 ohms when the hands are well wetted with salt water, to about 12,000 when the hands are dry. When the outer skin is removed by a blister, the resistance is very much diminished. The resistance of the compound conductor was probably a fraction of an ohm. See Note 31.

The total weight of each solution was 3 pounds 10 ounces and 12 pennyweights, or 1116 pennyweights Troy. The quantity of each substance when reduced to pennyweights is in every case very nearly the equivalent weight of that substance in the system adopted at present, in which the equivalent weight of hydrogen is taken as unity*.

Now these experiments were made in 1777, and it is difficult to see from what source, other than determinations of his own, he could have derived these numbers. Wenzel's *Lehre von den Verwandschaften* was published in 1777. I have not been able to consult the work itself, but from the account of it given in Kopp's *Geschichte der Chemie*, the equivalent numbers seem to have been larger than those used by Cavendish. Richter's *Anfangsgründe der Stöchyometrie* was not published till 1792.

It is difficult to account for the agreement not only of the ratios but of the absolute numbers given by Cavendish with those of the modern system, in which the equivalent weight of hydrogen is taken as unity. I can only conjecture from several parts of his paper on Factitious Airs (*Phil. Trans.* 1766), that Cavendish was accustomed to compare the quantity of fixed air from different carbonates with that from 1000 grains of marble. Now the modern equivalent weight of marble is 100, so that if Cavendish took 100 pennyweights as the equivalent weight of marble, the equivalents of other substances would be as he has given them. This I think is more likely than that he should have selected inflammable air as his standard substance at a time when even his own experiments left it doubtful whether inflammable air was always of the same kind.

In his journal, Cavendish writes down these equivalent weights just as a modern chemist might do, without a hint that a list of these numbers was not at that time one of the things which every student of chemistry ought to know by heart. It is only by comparing the date of these researches with the dates of the principal discoveries in chemistry, that we become aware, that in the incidental mention of these numbers we have the sole record of one of those secret and solitary researches, the value of which to other men of science Cavendish does not seem to have taken into account, after he had satisfied his own mind as to the facts.

I take this opportunity of expressing my thanks to the many friends who have given me assistance in preparing this edition, and in particular to Mr C. Tomlinson, who gave me valuable information about the manuscripts; to Mrs Sime, who lent me a manuscript book of letters, &c., relating to Cavendish, collected by her brother, the late Dr George Wilson; to Mr W. Garnett, of St John's College, Cambridge, who copied out Arts. 236–294; and Mr W. N. Shaw, of Emmanuel College, who took the photographs from which the facsimile figures were executed; to Mr H. B. Wheatley, who furnished me with information connected with the history

* See Note 34.

of the Royal Society; to Prof. Dewar, Mr P. T. Main, Mr G. F. Rodwell, and Dr E. J. Mills, who gave me information on chemical subjects; and Mr Dew Smith and Mr F. M. Balfour, of Trinity College, and Prof. Ernst von Fleischl, of Vienna, who gave me information about electrical fishes, and the physiological effect of electricity.

P.S. 14th June, 1879.

Just before sending this sheet to press I have received from Mr Robert H. Scott, F.R.S., a small packet marked "Cavendish Papers," which had been sent to the Meteorological Office by Sir Edward Sabine.

These papers relate entirely to magnetism, and do not fall within the scope of this volume*, though they may supply important materials for the magnetic history of the earth, and are in all respects excellent specimens of Cavendish's scientific procedure.

I shall therefore only mention a few particulars in which these papers throw some additional light on Cavendish's life and work.

The descriptions of Cavendish by Cuvier, Young, Thomson and Wilson agree in representing him as living in London, and regularly attending the meetings of the Royal Society, but in other respects leading an isolated life, very much detached from the interests, whether social or scientific, of other men.

It has also been hinted that Lord Charles Cavendish, who, as we have already seen, was himself addicted to scientific pursuits, did not entirely approve of his son's devotion to science, or at least, for some reason or other, restricted him in the means of carrying on his work.

In these manuscripts, however, we have the details of a laborious series of observations undertaken to determine the errors of the variation compass and the dipping needle belonging to the Royal Society, and on Sept. 16, 1773, we find "Observations of needle in Garden by Father and Self," and a "Comparison of Society's compass in house and in soc[iety's] garden with Father's compass in room."

It appears, therefore, that Lord Charles Cavendish not only placed his instruments at his son's disposal, but made observations of the compass in concert with him, and that these observations were undertaken in order to make the instruments belonging to the Royal Society more available for accurate measurements. In the same Journal there are also "Measures taken for setting Dr Knight's magnets so that their poles shall be equidistant from var[iation] comp[ass] and dipp[ing] need[le] in 1775." The results of this enquiry are briefly stated by Cavendish in his paper on the Instruments belonging to the Royal Society in the "Philosophical Transactions" for 1776. In the same volume there is an account of Dr Knight's great Magazines of magnets by Dr Fothergill.

* [See Dr Chree's account of Cavendish's Magnetic Researches in vol. II.]

A considerable portion of the MS. is taken up with "Directions for using the Dipping Needle," written out at greater or less length (probably according to the scientific capacity of the recipient) "for Captain Pickersgill," "for Captain Bayley," "for Dalrymple" [Hydrographer to the Honourable East India Company] &c.

There is also a treatise of 26 pages "On the different forms of construction of dipping needles."

Besides this, there is a series of observations of the magnetic variation and also of the dip, at various times, from 1773 to August 1809 (Cavendish died Feb. 24, 1810).

These observations were made for the most part only in the summer months, but during that time were carried on with the greatest regularity, and results for each year calculated from them.

We also find the record of "Trials of Nairne's needle in different parts of England in August, 1778."

It was tried "in Garden, Aug. 8. In Garden of Observatory at Oxford, Aug. 14. At Birmingham, in Bowling-green, Aug. 15. At Towcester, in Garden, Aug. 17. At St Ives, in Garden, Aug. 18. At Ely, in Garden, Aug. 18. At London, Aug. 19 and 22." From these trials he finds that "Lines of equal dip should seem to run about 44° to south of west, and dip should increase about 42′ by going 1° to N.W."

There is a long and valuable series of experiments on the magnetic properties of forged iron, blistered steel, and cast iron. "Some bars were got from Elwell $31\frac{3}{4}$ inch long, 2·1 broad, and about ·5 thick. On May 29, 1776, one of each was made magnetical, the marked end being the south pole. In trying the experiment the bars were placed perpendicularly against a wall 25 inches distant from the center of the needle, $91°\frac{1}{4}$ to west of usual magnetic north, either the top or bottom of the bar being always on a level with the needle. They were kept constantly with the marked end upwards till after the observations of June 30, after which they were kept with the marked end downwards."

Cavendish determines in every case the "fixed magnetism" and the "moveable magnetism" of the bar, and also its magnetism when "struck 100 times on an anvil, falling 1·6 inches by its weight, and tried immediately after."

There are also 23 pages of experiments on the effect of heat on magnets, and a mathematical investigation of the bending of the dipping-needle by its own weight as affecting the determination of the dip, together with measurements of the elasticity of steel and of glass.

CAVENDISH'S HOUSE AT CLAPHAM

INTRODUCTION

COMPARATIVELY little is known concerning the personal history of the author of these memoirs. Nor is there much hope now that more may be gleaned. It may be doubted, indeed, whether there is much more to learn, for, apart from his scientific achievements, his life was singularly uneventful. He lived a solitary, secluded existence, and, despite his rank, and, in his later years, his great wealth, he deliberately refrained from any attempts to exercise the slightest social influence. He left no personal records, and few of his letters seem to have been preserved, possibly because few were written. Such as are known relate almost exclusively to matters of science and are otherwise of very slight human interest. All the knowledge of him we possess is based upon the fragmentary notices of a few contemporaries, principally Thomas Young, Thomas Thomson, of Glasgow, Sir Humphry Davy, and Lord Brougham. Their accounts, together with the reminiscences of others who had a certain small measure of personal acquaintance with him, or were able to communicate hearsay information concerning his character, habits and mode of life, have been brought together by the late Dr George Wilson, of Edinburgh, whose *Life of the Hon^ble^ Henry Cavendish*, written at the request of the Cavendish Society, and published in 1851, still remains the only authoritative biography of the philosopher.

The following brief summary of his life and scientific achievements, which it seems desirable to prefix to this collection of his memoirs and papers, is almost wholly based upon that work.

The Honourable Henry Cavendish was born on October 10th, 1731, at Nice, where his mother was residing at that time for the sake of her health. His father, Lord Charles Cavendish, was the fifth son[1] of the second Duke

[1] In Wilson's *Life*, and also in the Introduction to Professor Clerk Maxwell's account of Cavendish's *Electrical Researches* (now vol. 1 of Scientific Papers), it is stated that Lord Charles Cavendish was the *third* son of the second Duke of Devonshire. The Rev. Walter H. Green, Vicar of All Saints' Church, Derby, to whom I am indebted for information concerning Cavendish's place of burial, and the non-existence of any memorial to him in the church, informs me, on the authority of Cox and St John Hope, who wrote a *History of All Saints' Church*, and who give copies of all the inscriptions on the coffin plates in the vault beneath the Devonshire Chapel, that the third son of the second Duke was Lord James Cavendish, who was buried in 1741. Lord Charles Cavendish, who died on April 28th, and was buried in the Devonshire vault on May 7th, 1783, is stated, on his coffin-plate, to have been the *fifth* son of the second Duke.

of Devonshire; his mother, formerly Lady Anne Grey, was the fourth daughter of the Duke of Kent. She died when her son Henry was about two years old, shortly after the birth, in England, of a second son, Frederick.

Very little has been recorded concerning Henry Cavendish's earliest years. When eleven years old he was sent to Dr Newcome's school at Hackney, together with his brother Frederick and other members of the Cavendish family. This seminary is described by Lord Campbell in his *Lives of the Chancellors* as "a most excellent school," and the master as "a sound classical scholar, and a strict disciplinarian," but we have no information concerning the courses of instruction or of the degree of proficiency which Cavendish reached in them. He remained at school until 1749 when he was entered at Peterhouse, Cambridge. No particulars of his life at the University have come down to us. He left in 1753 without taking a degree, it is surmised, because he objected to the tests, which at that time were very stringent. Cavendish, in fact, was not a member of any religious body, and seems at no time to have professed any religious faith and never to have attended a place of worship. His brother Frederick, who came up to Peterhouse in 1751, also left without taking a degree. After leaving Cambridge the brothers would appear to have made a journey together on the Continent, but no particulars of their tour have been recorded beyond the gruesome story of their having seen a corpse in their hotel at Calais, laid out for burial in a room adjoining that which they occupied, to the absolute unconcern of the elder brother.

On leaving the University, Cavendish took up his residence in his father's house in London, where, according to Thomas Thomson, a set of stables were fitted up for his accommodation. It is probable that the stables were simply his laboratory and workshop, for, as his early writings show, it was at about this time that he entered upon the mathematical, chemical and physical studies which led to his investigations and discoveries. There is reason to suppose that his first experimental work was in connection with his father's scientific labours. Among his MSS. papers is a quarto sheet in his handwriting, headed "Table to reduce divisions on nonius of father's thermometer to degrees on new plate"; and this is followed by "Trials of father's th. by father, April 12, 1757." Lord Charles Cavendish had joined the Royal Society in 1727. He paid considerable attention to thermometry, and in 1757, when a Vice-President, contributed to the *Philosophical Transactions* descriptions of two forms of maximum and minimum thermometers for which he received the Copley Medal, or, as it is expressed in the terms of the award, "for his curious invention of making thermometers shewing respectively the greatest degree of heat and cold which have happened at any time during the absence of the observer." There is a paper by him in the Journal-Book of the Royal Society on Canton's experiments on the compressibility of water (*Phil. Trans.* 52, 1765, p. 640). As his son informs us in a paper which will be referred to subsequently, he also made accurate observations of the depression of the mercurial column in barometer tubes of various bores, which first established the relation between the depression and the internal diameter of the tube. Occasional allusions are to be met with in Cavendish's papers to work done in concert with his father, and to instruments which they used in common. These facts serve to throw light upon the relations of father and son. It has been stated that the family were disappointed that Cavendish should have declined to enter public life, and that his father treated him with niggardliness in consequence. There is no real evidence to support this supposition, which would seem to rest mainly on the doubtful authority of Lord Brougham. Cavendish, with his nervous embarrassed manner, his extraordinary shyness, his thin shrill voice, and hesitation of speech, was singularly unfitted for a public career, and it is unlikely that he was ever pressed to embark upon it. It is true that for the first forty years of his life he was a comparatively poor man, and stories have been related of the parsimonious habits he thereby contracted. But Lord Charles Cavendish was not rich, and, according to the botanist, Robert Brown, who had good means of knowing, he allowed his son as much as he could afford; and it is added he fully appreciated his son's abilities and never treated him unkindly. As regards money, he had probably as much as his habits and simple tastes required.

At some period subsequent to 1780 he became wealthy—how is not exactly known, but probably through bequests from relatives. Although he was, as Biot expressed it, "le plus riche de tous les savans et le plus savant de tous les riches," he was singularly indifferent to money and had little interest or concern in spending it. There is a well-known story of his threatening to remove his money if his bankers, who were concerned at the amount lying idle in their hands, continued to bother him about it. He could be liberal at times, almost to the point of extravagance, when some one pressed a worthy object on his notice, but an impulsive generosity was wholly foreign to his disposition, and he made little use of his wealth, which steadily accumulated, until, at his death, he was found to be the largest holder of bank-stock in England and to possess upwards of a million in different public funds—in addition to £50,000 in the hands of his bankers, a freehold estate of £8000 a year besides canal and other personal property.

Cavendish, who was never married, would appear to have resided in his father's house until he was near fifty years of age. Lord Charles Cavendish died in 1783, when, or possibly shortly before, his son moved to a house at Hampstead, and to a town-house close to the British Museum, at the corner of Montague Place and Gower St. We are told that few visitors crossed the threshold of either place, but those who were admitted found that its chief furniture consisted of books and apparatus. But the greater part of his library was contained in a separate house in Dean St., Soho, then a fashionable residential neighbourhood. Here he had brought together a large collection of works on science which he freely allowed all engaged in research to consult, and to which on occasion he himself went, signing a formal receipt for such books as he took away. For a time he lived at 4, Bedford Square.

During his later years he resided on Clapham Common, in a low white building surrounded by a garden. Very little in the house was set apart for personal comfort. What was intended to be the drawing room was converted into a laboratory. A forge stood in an adjoining room. The upper apartments constituted an astronomical observatory. A large registering thermometer of its owner's design (see p. 395) formed a sort of land-mark to the house; on the lawn was a wooden stage affording access to the top of a large tree to which in the course of his meteorological or electrical researches he would occasionally ascend. He lived most abstemiously and seldom saw company. We are told that if anyone dined with him he was invariably treated to a leg of mutton and nothing else. It was said that when on one occasion three or four scientific men were to dine with him the housekeeper remarked that one leg would not suffice. "Then get two," was the reply.

This solitary eventless life came to an end, after a very short illness, on February 24th, 1810. He died unattended, and was buried in All Saints' Church, Derby, in the Devonshire vault, near the splendid tomb which his ancestress, the redoubtable Elizabeth Hardwicke, the founder of his family, had built for herself[1]. No slab or monument of any kind marks the place of his sepulture.

The accounts of Cavendish which have been furnished by his contemporaries are singularly uniform in the impression they convey. They represent him as a man almost morbidly shy, nervous and embarrassed, extremely taciturn and reserved, who hated to be addressed, and who had a horror of a strange face; when he could be induced to talk he spoke in a thin shrill voice, and as if he had difficulty in articulation. He was described as of fair complexion, with small and not marked features, awkward in manner, and walking, with one hand behind his back, with a peculiar slouching gait. His dress was that of the preceding half-century and was never varied—a faded violet suit, frilled shirt-wrists, high coat

[1] Wilson speaks of a "funeral tablet," but according to Professor Clerk Maxwell, on the authority of Mr Cooling, a former Churchwarden of All Saints', there is nothing of the kind in the church. This is confirmed by the present vicar, Rev. Walter H. Green, who, moreover, thinks it doubtful if there ever was one. Several historians of Derby and its churches make mention of Henry Cavendish, but say nothing of any tablet or memorial of him. The vault in which he was buried is now permanently built up. It appears to have been last opened by Cox and St John Hope in 1879 for the purpose of making a full list of all interred there. They carefully copied all the coffin plates, 44 in number. Amongst them is No. XXVIII—that of Henry Cavendish who is stated to have died on February 24th and to have been buried on March 12th, 1810.

collar, knocker tailed periwig and cocked hat. He was a confirmed misogynist. He would never see a female servant, and if an unfortunate maid showed herself she was promptly dismissed. On one occasion he met one of his domestics with a broom and a pail on the stairs, and was so annoyed that he immediately ordered a back staircase to be built. There is, however, a story to the effect that, during one of his solitary walks, he so far overcame his bashfulness as to rescue a lady from the attacks of an infuriated cow—a circumstance which made a great sensation at the time at Clapham, where he was regarded with a certain amount of awe as a woman-hating wizard.

As to his character as it appeared to his biographer Wilson:

He was almost passionless....His brain seems to have been but a calculating machine....His Theory of the Universe seems to have been, that it consisted *solely* of a multitude of objects which could be weighed, numbered and measured; and the vocation to which he considered himself called was, to weigh, number and measure as many of those objects as his allotted three-score years and ten would permit. This conviction biassed all his doings, alike his great scientific enterprises and the petty details of his daily life....Throughout his long life he never transgressed the laws under which he seems to have instinctively acted.... It seems, indeed, to have been impossible for Cavendish to investigate any question otherwise than quantitatively....

Whatever, accordingly, we may think of the ideal which Cavendish set before him, we must acknowledge that he acted up to it with undeviating consistency; and that he realised it to a far greater extent than most men realise the more lofty ideals which they set before them. The pursuit of truth was with him a necessity, not a passion. In all his researches he displayed the greatest caution, not from hesitation or timidity, but from his recognition of the difficulties which attend the investigation of nature; from his delight in reducing everything to numerical rule, and his hatred of error as a transgression of law. *Cavendo tutus* was the motto of his family, and seems ever to have been before him.

On the other hand, Davy, in the course of an eloquent tribute to the character and achievements of Cavendish as a man of science, delivered in the theatre of the Royal Institution, a few weeks after his death, stated with perfect justice,

it ought to be mentioned in estimating the character of Mr Cavendish that his grand stimulus to exertion was evidently the love of truth and of knowledge:—unambitious, unassuming, it was often with difficulty that he was persuaded to bring forward his important discoveries. He disliked notoriety; he was, as it were, fearful of the voice of fame[1]. His labours consequently are recorded with the greatest dignity and simplicity and in the fewest possible words, without

[1] It was said of him that "he was peevishly impatient of the inconveniences of eminence, detested flattery, and was uneasy under merited praise." Brand's Preface to Suppl. to *Encycl. Brit.*

parade or apology; and it seemed as if in publication he was performing not what was a duty to himself, but what was a duty to the public.

Indeed, as regards the style in which his memoirs are put together, Cavendish would seem to have been ever faithful to the injunction laid upon him by the original Statutes of the Royal Society and which remained in force long after he had passed away, viz. that "in all reports of experiments to be brought into the Society, the matter of fact shall be barely stated, without any prefaces, apologies, or rhetorical flourishes."

Cavendish is usually regarded in the personal history of science as a chemist. Although his chemical discoveries are among his greatest scientific achievements, it may be questioned whether he himself would so reckon them. In reality he was a natural philosopher on a very broad gauge. Almost every department of the physical science of his time appealed to him with equal force and he pursued all with equal zeal and success. Nevertheless, compared with some of his contemporaries, the bulk of his published work is not large. Most of his investigations seem to have been made to satisfy his own spirit of inquiry, and he pursued them as opportunity and his mood impelled him. He cared little for the judgement and opinion of his fellows and was wholly indifferent to scientific fame. At no time was he in a hurry to give the results of his labour to the world, and it not seldom happened that these either remained unpublished, or were withheld for years after they were actually obtained. It may be, as many of his drafts would seem to imply, that literary composition was irksome to him. This was unfortunate no less for science than for his own reputation. The earlier publication of the cardinal discovery of the compound nature of water, for example, would, possibly, although by no means certainly, have effected the speedier downfall of phlogistonism; it would at least have spared us what is known in the history of science as the Water Controversy, with its regrettable and unfounded aspersions on Cavendish's scientific character and moral worth.

In his dislike of notoriety, his taciturnity, and disinclination to publish, Cavendish, as in many other respects, affords a striking contrast to his contemporary Priestley. It was characteristic of Priestley to write:

> When, for the sake of a little more reputation, men can keep brooding over a new fact, in the discovery of which they might, possibly, have very little real merit, till they think they can astonish the world with a system as complete as it is new, and give mankind a prodigious idea of their judgment and penetration; they are justly punished for their ingratitude to the fountain of all knowledge, and for their want of a genuine love of science and of mankind, in finding their boasted discoveries anticipated, and the field of honest fame pre-occupied by men, who from a natural ardour of mind, engage in philosophical pursuits, and with an ingenuous simplicity immediately communicate to others whatever occurs to them in their inquiries. (Preface to *Experiments and Observations on Different Kinds of Air*, Second Edition, 1775.)

This, we may be reasonably sure, must have been read by Cavendish, who was on good terms with Priestley and occasionally corresponded with him, but it is no less certain that he was entirely uninfluenced by it.

Cavendish became a Fellow of the Royal Society in 1760 and its meetings, in Crane Court, Fleet St., together with those of the Royal Society Club, which at that period dined at the "Mitre" or the "Crown and Anchor Inn," the Sunday conversaziones at Sir Joseph Banks's residence in Soho Square, and an occasional christening at Devonshire or Burlington House, were the only forms of social relaxation he seems to have allowed himself.

In spite of his shyness and his dislike of publicity, he was an active member of the Society, served on its Council, took a leading part in a number of its Committees, and was a loyal and zealous supporter of Sir Joseph Banks throughout the long and occasionally turbulent reign of that masterful President.

In commenting on Cavendish's work, other than his electrical researches which have been edited, mainly from the unpublished manuscripts in the possession of the Duke of Devonshire, by Professor Clerk Maxwell, and form vol. I of this collection, it may be desirable to deal first with his published chemical memoirs, upon which his fame mainly rests; and then with the rest of his published papers, which are not so conveniently grouped.

His published chemical researches are described in seven papers contributed at intervals between 1766 and 1788 to the *Philosophical Transactions*, where, in fact, all his published work of whatever kind first appeared. The seven papers referred to deal mainly with pneumatic chemistry.

They may be said, in a certain sense, to form a series in so far as they are concerned with a particular class of phenomena, but strictly speaking their subjects are independent.

The first communication, published in 1766, is entitled, "Three Papers, containing Experiments on factitious Air." The title is significant having regard to current doctrine concerning the nature of "air." "By factitious air," he says, "I mean in general any kind of air which is contained in other bodies in an unelastic state, and is produced from thence by art." It was in this sense that the term was first used by Boyle. Cavendish then proceeds to give, by way of introduction, a general account of his methods of manipulating the several varieties of factitious air he describes. His arrangements were not very dissimilar to, nor hardly an improvement upon, those of Stephen Hales, the real father of pneumatic chemistry, with whose work he was, no doubt, familiar. Like his predecessor, he collected the "airs" over water, suspending the collecting vessels by means of strings. The idea of the shelf, which constituted the essential feature of the pneumatic trough contrived by Priestley, the "tub," as it was called in the terminology of the time, may be said to have originated with Dr William Brownrigg and is described in a paper on "An Experimental Enquiry into the Mineral Elastic Spirit, or Air, contained in Spa Water; as well as into the Mephitic Qualities of this Spirit," printed in the *Phil. Trans.* for 1765, p. 218. Brownrigg's shelf, however, was a sort of rack fixed above the level of the water and perforated with holes in which the cylinders used for collecting the "air" were inserted and held in position by wedges. This was certainly a more convenient arrangement than the method of suspending the vessels by strings, adopted by Hales and Cavendish. It was a very simple step to place the shelf below the level of the water, but it was left to Priestley's nimble wit, some five or six years after the publication of Cavendish's paper, to make it. Cavendish, in fact, showed no great amount of ingenuity in the construction of new apparatus or in the modification of that already existing. He introduced no new or permanent contrivance into operative chemistry but was content to make the best use possible to him of such as lay ready to his hand. Whatever alterations he may have devised were directed to making them capable of affording quantitative results. He cared little for their appearance as regards elegance of form: his main concern was that they should be efficient.

The published paper on factitious airs is divided into three parts. Part I consists of a description of experiments on inflammable air from metals (hydrogen). Cavendish says he knew of only three metallic substances, namely zinc, iron and tin, that generate inflammable air on solution in acids; and those only by solution in the diluted vitriolic acid (sulphuric acid) or spirit of salt (hydrochloric acid). He made observations on the comparative rates of solution of the several metals in the different acids, studied the influence of dilution and temperature, and ascertained the volume of "air" evolved. He found that 1 ounce (480 grains) of zinc produced 356 ounce measures of "air," the quantity being "the same which-soever acids of these it is dissolved in." The same quantity of iron wire produced about 412 ounce measures of air; the quantity was "just the same, whether the oil of vitriol was diluted with $1\frac{1}{2}$ or 7 times its weight of water." One ounce of tinfoil by solution in strong spirit of salt yielded 202 ounce measures of inflammable air. The volumes of air were measured "when the thermometer was at 50° and the barometer at 30 inches." These results are in substantial accordance with those demanded by the respective atomic weights of the metals.

He next studied the action of "nitrous acid" (nitric acid) and of undiluted oil of vitriol when heated upon the three metals, but found that the "air" which was generated in each case was "not at all inflammable." He made no further study of the "airs" produced in these cases but evidently considered them as essentially the same as in the former case but "modified" in their nature by the action of the acid used in their production.

In explanation of the difference in the character of the "airs" he says:

It seems likely from hence that when either of the above-mentioned metallic substances are dissolved in spirit of salt, or the diluted vitriolic acid, their phlogiston flies off, without having its nature changed by the acid, and forms the inflammable air; but that, when they are dissolved in the nitrous acid, or united by heat to the vitriolic acid, their phlogiston unites to part of the acid used for their solution, and flies off with it in fumes, the phlogiston losing its inflammable property by the union. The volatile sulphureous fumes, [sulphur dioxide] produced by uniting these metallic substances by heat to the undiluted vitriolic acid, shew plainly, that in this case their phlogiston unites to the acid; for it is well known, that the vitriolic sulphureous acid consists of the plain vitriolic acid united to phlogiston[1].

As to the inflammable air, produced by dissolving these substances in spirit of salt or the diluted vitriolic acid, there is great reason to think that it does not contain any of the acid in its composition; not only because it seems to be just the same whichever of these acids it is produced by; but also because there is an inflammable air, seemingly much of the same kind as this, produced from animal substances in putrefaction, and from vegetable substances in distillation, as will be shown hereafter; though there can be no reason to suppose that this kind of inflammable air owes its production to any acid.

We have here a striking illustration of what has been styled "the prejudice of that epoch" which was "not to regard compound substances as simple, but to consider undecompounded substances as compound." It was no doubt a prejudice, which as Mr Harcourt asserts, "infected the whole of chemistry" at the time, and Cavendish was not insensible to it. But in spite of the special pleading of the learned President of the British Association there can be no question that Cavendish remained fettered by the complexity of the phlogistic theory to the end; he never at any time sat "loose" to the favourite hypothesis: on the contrary he clung to it with as much persevering tenacity as any one of his countrymen.

Cavendish, it will be gathered, imagined that the inflammable air in all cases came from the metals as they were dissolved, and not from the diluted acids. This of course was in conformity with current doctrine. He was at first led to believe that the inflammable air unmodified by the acid was in fact pure phlogiston, but although he afterwards changed this opinion, the identity of phlogiston with hydrogen was, from this time forth, held to be established by one section at least of the Phlogiston School, notably by Priestley, Kirwan and Watt.

Cavendish then proceeded to study the properties of hydrogen. He

[1] Footnote in original paper. "Sulphur is allowed by chymists, to consist of the plain vitriolic acid united to phlogiston. The volatile sulphureous acid appears to consist of the same acid united to a less proportion of phlogiston than what is required to form sulphur. A circumstance which I think shows the truth of this, is that if oil of vitriol be distilled from sulphur, the liquor, which comes over, will be the volatile sulphureous acid."

found that it had no tendency to lose its elasticity by keeping, and that it was not absorbed by water or by fixed or volatile alkalis. Its explosibility when mixed with common air had, he says, been observed by others, and he sought to determine how the effect varies with the proportions in which the "airs" are mixed, by noting the loudness of the sound when a piece of lighted paper is applied to the mouth of the bottle. He offers no opinion as to the real cause or significance of the explosion, nor, inasmuch as the mixture was made over water, is it at all surprising that he failed to notice the appearance of moisture as a result of the combustion of the inflammable air.

He compared the hydrogen obtained by the use of the different metals and by the action of both sulphuric and hydrochloric acids, but was unable to perceive any difference. His general conclusion is that

> it appears from these experiments that this [inflammable] air, like other inflammable substances, cannot burn without the assistance of common air. It seems too, that, unless the mixture contains more common than inflammable air, the common air therein is not sufficient to consume the whole of the inflammable air; whereby part of the inflammable air remains, and burns by means of the common air, which rushes into the bottle after the explosion.

He next attempted to ascertain the relative density of the inflammable air from metals as compared with common air by ascertaining the weight of the same bladder when filled successively with the two "airs." The experiments were repeated with hydrogen from the various sources but no certain difference could be perceived: "the small difference observed in these trials is in all probability less than what may arise from the unavoidable errors of the experiment." The average of the trials showed that 80 ounce measures of inflammable air weigh 41 grains less than an equal bulk of common air. On the assumption that water is 800 times denser than common air, this air would be seven times heavier than inflammable air: if Hauksbee's value of 850 be adopted, then common air will be eleven times heavier than hydrogen. Both values are, of course, erroneous, hydrogen being more than fourteen times lighter than air. Cavendish's method was faulty in principle. He was aware of certain sources of error due to leakage and admixture with common air and the presence of water-vapour. He made an attempt to estimate the influence of water-vapour by passing a known volume of the gas over dried pearl-ashes, and noting the increase in weight of the drying material. To check his results he made another series by a second method based on a different principle. He sought to determine the weight of hydrogen evolved on the solution of a known weight of metal, taking the precaution to pass the hydrogen over dry pearl-ash which he found by direct experiment not to absorb any sensible quantity of inflammable air. Knowing from his previous experiments the volume of hydrogen at a known temperature and pressure produced by the solution of a known weight of the metal he was in a position to calculate the specific gravity of hydrogen as compared with water and thence with common air.

The experiments, as in the first series, were made with hydrogen produced by the action of sulphuric and hydrochloric acids upon the different metals but, as in that case, no certain difference could be ascertained. "By a medium of the experiments, inflammable air comes out 8760 times lighter than water, or eleven times lighter than common air."

Cavendish concludes this section of his paper by an account of the result of an attempt to ascertain if any inflammable air could be obtained from copper by solution in spirit of salt. He could not obtain it by this means but the phenomena seemed "remarkable enough to deserve mentioning." In the cold there was no action;

> but, with the assistance of a heat almost sufficient to make the acid boil, it made a considerable effervescence,...when the water [from the pneumatic trough] rushed violently through the bent tube...and filled it almost intirely full.

He then varied the experiment so that the end of the tube of the generating vessel was exposed to the air.

> As soon as the effervescence began, the vapours issued visibly out of the bent tube; but they were not at all inflammable, as appeared by applying a piece of lighted paper to the end of the tube. A small empty phial was then inverted over the end of the bent tube, so that the mouth of the phial was immersed in the water, the end of the tube being within the body of the phial and out of the water. The common air was by degrees expelled out of the phial, and its room occupied by the vapours; after which, having chanced to shake the inverted phial a little, the water suddenly rushed in and filled it almost full; from thence it passed through the bent tube into the bottle and filled it quite full.

Had Cavendish followed up this observation he would in all probability have discovered the nature of the "elastic fluid which," as he says, "immediately loses its elasticity, as soon as it comes in contact with the water." As it was, the discovery of the "Marine Acid Air" was left to Priestley, who in 1772, on repeating Cavendish's experiment, found that the copper was not really concerned in the generation of the "air" which came from the acid when heated.

The second part of the paper is entitled: *Containing Experiments on Fixed Air, or that Species of Factitious Air, which is produced from Alkaline Substances, by Solution in Acids or by Calcination.*

The particular form of the title calls for a few words of comment. The term Fixed Air although used prior to Black to denote air which entered into the composition of substances and became thereby latitant was restricted by Black to the gas we now term carbon dioxide. Strictly speaking it was of course equally applicable to any form of "factitious air," as this was defined by Boyle and Cavendish. The fact that it was not sufficiently descriptive was perfectly obvious to Cavendish, as he implies in the opening paragraph of his paper. But inasmuch as the term was current in the chemical literature of the period, he uses it in the sense to which it was first restricted by Black in or about 1754 or 1755. In his famous paper *Experiments upon Magnesia Alba* Black gives no detailed account of the properties of Fixed Air in the free state; his main object was to show its influence in determining the "mildness" of alkalis. What he actually stated was that

> quicklime...is capable of being joined to one particular species [of air] only, which is dispersed through the atmosphere, either in the shape of an exceedingly subtle powder, or more probably in that of an elastic fluid. To this I have given the name of *fixed air*, and perhaps very improperly; but I thought it better to use a word [air] already familiar in philosophy, than to invent a new name, before we be fully acquainted with the nature and properties of this substance, which will probably be the subject of my further inquiry.

The results of this further inquiry, if made, were never published by him.

Black was clearly conscious that his fixed air was distinct from common air and, as stated, may possibly have illustrated the distinction in his lectures at Glasgow and Edinburgh, but no published account of these appeared until some years after the date of Cavendish's paper.

Cavendish prepared the fixed air of Black by the action of spirit of salt upon marble, collecting the gas in the usual way over the pneumatic trough. He found, as had been already observed by Macbride, as indeed he remarks, that the air was absorbed by water and that the solution precipitated "the earth from lime-water; a sure sign that it had absorbed fixed air." He noticed that it could be preserved, apparently indefinitely, over mercury. By collecting a known volume of the gas over the mercurial pneumatic trough in a graduated cylinder in which was introduced a known volume of water "well purged of air by boiling," it was found that water, when the thermometer is about 55° F., will absorb rather more than an equal bulk of the gas, more being absorbed in cold weather than in warm. "Water heated to the boiling point is so far from absorbing air that it parts with what it has already absorbed." After being so heated it was found, on cooling, "not to make any precipitate, or to become in the least cloudy on mixing it with lime water." Exposed to the open air in a saucer for a few days the solution also parts with the fixed air, as the lime-water no longer renders it cloudy. Spirits of wine at the heat of 46° F. absorbs near 2¼ times its bulk of fixed air, and olive oil rather more than an equal bulk. Cavendish was disposed to think, however, that "fixed air contained in marble consists of substances of different natures, part of it being more soluble in water than the rest," an opinion from which Black dissented, but which, curiously enough, has recently been revived in a modified form in Russia.

A determination by Cavendish of its specific gravity by means of a bladder showed that fixed air is 1·57 times heavier than common air—a fair approximation to the truth. "Fixed air has no power of keeping fire alive, as common air has; but, on the contrary, that property of common air is very much diminished by the mixture of a small quantity of fixed air." Air containing about one-ninth of its bulk of fixed air will not support the flame of a candle.

He next made a series of determinations of the amount of fixed air in various "alcaline substances" by observing the loss of weight which known amounts suffered by solution in hydrochloric or sulphuric acid. The apparatus was identical with that employed in ascertaining the weight of hydrogen evolved by the action of acids on metals, except that the drying tube "was filled with shreds of filtering paper instead of dry pearl ashes; for pearl ashes would have absorbed the fixed air that passed through them." The substances examined were marble, volatile sal ammoniac (ammonium carbonate), pearl ashes, and acid potassium carbonate which Cavendish prepared by exposing "salt of tartar" (potassium carbonate) to an atmosphere of carbonic acid. The quantitative results are only rough approximations to the truth, but they are historically interesting as being the first fairly successful attempts to analyse carbonates.

Part III contains the results of "Experiments on the Air produced by Fermentation and Putrefaction."

Macbride had shown that vegetable and animal substances yield fixed air by fermentation and putrefaction. Cavendish found that this was the case in the alcoholic fermentation of sugar and apple-juice, and that the fixed air formed was identical with that produced from marble. Nor was the nature of common air affected by the fermenting liquid, since

> a small phial being filled with one part of this air and two of inflammable air; the mixture went off with a bounce, on applying a piece of lighted paper to the mouth, with exactly the same appearances, as far I could perceive, as when the phial was filled with the same quantities of common and inflammable air.

The "air" produced from gravy broth and from raw meat by putrefaction was found to consist of a mixture of fixed air and inflammable air, which could be separated by soap-leys. He determined the explosibility of a mixture of this inflammable air and common air: "it went off with a gentle bounce on applying the lighted paper; but I think not so loud as when the phial was filled with the last mentioned quantities of air from zinc and common air." A determination of its specific gravity showed "that this factitious air should seem to be rather heavier than air from zinc; but the quantity tried was too small to afford any great degree of certainty." The conclusion was that "on the whole it seems that this sort of inflammable air is nearly of the same kind as that produced from metals. It should seem however, either to be not exactly the same, or else to be mixed with some air heavier than it, and which has in some degree the property of extinguishing flame, like fixed air."

It would appear that this paper was originally intended to consist of four parts. Part IV, although completed and ready for press, for some reason was withdrawn. It has already been made public by the Rev. William Harcourt in the Postscript to his Presidential Address to the British Association in 1839[1]. It is to be found among the MSS. preserved at Chatsworth and will be reprinted in this volume and referred to subsequently.

The papers by Cavendish on "Factitious Airs" and that of Black, published a dozen years previously, together constitute an epoch in the history of chemistry. The early alchemists appear to have had some slight knowledge of an inflammable air produced by the action of acids upon metals. Van Helmont, to whom we owe the term *gas*, was aware of the existence of various forms of "air," as were Boyle and Mayow. That hydrogen was combustible was clearly recognised by Turquet de Mayerne in the seventeenth century and Lémery in 1700 noticed the detonating property of a mixture of this gas with common air. That an inflammable air was occasionally present in coal-workings was also known, and a distinction was drawn between fulminating- or fire-damp and choke-damp. Stephen Hales in his *Vegetable Staticks* (1727) and in his *Hæmastaticks* (1732) had shown that "airs" of different characters might be obtained by a variety of operations. But, like his predecessors, he attached no very definite importance to the diversities of colour, smell, solubility in water, inflammability, etc., which he noticed; and his imperfect attempts to determine the relative densities of the various airs only served to confirm him in his belief that they were essentially identical, but "infected" or "tainted" with extraneous "fumes," "vapours" and "sulphurous spirits." Hales's Essays had a considerable vogue in their day but whilst they may be said in one sense to have laid the foundations of pneumatic chemistry, in another they probably retarded its development, by serving to strengthen the belief, as a relic of mediæval scholasticism, in one universal and elementary air. It was only after the publication of Cavendish's paper when he conclusively established that the constant differences in the characters of "airs," to some of which he was able to assign quantitative values, were the real indications of their individuality, that Hales's Essays acquired their full importance. The properties which Hales noted, but of which he failed to perceive the significance, were then recognised as possible clues to the existence of a variety of elastic fluids differing essentially from common air.

This, then, is the great merit of Cavendish's paper: it gave a final and decisive blow to the conception of a universal air, elementary and primordial. That its true significance was everywhere clearly perceived is abun-

[1] *Report of the Ninth Meeting of the British Association for the Advancement of Science*, London, 1840, p. 58.

dantly proved by the literature of the period. The Royal Society showed their appreciation of its merit by awarding its author a Copley Medal. It gave a great impetus to the study of pneumatic chemistry and no doubt influenced Priestley in choosing this particular branch of chemical inquiry for his first essays in original investigation, thereby leading to a great extension of our knowledge of gases, and eventually to a complete revolution in chemical doctrine.

It seems to have been Cavendish's practice to keep several investigations running concurrently, although these were not invariably experimental, or concerned with a single department of science. During the time that he was engaged on his inquiry into "factitious airs" he was also occupied with an examination of certain natural waters, made primarily with a view of throwing light upon the cause of the "suspension [solution] of the calcarious earth which is separated from them by boiling." The results of this inquiry were read to the Royal Society on February 19th, 1767, and are published in the *Phil. Trans.* for that year.

Dr Brownrigg in a paper entitled "An Experimental Enquiry into the Mineral Elastic Spirit, or Air, contained in Spa Water; as well as into the Mephitic Qualities of this Spirit," published in the *Phil. Trans.* for 1765, had already thrown some light upon this matter and had shown that this "spirit was" identical with that

> most subtile and active exhalation, which, in many places perspires from springs and lakes, and other openings of the earth; or arises in pits and mines, where it is discovered by extinguishing flame; and from its pernicious effects, in killing all animals that breathe therein, is known to our miners by the name of choak-damp.

Shaw had previously collected "air" from Scarborough water, as also had Home from the chalybeate water of Dunse, in Scotland, but they appear to have regarded it as identical with common air. Brownrigg drew attention to the fact "that in proportion as this mineral air is separated by heat in the same proportion the more gross earthy parts of the water seem also to separate from it," but he offers no sufficient explanation of the phenomenon. The examination of the Rathbone Place water by Cavendish resulted in clearing up the matter. At that period most districts in London were supplied by wells or springs; the water of Rathbone Place was formerly raised by an engine and served the immediate neighbourhood.

Cavendish incidentally made an attempt to gain some information concerning the amount and nature of the saline ingredients in the water which he found to contain 17·5 grains of soluble matter to the pint, of which about half was observed to consist of calcium carbonate and a little magnesium carbonate, the rest being sodium chloride with small quantities of the sulphates of lime and magnesia. An unsuccessful search was made for nitrates, which were found in other London well waters, but a notable amount of "volatile alcali" was detected, which seems to reflect somewhat upon the purity of the supply. The details of the inquiry are interesting as an early example of water-analysis. The examination of the gaseous contents of the water appears to have chiefly interested Cavendish, and the greater part of the paper is devoted to a description of the method of procuring and analysing them. He found that the greater quantity was fixed air, and that the rest was common air, which he tested by his explosion method with hydrogen, and by determining its specific gravity. With characteristic caution he satisfied himself that the fixed air was not generated by boiling, but that it pre-existed in the water. As regards the effect of fixed air in determining the solubility of the earthy carbonates his experiments led him to conclude that:

It seems likely from hence, that the suspension [solution] of the earth in the Rathbone place water is owing merely to its being united to more than its natural proportion of fixed air; as we have shown that this earth is actually united to more than double its natural proportion of fixed air, and also that it is immediately precipitated, either by driving off the superfluous fixed air by heat, or absorbing it by the addition of a proper quantity of lime-water.

The experiments, in fact, plainly foreshadow the lime-process of water-softening. Cavendish comments upon the paradoxical fact that calcareous earths should be

rendered soluble in water, by furnishing them with more than their natural proportion of fixed air, i.e. that they should be rendered soluble, both by depriving them of their fixed air, and by furnishing them with more than their natural quantity of it,

and he multiplies experiments in proof of it.

Cavendish's third chemical memoir was not published until 1783 when he communicated to the Royal Society a paper on "A New Eudiometer" printed in the *Phil. Trans.* for that year.

During the years that followed the publication of his paper on the Rathbone Place water he was occupied with other than chemical work, notably with the electrical researches which form the subject of the volume to which this is a companion, as well as with other inquiries which will be referred to later.

The seventeen years which elapsed between the publication of his second and third chemical memoirs were singularly fruitful in discoveries in pneumatic chemistry, thanks more especially to the labours of Priestley and Scheele. The existence of a large number of gaseous substances, some of which, it was subsequently found, had actually been prepared, but not clearly recognised by Hales, was now definitely established. Owing, however, to the limited means of analytical chemistry at this period the composition and true relationships of these gases were very imperfectly understood. The phenomena they exhibited were explained, whenever possible, in terms of the phlogistic hypothesis, and the explanation, so far from elucidating the facts, frequently wholly obscured them. Until late in the eighteenth century the various "airs" were still roughly grouped as *fixed*, *mephitic* and *inflammable* although more than one observer had pointed out the inadequacy of such a classification, many "airs" being both inflammable and mephitic, whilst the term "fixed" was, strictly speaking, applicable to all forms of "factitious air."

Even after the existence of a variety of "airs" had been made known, it was years before the fact that they were individual and independent substances and not necessarily co-related was clearly and definitely recognised—so ingrained was the general belief of the schoolmen in "air" as an elementary and primordial substance. The specific differences in mode of origin and characters of the various "inflammable airs" were, it might be thought, sufficient to indicate that, as Keir wrote to Priestley so late as 1790, "there is not one inflammable but many inflammables, which opinion you now think as heterodox as the Athanasian system." Priestley's inability to recognise the difference between carbonic oxide and hydrogen led Watt to conclusions based upon wholly fallacious data, whilst strengthening his own belief in the invalidity of Cavendish's interpretation of the experiments which established the compound nature of water and the nature and proportion of its constituents.

That common air was a mixture of two dissimilar "airs," one only of which was concerned with the phenomena of respiration and combustion, had been recognised, more or less clearly, since the time of Mayow, but by the followers of Stahl it was surmised that the essential distinction between the two airs consisted in the presence or absence in them of phlogiston—the respirable and fire-air being devoid of phlogiston whereas the other air—the mephitic air in the atmosphere (nitrogen)—was saturated with it. Combustion, decay and putrefaction, the breathing of animals, and other processes, resulting as we now recognise in the abstraction of oxygen, were considered as phlogisticating the air, and the relative goodness or badness of common air was for a time considered to depend upon the degree of its phlogistication. Accordingly the efforts of chemists were directed to finding a method whereby the extent to which common air was phlogisticated by the various vitiating processes to which it was subjected could be, if possible, quantitatively ascertained. It was imagined that in this manner a definite numerical value could be associated with the relative salubrity of air in different localities and in different climates. Priestley's discovery that *nitrous air* (nitric oxide) when brought in contact with common air in a vessel standing over water, combined with that portion devoid of phlogiston, the product dissolving in water, seemed to afford the basis of a method for determining the amount of air not already phlogisticated—in other words, a method for ascertaining the relative goodness of air.

A considerable number of arrangements based upon this principle were accordingly devised by chemists, notably by Priestley, Fontana, Magellan, Dobson, and Landriani, the last-named coining the word *Eudiometer* by which they were commonly designated. The chemical theory of the method was, of course, unknown, as was the fact that nitric oxide and oxygen are capable of uniting in different proportions by volume, according to circumstances. It was quickly recognised however that concordant and comparable results could only be obtained by operating in a uniform manner, and the differences in the various methods suggested from time to time consisted for the most part in the mode in which uniformity of procedure was thought to be secured.

Among these experimenters was Cavendish, who, in 1781, entered upon a critical inquiry at his laboratory in Great Marlborough St., on the working of the nitric oxide eudiometer, and made considerable modifications in the apparatus with a view of obviating certain sources of irregularity which he noticed in the course of his trials. These he sets out at length in his paper. His eudiometer suffered in portability, ease and rapidity of working when compared with other eudiometers, such as Priestley's, but it was far more capable of giving constant results, and in his hands effected what was nothing less than a revolution in current doctrine concerning the constitution and functions of the atmosphere. He conclusively established that common air so far from being widely variable in character, as might reasonably be supposed from the numerous vitiating influences to which it was exposed, was, in reality, remarkably uniform.

"During the last half of the year 1781," he says, "I tried the air of near 60 different days, in order to find whether it was sensibly more phlogisticated at one time than another; but found no difference that I could be sure of, though the wind and weather on those days were very various; some of them being very fair and clear, others very wet, and others very foggy.... I made some experiments also to try whether the air was sensibly more dephlogisticated at one time of the day than another, but could not find any difference. I also made several trials with a view to examine whether there was any difference between the air of London and the country, by filling bottles with air on the same day, and nearly at the same hour, at Marlborough-Street, and at Kensington. The result was, that sometimes the air of London appeared rather the purest, and sometimes that of Kensington; but the difference was never more than might proceed from the error of the experiment; and by taking a mean of all, there did not appear to be any difference between them. The number of days compared was 20, and a great part of them taken in winter, when there are a greater number of fires, and on days when there was very little wind to blow away the smoke."

Having thus proved that there existed "very little difference in the purity of common air at different times and places," Cavendish proceeded to point out that

it is very much to be wished, that those gentlemen who make experiments on factitious airs, and have occasion to ascertain their purity [that is their degree of phlogistication] by the nitrous test would reduce their observations to one common scale, as the different instruments employed for that purpose differ so much, that at present it is almost impossible to compare the observations of one person with those of another.

Accordingly he suggested that a common scale applicable to all nitric oxide eudiometers might be made "by assuming common air and perfectly phlogisticated air as fixed points." By perfectly phlogisticated air Cavendish implicitly means nitrogen, as shown by the methods he subsequently describes for obtaining it. On this scale perfectly phlogisticated air was zero and pure dephlogisticated air (oxygen) was 4·8.

If the test of any air [factitious air] be found to be the same as that of a mixture of equal parts of common and phlogisticated air, I would say, that it was half as good as common air; or, for shortness, I would say, that its standard was $\frac{1}{2}$: and, in general, if its test was the same as that of a mixture of one part of common air and x of phlogisticated air, I would say, that its standard was $\frac{1}{1+x}$. In like manner, if one part of this air would bear being mixed with x of phlogisticated air in order to make its test the same as that of common air, I would say, that it was $1 + x$ times as good as common air, or that its standard was $1 + x$; consequently, if common air, as Mr Scheele and La Voisier suppose, consist of a mixture of dephlogisticated and phlogisticated air, the standard of any air is in proportion to the quantity of pure dephlogisticated air in it. In order to find what test on the Eudiometer answers to different standards below that of common air, all which is wanted is, to mix common and perfectly phlogisticated air in different proportions, and to take the test of those mixtures; but in standards above that of common air, it is necessary to procure some good dephlogisticated air, and to find its standard by trying what proportion of phlogisticated air it must be mixed with, in order to have the same test as common air, and then to mix this dephlogisticated air with different proportions of phlogisticated air, and find the test of those mixtures[1].

These extracts are of importance for several reasons. In the first place they show that the eudiometer was an instrument designed to determine the degree of phlogistication of "air," whether common or factitious. In the next they serve to indicate what was in the mind of Cavendish con-

[1] The rule for computing the standard of any mixture of dephlogisticated and phlogisticated air is as follows. Suppose the test of a mixture of D parts of dephlogisticated air and P of phlogisticated air, is the same as that of common air, then is the standard of the dephlogisticated air $\frac{D+P}{D}$. Let now δ parts of this dephlogisticated air be mixed with ϕ parts of phlogisticated air, the standard of the mixture will be $\frac{D+P}{D} \times \frac{\delta}{\delta+\phi}$.

2—2

cerning the essential nature and constitution of the various factitious airs which had been prepared prior to 1783. They were fairly numerous and very different in characteristics. Priestley had discovered nitric oxide in 1772; ammonia, hydrogen chloride and oxygen in 1774; sulphur dioxide and silicon tetrafluoride in 1775 and nitrous oxide in 1776. The existence of some of them might indeed have been inferred from the work of some of Priestley's predecessors, particularly Stephen Hales. But to Cavendish as also to Priestley, the shadow of phlogiston lay over them all. With the exception of oxygen which seemed to be wholly devoid of that obscure and protean principle, they were all more or less "infected" with phlogiston and their properties were imagined to depend upon the degree of phlogistication, although such attempts as were made to explain them in terms of this inconceptible and elusive entity were occasionally felt to be more ingenious than satisfactory. In seeking to get at Cavendish's views concerning gaseous phenomena it must not be forgotten that he was never able to shake himself free from the trammels of phlogistonism. Its conceptions dominated and coloured all his attempts to arrive at the true meaning of the facts he observed. As we shall see, they largely obscured his recognition of the full significance of his great discovery of the compound nature of water. His guarded reference to the views of Scheele and Lavoisier concerning the composition of common air does not imply that he shared them. Nowhere in the paper on the new eudiometer does he commit himself to a definite statement of belief that common air is a mixture of essentially different elastic fluids. His statements as regards common air invariably refer to its "degree of phlogistication," that is, extent of vitiation; or conversely, "degree of purity," which only implicitly and incidentally meant amount of oxygen.

Cavendish concludes his paper with some remarks as to the limited value of the nitric oxide eudiometer in affording information concerning the salubrity or extent of vitiation of common air.

Where the impurities mixed with the air have any considerable smell, our sense of smelling may be able to discover them, though the quantity is vastly too small to phlogisticate the air in such a degree as to be perceived by the nitrous test, even though those impurities impart their phlogiston to the air very freely. For instance, the great and instantaneous power of nitrous air in phlogisticating common air is well known; and yet ten ounce measures of nitrous air, mixed with the air of a room upwards of twelve feet each way, is sufficient to communicate a strong smell to it, though its effect in phlogisticating the air must be utterly insensible to the nicest Eudiometer; for that quantity of nitrous air is not more than 140,000th part of the air of the room, and therefore can hardly alter its test by more than $\frac{3}{140000}$ or $\frac{1}{47000}$ part.... In like manner it is certain, that putrefying animal and vegetable substances, paint mixed with oil, and flowers, have a great tendency to phlogisticate the air; and yet it has been found, that the air of an house of office, of a fresh painted room, and of a room in which such a number of flowers were kept as to be very disagreeable to many persons, was not sensibly more phlogisticated than common air. There is no reason to suppose from these instances, either that these substances have not much tendency to phlogisticate the air, or that nitrous air is not a true test of its phlogistication, as both these points have been sufficiently proved by experiment; it only shews, that our sense of smelling can, in many cases, perceive infinitely smaller alterations in the purity of the air than can be perceived by the nitrous test, and that in most rooms the air is so frequently changed, that a considerable quantity of phlogisticating materials may be kept in them without sensibly impairing the air. But it must be observed, that the nitrous test shews the degree of phlogistication of air, and that only; whereas our sense of smelling cannot be considered as any test of its phlogistication, as there are many ways of phlogisticating air without imparting much smell to it; and I believe there are many strong smelling substances which do not sensibly phlogisticate it.

In spite of its limitations, and its imperfect theory, the paper on the New Eudiometer is a very notable contribution to the history of our knowledge of the atmosphere. It clearly established, for the first time, that common air was sensibly uniform in its character. Cavendish would have expressed this by saying that the extent to which it was phlogisticated was practically constant, and independent of locality or meteorological conditions: Scheele and Lavoisier would regard it as proving that the relative proportions of the dephlogisticated and phlogisticated air were invariable and this view gradually gained acceptance. It served to sweep away all the attempts which had been made by eudiometrical tourists to establish, in the words of Landriani, that the air of all those "places which from the long experience of the inhabitants had been reputed unwholesome," could be shown to be so by the instrument. Etymologically the name had no longer any significance as there were no degrees of goodness to be measured. Curiously enough it survives in our literature as the only remnant of the terminology of phlogistonism.

As might be expected from his halting views concerning the real nature of atmospheric air, Cavendish made no attempts to determine the relative volume of that portion of the air which he found to require a constant degree of phlogistication, which seems to show what little importance he attached to the views of Scheele and Lavoisier. Nevertheless the data he furnishes, as demonstrated by Wilson, would have enabled him by the formula he gives in connection with his method of graduation to obtain the relative proportion of the dephlogisticated air (oxygen) and phlogisticated air (nitrogen).

Taking 100 volumes of air, $D + P = 100$. By Cavendish's formula $\frac{D+P}{D} = 4{\cdot}8$; substituting the value of $D + P$;

$$\frac{100}{D} = 4{\cdot}8 \text{ and } D = \frac{100}{4{\cdot}8} = 20{\cdot}83.$$

Therefore 100 volumes of common air consist, on Cavendish's showing, of

Dephlogisticated air (oxygen)	20·83
Phlogisticated air (nitrogen)	79·17
	100·00

which is remarkably close to the truth, and a striking proof of his care and manipulative skill.

In a small packet of papers among the Chatsworth MSS. which had been previously examined by Dr George Wilson and labelled, "This parcel contains various interesting tables of the analysis of air, in 1780–1781, connected with the eudiometrical researches of that period," forming the analytical material for Cavendish's memoir on "The New Eudiometer," is a quarto sheet, the significance of which would appear to have escaped his biographer's notice. On it is written in Cavendish's handwriting:

Air taken by Dr Jeffries: tried Dec. 3, 1784

		1st trials		
No. 2	....	1·042	2nd tr.	1·037
No. 1	....	1·05	2d tr.	1·049
No. 5	3rd trial 1·05	1·041	2nd trial	1·05
No. 6	....	1·052	2d tr.	1·052
No. 3	....	1·05	2d tr.	1·052

Air taken at Hampstead at the time of the trial Dec. 3, 1784

....	1·067	2 tr.	1·068

On November 30th, 1784, Jean Pierre Blanchard, a native of Le Petit Andelys, on the Seine, and one of the most successful of the earlier aeronauts, made a balloon ascent in the neighbourhood of London, accompanied by Dr J. Jeffries, an American physician, who subsequently, also with Blanchard, made the first Channel crossing by balloon from Dover to Calais[1]. There can be little doubt that Cavendish had made arrangements with Jeffries to collect samples of air at various heights during the ascent of November 30th, 1784. The usual method of procuring air for analysis at that period was to empty stoppered bottles filled with water at the spot at which the air was to be collected, and the Nos. 1–6 evidently refer to the samples so taken. The "trials" (analyses) were made by "the new Eudiometer" three days after the ascent, and the results compared with the air "taken out at Mr Cavendish's S. window at the same time." Nothing is said concerning the heights at which the several collections were made, but it will be observed that the "trials" proved that the samples were fairly uniform in composition and showed little variation from that at the ground-level.

These are the first analyses of the upper air of which there is any record. Cavendish thus anticipated Gay Lussac by about twenty years.

[1] *Encycl. Brit.*, 9th edit., art. Aeronautics, p. 191.

Incidentally, it may be remarked, Cavendish showed great interest in aeronautics. Among his papers are observations on the track of Blanchard's balloon in an ascent prior to that on which the samples of air were taken, with letters from a correspondent in Paris who supplied him with information concerning Montgolfier's aerostatic machines.

In the year following the publication of his memoir on "A New Eudiometer," Cavendish communicated to the Royal Society a paper entitled "Experiments on Air," which is printed in the *Phil. Trans.* for 1784. It is concerned with an inquiry which occupied him during 1781, of which the paper on the eudiometer is to be looked upon as a side issue. When regard is had to its contents the title of the new paper is not without significance as a further exemplification of Cavendish's views as to the essential nature of "air." Although its chief importance consists in the fact that it afforded the first clear and incontestable proof of the compound nature of water, and of the nature and relative proportion of its constituents, this discovery was an unlooked-for incident in the inquiry and not its primary object. This object was, in Cavendish's words, "principally with a view to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed." Its relation to the paper immediately preceding it thus becomes apparent. The lack of accurate means of measuring the diminution by phlogistication of common air led him to make a critical examination of the nitric oxide eudiometer, with a view to remedying its acknowledged defects, and as a necessary preliminary to the general inquiry.

Cavendish begins his paper by recalling all the various modes of diminution of common air by phlogistication known to him. It had been surmised that fixed air was either generated or separated from atmospheric air by phlogistication and his first experiments were made in order to ascertain if such were the case. He rejects all experiments with animal and vegetable substances as affording no certain proof of the origin of the fixed air. "The only methods I know," he says, "which are not liable to objection are by the calcination of metals, the burning of sulphur or phosphorus, the mixture of nitrous air, and the explosion of inflammable air." Experiments by others had seemed to show that the passage of electric sparks through common air produced fixed air, but he thinks the evidence inconclusive, owing to the conditions under which the trials were made.

With regard to the four unobjectionable methods he finds no reason to think that the calcination of metals, although phlogisticating common air, produces any fixed air; nor is it produced by the burning of sulphur or phosphorus. The allegation that it is formed by mixing nitrous air (nitric oxide) with common air he disproves by taking care to free the common air from any fixed air originally present, and the nitrous air from any fixed air in or derived from the calcareous earth (calcium carbonate) dissolved in the water of the trough over which it was collected, by passing the gases before admixture through lime-water. Nor was any fixed air produced by the explosion of the inflammable air obtained from metals, with either common or dephlogisticated air when all the "airs" had been previously washed with lime-water.

On the whole, though it is not improbable that fixed air may be generated in some chymical processes, yet it seems certain that it is not the general effect of phlogisticating air, and that the diminution of common air is by no means owing to the generation or separation of fixed air from it.

Although the relevance of this conclusion may not be very apparent to-day, nevertheless, in view of chemical opinion at the time, and especially of the speculations of Kirwan, whose theoretical opinions exercised a certain amount of influence at that period, altogether disproportionate to their intrinsic merit, it was a distinct step in advance and, although not without controversy, eventually settled an important point concerning the origin of fixed air and its relations to air in general. Kirwan, who was a specious but fallacious reasoner, made an attempt to substantiate his position; this, contrary to his practice of avoiding polemics, provoked a reply from Cavendish and, of course, a rejoinder from Kirwan, but time has sided with Cavendish as to the merits of the controversy which is now forgotten. Some experiments by Priestley appeared to show that dephlogisticated air (oxygen) could be obtained from nitrous (nitric) acid and from vitriolic (sulphuric) acid. Cavendish therefore "tried whether the dephlogisticated part of common air might not, by phlogistication, be changed into nitrous or vitriolic acid" by burning sulphur over lime-water, and by the action of liver of sulphur on phlogisticated common air. In neither case was any nitrous salt (nitre) obtained, but in addition to ordinary selenite there was obtained what Cavendish regarded as a form of selenite which was "very soluble, and even crystallised readily, and was intensely bitter," but which by repeated solution and evaporation was gradually changed into ordinary selenite. Concerning this phenomenon Cavendish makes the following observation:

The nature of the neutral salts made with the phlogisticated vitriolic [sulphites] and phlogisticated nitrous acid [nitrites] has not been much examined by the chemists, though it seems well worth their attention;...Nitre formed with the phlogisticated nitrous acid [potassium nitrite] has been found to differ considerably from common nitre, as well as sal polychrest [normal potassium sulphate] from vitriolated tartar [acid potassium sulphate].

Cavendish in fact had prepared calcium sulphite and thiosulphate, the properties of which correspond with the description he gives. The term phlogisticated vitriolic acid, as applied to sulphurous acid, was of course in conformity with the terminology of the phlogiston school, inasmuch as this substance could be prepared by heating oil of vitriol with charcoal or sulphur, both of which conceivably imparted their phlogiston, in which they were rich, being so combustible, to the oil of vitriol.

The same results were obtained when pure dephlogisticated air was substituted for common air.

No vitriolic acid could be detected in the solution when common air was phlogisticated by nitrous air over water. Only nitre, with no traces of vitriolated tartar, could be obtained when the solution was "exactly saturated with salt of tartar [potassium carbonate] and evaporated."

"Having now mentioned the unsuccessful attempts I made to find out what becomes of the air lost by phlogistication, I proceed to some experiments, which serve really to explain the matter." In Dr Priestley's last volume of experiments[1] is related an

experiment of Mr Warltire's, in which it is said that, on firing a mixture of common and inflammable air by electricity, in a close copper vessel holding about three pints, a loss of weight was always perceived, on an average about two grains, though the vessel was stopped in such a manner that no air could escape by the explosion. It is also related, that on repeating the experiment in glass vessels, the inside of the glass, though clean and dry before, immediately became dewy; which confirmed an opinion he [Priestley] had long entertained, that common air deposits its moisture by phlogistication. As the latter experiment seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely. The first experiment also, if there was no mistake in it, would be very extraordinary and curious; but it did not succeed with me; for though the vessel I used held more than Mr Warltire's, namely, 24,000 grains of water, and though the experiment was repeated several times with different proportions of common and inflammable air, I could never perceive a loss of weight of more than one-fifth of a grain, and commonly none at all. It must be observed however, that though there were some of the experiments in which it seemed to diminish a little in weight, there were none in which it increased. In all the experiments, the inside of the glass globe became dewy, as observed by Mr Warltire; but not the least sooty matter could be perceived. Care was taken in all of them to find how much the air was diminished by the explosion, and to observe its test. The result is as follows: the bulk of the inflammable air being expressed in decimals of the common air.

Common air	Inflammable air	Diminution	Air remaining after the explosion	Test of this air in first method	Standard
1	1·241	·686	1·555	·055	·0
	1·055	·642	1·413	·063	·0
	·706	·647	1·059	·066	·0
	·423	·612	·811	·097	·03
	·331	·476	·855	·339	·27
	·206	·294	·912	·648	·58

[1] *Experiments and Observations on Different Kinds of Air.* By Joseph Priestley, LL.D., F.R.S. London: Printed for J. Johnson, No. 72, in St Paul's Church-Yard.

In these experiments the inflammable air was procured from zinc, as it was in all my experiments, except where otherwise expressed; but I made two more experiments, to try whether there was any difference between the air from zinc and that from iron, the quantity of inflammable air being the same in both, namely, 0·331 of the common; but I could not find any difference to be depended on between the two kinds of air, either in the diminution which they suffered by the explosion, or the test of the burnt air [i.e. that remaining after the explosion].

In explanation of these numbers, the third column shows the diminution after the explosion in the aggregate relative volumes of the common air and inflammable air as given in the first and second columns; the fourth column gives the relative volume of the remaining "air," and columns five and six its test and standard, that is the amount of oxygen (if any) it still contained. These last numbers were doubtless obtained by the nitric oxide eudiometer described in the previous paper: thus the fifth column indicates the contraction which took place when the "air" remaining after the explosion was mixed with a little more than its own volume of nitric oxide; and the sixth the volume of oxygen in that air according to the scale suggested by Cavendish in that paper, phlogisticated air being zero, common air 1 and dephlogisticated air 4·8.

Cavendish concentrates attention on the fourth experiment. Having regard to the primary object of his inquiry, its significance was unmistakable. He says respecting it:

From the fourth experiment it appears, that 423 measures of inflammable air are nearly sufficient to completely phlogisticate 1000 of common air; and that the bulk of the air remaining after the explosion is then very little more than four-fifths of the common air employed; so that, as common air cannot be reduced to a much less bulk than that, by any method of phlogistication, we may safely conclude, that when they are mixed in this proportion, and exploded, almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity, and are condensed into the dew which lines the glass.

There can, of course, be no question that this statement implicitly contains an announcement of the synthetical production of water by the union of hydrogen and oxygen, but it may be doubted whether Cavendish intended it to convey that meaning in the sense we now interpret it. This will be still more evident from subsequent expressions in his paper. It must never be forgotten that the object of his inquiry, which he kept steadily in view, was to follow the transference of phlogiston in the various reactions which he set out to study. At the same time he seized upon the formation of the dew as a fact of cardinal importance, and proceeded to collect larger quantities of it with a view to ascertain its real nature. He continues:

The better to examine the nature of this dew, 500000 grain measures of inflammable air were burnt with about 2½ times that quantity of common air, and the burnt air [i.e. the products of the combustion as well as the remaining "air"—mainly nitrogen] made to pass through a glass cylinder eight feet long and three-quarters of an inch in diameter, in order to deposit the dew. The two airs were conveyed slowly into this cylinder by separate copper pipes, passing through a brass plate which stopped up the end of the cylinder; and as neither inflammable nor common air can burn by themselves, there was no danger of the flame spreading into the magazines from which they were conveyed. Each of these magazines consisted of a large tin vessel, inverted into another vessel just big enough to receive it. The inner vessel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer vessel; and in order that the quantity of common air expelled should be 2½ times that of the inflammable, the water was let into the outer vessel by two holes in the bottom of the same tin pan, the hole which conveyed the water into that vessel in which the common air was confined being 2½ times as big as the other.

In trying the experiment, the magazines being first filled with their respective airs, the glass cylinder was taken off, and water let, by the two holes, into the outer vessels, till the airs began to issue from the ends of the copper pipes; they were then set on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grains of water were condensed in the cylinder, which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it yield any pungent smell during the evaporation; in short, it seemed pure water.

...By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almost all the inflammable air, and near one-fifth of the common air, lose their elasticity, and are condensed into dew. And by this experiment it appears, that this dew is plain water, and consequently that almost all the inflammable air, and about one-fifth of the common air, are turned into pure water.

That Cavendish surmised that it was the dephlogisticated portion of common air that was "turned into pure water" by uniting with the inflammable air, is evident from his next experiment, which he thus describes:

. In order to examine the nature of the matter condensed on firing a mixture of dephlogisticated and inflammable air, I took a glass globe, holding 8800 grain measures, furnished with a brass cock, and an apparatus for firing air by electricity. This globe was well exhausted by an air-pump and then filled with a mixture of inflammable and dephlogisticated air, by shutting the cock, fastening a bent glass tube to its mouth and letting up the end of it into a glass jar inverted into water, and containing a mixture of 19500 grain measures of dephlogisticated air, and 37000 of inflammable; so that, on opening the cock, some of this mixed air rushed through the bent tube, and filled the globe[1]. The cock was then shut, and the included air fired by electricity, by which means almost

[1] "In order to prevent any water from getting into this tube, while dipped under water to let it up into the glass jar, a bit of wax was stuck on the end of it, which was rubbed off when raised above the surface of the water." [*Note*. This tube was filled with common air to begin with, the nitrogen of which would find its way into the globe.]

all of it lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the sixth part of the mixture, almost the whole of it was exploded therein, without any fresh exhaustion of the globe.

Although it is not so stated, it may be presumed that the bent tube remained in the jar containing the mixed gases throughout the course of the experiment, otherwise it would have been refilled with more or less common air between successive explosions, and so have introduced nitrogen into the globe at each operation.

As Cavendish wished to ascertain the volume and character of the "air" remaining in the globe after the series of explosions, as well as the weight and nature of the liquid produced, he proceeded as follows:

As I was desirous to try the quantity and test of this burnt [residual] air, without letting any water into the globe, which would have prevented my examining the nature of the condensed matter [i.e. the water produced], I took a larger globe, furnished also with a stop-cock, exhausted it by an air-pump, and screwed it on upon the cock of the former globe; upon which, by opening both cocks, the air rushed out of the smaller globe into the larger, till it became of equal density in both; then, by shutting the cock of the larger globe, unscrewing it again from the former, and opening it under water, I was enabled to find the quantity of the burnt air in it; and consequently, as the proportion which the contents of the two globes bore to each other was known, could tell the quantity of burnt air in the small globe before the communication was made between them. By this means the whole quantity of the burnt air was found to be 2950 grain measures: its standard was 1·85.

The liquor condensed in the globe, in weight about 30 grains, was sensibly acid to the taste, and by saturation with fixed alkali, and evaporation, yielded near two grains of nitre; so that it consisted of water united to a small quantity of nitrous acid. No sooty matter was deposited in the globe. The dephlogisticated air used in this experiment was procured from red precipitate [not *mercurius calcinatus per se*], that is, from a solution of quicksilver in spirit of nitre distilled till it acquires a red colour.

Cavendish's procedure was ingenious but it was open to several sources of error which must have affected the quantitative measurements. To begin with, it depended upon the efficiency of the air-pump which at that period was not very high. Hence the volume of the residual "air" was almost certainly overestimated. The standard of the residual "air" was 1·85 which means that it contained more oxygen than common air in the ratio of 1·85 to 1. That is, in round numbers it contained about 39 per cent of oxygen, so that there must have been a notable amount of nitrogen present—not less than about 1800 grain measures. No standard is given for the oxygen employed: it was presumably regarded as pure: the greater part of the nitrogen must have arisen from the imperfect evacuation of the globes, leakage of stop-cocks, and difficulty of preparing and preserving hydrogen without admixture with common air.

Cavendish then proceeded to search for the origin of the nitrous (nitric) acid in the water.

As it was suspected, that the acid contained in the condensed liquor was no essential part of the dephlogisticated air, but was owing to some acid vapour which came over in making it, and had not been absorbed by the water, the experiment was repeated in the same manner. with some more of the same air, which had been previously washed with water, by keeping it a day or two in a bottle with some water and shaking it frequently.... The condensed liquor was still acid.

Dephlogisticated air prepared by heating red lead with oil of vitriol also gave an acid liquor, as did that from the leaves of plants prepared "in the manner of Doctors Ingenhousz and Priestley."

Cavendish noted that one circumstance common to all the experiments was that "the proportion of inflammable was such, that the burnt [residual] air was not much dephlogisticated; and it was observed that the less phlogisticated it was, the more acid was the condensed liquor." In other words, the acid appeared when the residual air was mainly oxygen, and, within the limits he observed, it seemed that the amount of acid increased with the quantity of oxygen. He therefore increased the proportion of inflammable air,

so that the burnt air was almost completely phlogisticated, its standard being $\frac{1}{10}$ [that is, the residual gas contained not more than about 2 per cent. by volume of oxygen]. The condensed liquor was then not at all acid, but seemed pure water: so that it appears, that with this kind of dephlogisticated air [from plants], the condensed liquor is not at all acid, when the airs are mixed in such a proportion that the burnt air is almost completely phlogisticated, but is considerably so when it is not much phlogisticated.

The experiment was repeated with oxygen from red precipitate and with variable proportions of inflammable air.

In the first, in which the burnt air was almost completely phlogisticated [that is in which there was no substantial excess of oxygen, if any, in the residual gas] the condensed liquor was not at all acid. In the second, in which its standard was 1·86 [i.e. containing about 39 per cent. of oxygen] that is, not much phlogisticated, it was considerably acid; so that with this air, as well as with that from plants, the condensed liquor contains, or is entirely free from, acid, according as the burnt air is less or more phlogisticated; and there can be little doubt but that the same rule obtains with any other kind of dephlogisticated air.

In order to see whether the acid, formed by the explosion of dephlogisticated air obtained by means of the vitriolic acid, would also be of the nitrous kind, I procured some air [oxygen] from turbith mineral [basic mercuric sulphate $HgSO_4 . 2HgO$—the *turpethum minerale* of the iatro-chemists], and exploded it with inflammable air, the proportion being such that the burnt air was not much phlogisticated. The condensed liquor manifested an acidity, which appeared by saturation with a solution of salt of tartar [potassium carbonate], to be of the nitrous kind; and it was found, by the addition of some terra ponderosa salita [barium chloride] to contain little or no vitriolic acid.

On repeating the experiment with common air

in such proportion that the standard of the burnt air was about $\frac{4}{10}$, the condensed liquor was not in the least acid.

He next mixed dephlogisticated air from red precipitate with perfectly phlogisticated air [nitrogen]

in such a proportion as to reduce it to the standard of common air...and then exploded with the same proportion of inflammable air as the common air was in the foregoing experiment, the condensed liquor was not in the least acid.

The conclusions to be drawn from this laborious and protracted inquiry, stated in Cavendish's own words, are as follows:

From the foregoing experiments it appears, that when a mixture of inflammable and dephlogisticated air is exploded in such proportion that the burnt air is not much phlogisticated, the condensed liquor contains a little acid, which is always of the nitrous kind, whatever substance the dephlogisticated air is procured from; but if the proportion be such that the burnt air is almost entirely phlogisticated, the condensed liquor is not at all acid, but seems pure water, without any addition whatever; and as, when they are mixed in that proportion, very little air remains after the explosion, almost the whole being condensed, it follows, that almost the whole of the inflammable and dephlogisticated air is converted into pure water.

Cavendish clearly recognised to what sources the residual gas left after the detonation might be due. He goes on to say:

It is not easy, indeed, to determine from these experiments what proportion the burnt air, remaining after the explosions, bore to the dephlogisticated air employed, as neither the small nor the large globe could be perfectly exhausted of air, and there was no saying with exactness what quantity was left in them; but in most of them, after allowing for this uncertainty, the true quantity of burnt air seemed not more than $\frac{1}{17}$th of the dephlogisticated air employed, or $\frac{1}{60}$th of the mixture. It seems, however, unnecessary to determine this point exactly, as the quantity is so small, that there can be little doubt but that it proceeds only from the impurities mixed with the dephlogisticated and inflammable air, and consequently that, if those airs could be obtained perfectly pure, the whole would be condensed.

He then comments upon the difference observed, as regards the production of nitric acid, between the experiments in which pure oxygen was used, and those in which common air, or a mixture of oxygen and nitrogen in the proportion in which they are present in common air, was employed.

With respect to common air, and dephlogisticated air reduced by the addition of phlogisticated air to the standard of common air, the case is different; as the liquor condensed in exploding them with inflammable air, I believe I may say in any proportion, is not at all acid; perhaps, because if they are mixed in such a proportion as that the burnt air is not much phlogisticated [that is, still contains much oxygen] the explosion is too weak, and not accompanied with sufficient heat.

A surmise which has been abundantly verified by subsequent experience.

The foregoing lengthy extracts from a paper which is classical have been purposely made in order that Cavendish's great discovery and the conclusions he drew from his experiments may be described in his own words and without any attempt to read into them any interpretation based upon or biased by subsequent knowledge. The facts of his experiments are stated with a clearness and precision which leave nothing to be desired, and it is impossible not to admire the care, patience, skill and sagacity with which he traced and accounted for what was without doubt a disturbing factor. He seems, indeed, to have perceived at an early stage of the inquiry that the formation of the nitric acid was purely fortuitous, and no necessary concomitant of the union by explosion of oxygen and hydrogen; nevertheless he recognised that his proof of the true nature of water and the mode of its synthetical formation could not be considered complete until the cause of the accidental contamination had been satisfactorily explained. This section of the inquiry seems to have occupied the greater portion of the time spent upon it, and presumably delayed the announcement of the cardinal discovery of the non-elementary nature of water for possibly a couple of years, with the unfortunate consequence of raising a controversy concerning Cavendish's claim to priority.

This circumstance is alluded to in the following paragraph which was not in the paper as originally received by the Royal Society but was interpolated before it was printed, no doubt with Cavendish's cognisance, by his friend Blagden who became one of the secretaries of the Society, and who had personal knowledge of the facts.

All the foregoing experiments, on the explosion of inflammable air with common and dephlogisticated airs, except those which relate to the cause of the acid found in the water, were made in the summer of the year 1781, and were mentioned by me to Dr Priestley, who in consequence of it made some experiments of the same kind, as he relates in a paper printed in the preceding volume of the Transactions. During the last summer also, a friend of mine gave some account of them to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time so far was M. Lavoisier from thinking any such opinion warranted, that, till he was prevailed on to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable that neither of these gentlemen found any acid in the water produced by the combustion; which might proceed from the latter [Lavoisier] having burnt the two airs in a different manner from what I did; and from the former [Priestley] having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it.

Among the Cavendish papers preserved at Chatsworth is a detached quarto sheet apparently in Blagden's handwriting, but unsigned by him, which seems to throw light upon the history of this interpolation. From a number of similar memoranda relating to foreign chemical literature, in other handwriting than that of Cavendish, to be found among his papers, it would appear he was not familiar with German, and was accustomed to rely upon others for information from contemporary German literature on matters of interest to him. Blagden's memorandum runs as follows:

In a number of Crell's Annals which I happened not to have looked over before (May, 1784) I found the following passage: Mr Cavendish in London has $\left\{\begin{matrix}\text{repeated}\\\text{imitated}\end{matrix}\right.$ the experiments of M. Lavoisier to produce water from dephlogisticated and inflammable air by combustion. He has laid before the Royal Society the result of his experiments which confirm that change of the airs, or the new generation of water. His memoir has met with great approbation and won the assent of such a well-informed chymist as Mr Kirwan.

Nothing appears by which it is possible to judge from whom Mr Crell received this information.

Thursday morning Mar. 10.

It is right to mention, that in the next number of the Annals (for June) there is a letter from Mr Kirwan mentioning your paper in proper terms, without any notice of Mr Lavoisier's name or pretensions.

There can be little doubt that this memorandum was the immediate occasion of Blagden's interpolation, and that its introduction was sanctioned by Cavendish to protect himself from the insinuation implied in the communication from which Blagden had quoted.

It will be convenient to defer further comment on this paragraph, as well as on other passages relating to the connection of Priestley, Watt and Lavoisier with the history of the discovery of the composition of water, until the account of the contents of the paper is completed.

Before discussing the meaning of the phenomena he had observed as regards the formation of the nitric acid, Cavendish ventures upon an opinion as to the real nature of phlogisticated air [nitrogen] and its relation to nitrous [nitric] acid. Phlogisticated air, he conceives, must be

nothing else than the nitrous acid united to phlogiston; for when nitre is deflagrated with charcoal, the acid is almost entirely converted into this kind of air....As far as I can perceive too, at present, the air into which much the greatest part of the acid is converted, differs in no respect from common air phlogisticated. A small part of the acid, however, is turned into nitrous air [nitric oxide], and the whole is mixed with a good deal of fixed, and perhaps a little inflammable air [carbonic oxide] both proceeding from the charcoal.

This, of course, is the case and, although only a partial interpretation, so far as it goes, it is consistent with the teaching of the phlogiston school. Here the phlogiston is derived from the charcoal. He further points to the fact "that the nitrous [nitric] acid is also convertible by phlogistication into nitrous air [nitric oxide]" and he sees an analogy in this example of the effect of partial phlogistication to the behaviour of vitriolic acid which when united to a smaller proportion of phlogiston forms the volatile sulphureous acid [sulphur dioxide] but when united to a larger proportion of phlogiston forms sulphur, which shows

no signs of acidity.... In like manner, the nitrous acid, united to a certain quantity of phlogiston, forms nitrous fumes and nitrous air...but when united to a different, in all probability a larger quantity, it forms phlogisticated air, which shows no signs of acidity, and is still less disposed to part with its phlogiston than sulphur.

"This being premised," as Cavendish says, let us see how he applies these conceptions to the explanation of the cause of the acidity of the water.

There seem two ways by which the phænomena of the acid found in the condensed liquor may be explained: first by supposing that dephlogisticated air contains a little nitrous acid which enters into it as one of its component parts, and that this acid, when the inflammable air is in a sufficient proportion, unites to the phlogiston, and is turned into phlogisticated air, but does not when the inflammable air is in too small a proportion; and, secondly, by supposing that there is no nitrous acid mixed with, or entering into the composition of, dephlogisticated air, but that, when this air is in a sufficient proportion, part of the phlogisticated air with which it is debased is, by the strong affinity of phlogiston to dephlogisticated air, deprived of its phlogiston, and turned into nitrous acid; whereas when the dephlogisticated air is not more than sufficient to consume the inflammable air, none then remains to deprive the phlogisticated air of its phlogiston, and turn it into acid.

Although, as we have seen, Cavendish probably regarded the nitric acid as an accidental contamination, its formation in his experiments was evidently considered by him as significant in throwing light upon the true nature of water and of dephlogisticated air. Indeed from the way in which he labours this part of the inquiry he would appear to consider the questions of the formation of the water and acid as inseparable and of almost equal importance. It is obvious from the above extracts that he regarded the presence of the acid as affording a possible clue to the constitution of dephlogisticated air and that he considered it was necessary to know this in order to form a just conception of the true nature of water and the mode of its synthesis.

He pointed out that if the nitrous acid is not to be considered as an essential constituent of dephlogisticated air, then, he says,

I think we must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose [and as Cavendish formerly supposed], or else water united to phlogiston; since, according to this supposition, these two substances united together form pure water. On the other hand, if the first explanation be true, we must suppose that dephlogisticated air consists of water united to a little nitrous acid and deprived of its phlogiston; but still the nitrous acid in it must make only a very small part of the whole, as it is found, that the phlogisticated air, which it is converted into, is very small in comparison of the dephlogisticated air.

It will be observed from the wording of this passage that Cavendish had changed the opinion which he held in 1766, and which he expressed in his paper on inflammable air from metals, that this air was in reality phlogiston. He now ascribes this view to Priestley and Kirwan, and inclines to the belief that inflammable air is a compound of water and phlogiston —a sort of phlogiston hydrate. In a long footnote he explains what had led him to alter his opinion. Its substance is this: whereas common or dephlogisticated air will combine more or less readily at ordinary temperatures with phlogiston already united to substances, these gases refuse to unite with inflammable air unless at a red heat,

and it seems inexplicable, that they should refuse to unite to pure phlogiston, when they are able to extract it from substances to which it has an affinity: that is, that they should overcome the affinity of phlogiston to other substances, and extract it from them, when they will not even unite to it when presented to them.

Another reason would seem to be that in all the operations known to him in which inflammable air is generated water is more or less concerned.

As regards the two views of the nature of dephlogisticated air, Cavendish inclines to the belief that the nitrous acid is not an essential constituent of it, and that in fact it was not directly derived from it, inasmuch as this acid was formed not only in the case of oxygen from red precipitate but also in that derived from plants and from turbith mineral;

and it seems not likely that air procured from plants, and still less likely that air procured from a solution of mercury in oil of vitriol should contain any nitrous acid. Another strong argument in favour of this opinion is, that dephlogisticated air yields no nitrous acid when phlogisticated by liver of sulphur; for if this air contains nitrous acid and yields it when phlogisticated by explosion with inflammable air, it is very extraordinary that it should not do so when phlogisticated by other means.

What Cavendish regarded as a "strong argument" as a matter of fact only shows how little the real nature of the change experienced when air or oxygen is brought into contact with liver of sulphur was known to him.

> But what forms a stronger, and I think almost decisive argument, in favour of this explanation is, that when the dephlogisticated air is very pure, the condensed liquor is made much more strongly acid by mixing the air to be exploded with a little phlogisticated air [nitrogen].

The details of two experiments are then given in which the same quantities of a mixture of hydrogen and oxygen are exploded, to one of which, however, a quantity of nitrogen was added; "the condensed liquor in both cases was acid, but that in the latter evidently more so," as appeared from the amount of lime required to neutralise it. In the case where the nitrogen was added the burnt air would be more phlogisticated than in the other, and in that case "from what has been before said" from the line of argument and the suppositious presence of nitric acid as an essential constituent of oxygen, the condensed liquor should be less acid; "and yet it was found to be much more so; which shows strongly that it was the phlogisticated air which furnished the acid."

Further comparative experiments of a similar kind were made with the same general result and leading to the same inference, and the conclusion of the whole matter is thus stated:

> From what has been said there seems the utmost reason to think, that dephlogisticated air is only water deprived of its phlogiston, and that inflammable air, as was before said, is either phlogisticated water, or else pure phlogiston; but in all probability the former.

This, then, is Cavendish's formal statement of his views of the nature of dephlogisticated and inflammable air, and of their several relations to water. The conclusion is expressed in terms of phlogiston, and it is impossible to gather from the statement as it stands, whether Cavendish was convinced that water was actually a compound substance. He does not explicitly say so. The issue is confused by his view as to the nature of inflammable air and by our ignorance of his own opinion as to the real nature of phlogiston. Did he regard it as a material entity, or an imponderable principle—simply an *affection* or quality which by transference to and fro, determined the characters of substances? As we read it his statement might imply that he considered that water was formed by the phlogiston of his hypothetical hydrate phlogisticating the dephlogisticated air, whereby the water of the hydrate was liberated. Evidently he did not regard hydrogen as a simple elementary substance, in the modern sense, and to that extent, at least, his interpretation of his results is incomplete and erroneous.

Had he still continued to regard phlogiston as identical with hydrogen, of which he had determined the relative weight, and of whose material existence he was therefore assured, our inference as to his real view of the nature of water would have had a surer basis. It would almost seem as if he still halted between two opinions, and that his judgement was biased by a lingering old-time belief in the essential and fundamental unity of all "airs" and of water as a primordial element.

The statement of Cavendish's views of the nature of dephlogisticated and inflammable airs and of their relation to water, is followed by a paragraph which occasioned much discussion in the course of the controversy to which his paper gave rise. It may be desirable therefore to reproduce it in full:

As Mr. Watt, in a paper lately read before this Society, supposes water to consist of dephlogisticated air and phlogiston deprived of part of their latent heat, whereas I take no notice of the latter circumstance, it may be proper to mention in a few words the reason of this apparent difference between us. If there be any such thing as elementary heat, it must be allowed that what Mr. Watt says is true; but by the same rule we ought to say, that the diluted mineral acids consist of the concentrated acids united to water and deprived of part of their latent heat; that solutions of sal-ammoniac, and most other neutral salts, consist of the salt united to water and elementary heat; and a similar language ought to be used in speaking of almost all chemical combinations, as there are very few which are not attended with some increase or diminution of heat. Now I have chosen to avoid this form of speaking, both because I think it more likely that there is no such thing as elementary heat, and because saying so in this instance, without using similar expressions in speaking of other chemical unions, would be improper, and would lead to false ideas; and it may even admit of doubt, whether the doing it in general would not cause more trouble and perplexity than it is worth.

This passage was interpolated by Cavendish after his paper was received by the Royal Society and before it was published in the *Phil. Trans.* and this circumstance gave rise to the assertion that if Cavendish did not actually owe to Watt his conception of the inference to be drawn from his experiments, to Watt at least belongs the credit of having been the first to point out that water is a compound substance and that its components are dephlogisticated air and phlogiston.

At first sight it may be thought the point of difference between the two philosophers as regards heat in relation to the composition of water is irrelevant. In reality this is not so, as something turns on their respective views of the nature of heat, Cavendish, at all events, being convinced, as will be subsequently evident, that it is not a material entity.

How it happened that Watt came into conflict with Cavendish on the question of priority will be stated later.

The greater part of the rest of the paper is concerned with speculations on the nature of common air and of dephlogisticated air which are of interest as throwing some light upon the extent to which his opinions had been modified by experience and reflection since the publication of his paper on inflammable air, seventeen years previously. He is now more inclined to the opinion of Lavoisier and Scheele that dephlogisticated and phlogisticated airs are "quite distinct substances, and not differing only in their degree of phlogistication; and that common air is a mixture of the two." He finds support for this view in his discovery that pure dephlogisticated air by complete phlogistication is converted into water instead of into phlogisticated air. It is interesting to note how closely he approaches to the view-point of the French school in the following passage: "From what has been said, it follows, that instead of saying air is phlogisticated or dephlogisticated by any means, it would be more strictly just to say, it is deprived of, or receives, an addition of dephlogisticated air." But however "strictly just" it may be, Cavendish, confirmed phlogistian as he was, cannot bring himself to say it, for he immediately adds "but as the other expression is convenient, and can scarcely be considered as improper, I shall still frequently make use of it in the remainder of this paper."

Considerable space is occupied by a discussion of the theory of the various methods employed in the production of dephlogisticated air, and he traverses Priestley's opinion that nitrous and vitriolic acids are convertible into dephlogisticated air: "Their use in preparing it is owing only to the great power they possess of depriving bodies of their phlogiston." He shows that the production of oxygen from red precipitate is not owing to any nitrous acid in that substance "and consequently that, in procuring dephlogisticated air from it, no acid is converted into air; and it is reasonable to conclude, therefore, that no such change is produced in procuring it from any other substance"—a sweeping generalisation on too limited a basis.

The way in which the nitrous [nitric] acid acts, in the production of it [dephlogisticated air] from red precipitate, seems to be as follows. On distilling the mixture of quicksilver and spirit of nitre, the acid comes over, loaded with phlogiston, in the form of nitrous vapour [oxides of nitrogen], and continues to do so till the remaining matter [mercuric nitrate] acquires its full red colour, by which time all the nitrous acid is driven over, but some of the watery part still remains behind, and adheres strongly to the quicksilver; so that the red precipitate may be considered, either as quicksilver deprived of part of its phlogiston, and united to a certain portion of water, or as quicksilver united to dephlogisticated air[1], after which, on further increasing the heat, the water in it rises deprived of its phlogiston, that is, in the form of dephlogisticated air, and at the

[1] In a footnote to this passage he says "It would be ridiculous to say, that it is the quicksilver in the red precipitate which is deprived of its phlogiston, and not the water, or that it is the water and not the quicksilver; all that we can say is that red precipitate consists of quicksilver and water, one or both of which are deprived of part of their phlogiston. In like manner, during the preparation of the red precipitate, it is certain that the acid absorbs phlogiston, either from the quicksilver or the water; but we are by no means authorised to say from which."

same time the quicksilver distils over in its metallic form.... Mercurius calcinatus appears to be only quicksilver which has absorbed dephlogisticated air from the atmosphere during its preparation; accordingly, by giving it a sufficient heat, the dephlogisticated air is driven off, and the quicksilver acquires its original form. It seems therefore that mercurius calcinatus and red precipitate, though prepared in a different manner, are very nearly the same thing.

It seems to be pretty obvious from this somewhat laboured account that Cavendish was disposed to think *mercurius calcinatus* and red precipitate, although "very nearly," were not in reality quite "the same thing," but that red precipitate differed from *mercurius calcinatus* in containing "a certain portion [amount] of water" and that when heated the water is deprived of its phlogiston, thereby liberating the dephlogisticated air—the phlogiston presumably attaching itself to the quicksilver which consequently resumes its metallic form.

Apart from the light it throws—however dimly—on Cavendish's views as to the essential nature of the relation between dephlogisticated air and water, this passage, as already pointed out by Dr Wilson, is instructive

as showing what many other passages in the papers of Cavendish, and his contemporaries also show, that the discovery of the composition of water would not, in the hands of the disciples of the phlogiston school, have materially altered the aspect of chemistry. The difference it introduced was little more than this, that where formerly transferences of phlogiston, from one body to another, were assumed to take place, now water instead of phlogiston was shifted backwards and forwards, and decomposed and recomposed as the exigencies of theory required[1].

An exemplification of the truth, as elsewhere remarked by Dr Wilson, of the necessity under which a false theory lies of multiplying falsities.

The production of oxygen from nitre, in the course of which Cavendish observed the formation of potassium nitrite, is accounted for in a similar way. Now that water was known to contain or to be capable of yielding dephlogisticated air, that "air" is to be regarded as coming from the water which the nitre is assumed to contain as an essential constituent. The same line of reasoning led him to conclude that "the rationale of the production of dephlogisticated air from turbith mineral, and from red precipitate, are nearly similar."

Cavendish then comments on the action of light on substances, such as the bleaching of an alcoholic solution of chlorophyll, the colouring of nitric acid, and of silver chloride, etc. which he attributes to the absorption of phlogiston from the water, and the liberation of dephlogisticated air. He conceives a similar action to take place in plants under the influence of sunshine: "it seems likely," he says, "that the use of light, in promoting the growth of plants and the production of dephlogisticated air from them,

[1] Wilson, *Life of Cavendish*, p. 248.

is that it enables them to absorb phlogiston from the water." He attempts to answer certain objections which may be urged against this view of the origin of the oxygen, and he seeks to explain the fact observed by Senebier

"that plants yield much more dephlogisticated air in distilled water impregnated with fixed air, than in plain distilled water," by suggesting that "as fixed air is a principal constituent part of vegetable substances, it is reasonable to suppose that the work of vegetation will go on better in water containing this substance, than in other water."

This sentence concludes the paper as it was originally received by the Royal Society. Before it was actually printed off Cavendish made a very significant addition from which it is desirable to quote pretty fully, as it shows that he was quite alive to the interpretation which the new school might put upon his results. It would almost seem as if he had anticipated the use which the opponents of phlogistonism would make of his discovery, and that it fell to him as a leading upholder of Stahl's doctrine to combat their arguments. He says:

There are several memoirs of Mr. Lavoisier published by the Academy of Sciences, in which he intirely discards phlogiston, and explains those phænomena which have been usually attributed to the loss or attraction of that substance, by the absorption or expulsion of dephlogisticated air; and as not only the foregoing experiments, but most other phænomena of nature, seem explicable as well, or nearly as well, on this as upon the commonly believed principle of phlogiston, it may be proper briefly to mention in what manner I would explain them on this principle, and why I have adhered to the other. In doing this, I shall not conform strictly to his theory, but shall make such additions and alterations as seem to suit it best to the phænomena; the more so, as the foregoing experiments may, perhaps, induce the author himself to think some such additions proper.

To seek to modify an opponent's theory by making such additions and alterations as in one's judgement may best suit the phenomena would seem at first sight an unwarrantable procedure on the part of a controversialist who strives to combat that theory; but, as a matter of fact, no reasonable objection could be urged against the manner in which Lavoisier's views are stated by Cavendish in explanation of the various phenomena related in his paper. Cavendish says that water according to Lavoisier, "consists of inflammable air united to dephlogisticated air"; and in effect that nitric oxide, sulphurous acid, and phosphoric acid are combinations respectively of nitrogen, sulphur and phosphorus with oxygen; and that by further oxidation nitric oxide and sulphurous acid may be converted respectively into nitric acid and oil of vitriol; that the metallic calces are oxides of the metals. The rationale of the production of red precipitate from mercury and nitric acid and its decomposition by heat into oxygen and mercury is correctly stated by him, as is the production of oxygen from nitre. The only ambiguity is in the production of oxygen from plants which is partially, at least, explicable by the circumstance that the composition of fixed air was unknown to Cavendish and the real significance of Senebier's observation was therefore not recognised by him.

But in spite of this perspicuous and impartial statement of Lavoisier's views, which of course had the crowning merit of explaining "the phænomena of nature" without the help of a purely hypothetical principle, concerning which no two of those who believed in it could agree as to its real character or attributes, Cavendish could not shake himself free from his orthodoxy.

It seems, therefore, from what has been said, as if the phænomena of nature might be explained very well on this principle, without the help of phlogiston; and indeed, as adding dephlogisticated air to a body comes to the same thing as depriving it of its phlogiston and adding water to it, and as there are, perhaps, no bodies entirely destitute of water, and as I know no way by which phlogiston can be transferred from one body to another, without leaving it uncertain whether water is not at the same time transferred, it will be very difficult to determine by experiment which of these opinions is the truest; but as the commonly received principle of phlogiston explains all phænomena, at least as well as Mr. Lavoisier's, I have adhered to that.

It is unnecessary to make any lengthened comment upon these statements. It might appear that in reality there is no essential difference in the views of Lavoisier and Cavendish as to the chemical nature of water. But that is not so. Lavoisier had a clear conception of the individuality of hydrogen, even before he or his associates coined that word; on the other hand Cavendish thought inflammable air was a common principle of bodies rich in phlogiston, and was capable of assuming an elastic form, either alone or in combination with water. He may have drawn a clearer distinction than Priestley between the various forms of "inflammable air," but he was no less convinced than Priestley that all of them were compounds of phlogiston. As regards his reference to Lavoisier's explanations of the phenomena of nature without reference to phlogiston, he makes no real attempt to combat them, and is apparently unaware of the *petitio principii* involved in his statement: he simply contents himself with reiterating his belief in the existence of phlogiston. "The human mind," wrote Davy, "is always governed not by what it knows, but by what it believes."

It can hardly be doubted, as his biographer admits, that Cavendish's defence of his views was an afterthought, added after his opinions had been made public, as a justification of what could not then be withdrawn.

We now turn to the allusions in Cavendish's paper to Lavoisier and Watt, in reference to their share in the experimental proof of the compound nature of water.

Some time before the results of Cavendish's work were made known to him, Lavoisier had enunciated the hypothesis that dephlogisticated air was the principle of acidity, since its combinations, more especially with substances supposed by his contemporaries to be rich in phlogiston, such as carbon, sulphur, phosphorus, etc., were, as he thought, invariably acids. Hence his scepticism as to the validity of Cavendish's results. It seemed to him incredible that the inflammable air from metals—which some indeed had regarded as phlogiston itself, should by its union with oxygen furnish a perfectly neutral substance. Lavoisier, in fact, had publicly stated only a year or two previously, "que l'air inflammable en brûlant devoit donner de l'acide vitriolique, ou de l'acide sulfureux."

In Lavoisier's own account of the experiment he was prevailed upon to repeat, he admits his prior acquaintance with Cavendish's results, and states that Blagden was his informant:

> Ce fut le 24 Juin, 1783, que fîmes cette expérience, M. de la Place et moi, en présence de Mm. le Roi, de Vandermonde, de plusieurs autres Académiciens, et de M. Blagden, aujourd'hui Secrétaire de la Société Royale de Londres; ce dernier nous apprit que M. Cavendish avoit déjà essayé, à Londres, de brûler de l'air inflammable dans des vaisseaux fermés, et qu'il avoit obtenu une quantité d'eau très sensible (*Mém. de l'Acad.* 1781, p. 472).

Blagden felt constrained to point out in a letter addressed to Crell, the editor of the *Chemische Annalen*, and published in that journal for 1786, that the above was a partial and somewhat disingenuous account of what had actually transpired. The letter which is not dated (according to Muirhead's translation) runs as follows:

> I can certainly give you the best account of the little dispute about the first discoverer of the artificial generation of water, as I was the principal instrument through which the first news of the discovery that had been already made was communicated to Mr. Lavoisier. The following is a short statement of the history:
>
> In the spring of 1783, Mr. Cavendish communicated to me and other members of the Royal Society, his particular friends, the result of some experiments with which he had for a long time been occupied. He showed us, that out of them he must draw the conclusion, that dephlogisticated air was nothing else than water deprived of its phlogiston; and *vice versâ* that water was dephlogisticated air united with phlogiston. About the same time the news was brought to London, that Mr. Watt of Birmingham had been induced by some observations [by Priestley] to form a similar opinion. Soon after this I went to Paris, and in the company of Mr. Lavoisier, and of some other members of the Royal Academy of Sciences, I gave some account of these new experiments and of the opinions founded upon them. They replied, that they had already heard something of these experiments, and particularly, that Dr. Priestley had repeated them. They did not doubt, that in such manner a considerable quantity of water might be obtained; but they felt convinced that it did not come near to the weight of the two species of air employed; on which account it was not to be regarded as water formed or produced out of the two kinds of air, but was already contained in and united with the airs, and deposited in their combustion. This opinion was held

by Mr. Lavoisier, as well as by the rest of the gentlemen who conferred on the subject; but as the experiment itself appeared to them very remarkable in all points of view, they unanimously requested Mr. Lavoisier, who possessed all the necessary preparations, to repeat the experiment on a somewhat larger scale, as early as possible. This desire he complied with on the 24th June, 1783 (as he relates in the latest volume of the *Paris Memoirs*). From Mr. Lavoisier's own account of his experiment, it sufficiently appears, that at that period he had not yet formed the opinion that water was composed of dephlogisticated and inflammable airs; for he expected that a sort of acid would be produced by their union. In general, Mr. Lavoisier cannot be convicted of having advanced anything contrary to truth; but it can less be denied, that he concealed a part of the truth. For he should have acknowledged that I had, some days before, apprised him of Mr. Cavendish's experiments, instead of which, the expression "il nous apprit" gives rise to the idea that I had not informed him earlier than that very day. In like manner, Mr. Lavoisier has passed over a very remarkable circumstance, namely, that the experiment was made in consequence of what I had informed him of. He should likewise have stated in his publication, not only that Mr. Cavendish had obtained "une quantité d'eau très sensible" but that the water was equal to the weight of the two airs added together. Moreover, he should have added, that I had made him acquainted with Messrs Cavendish and Watt's conclusions; namely, that water, and not an acid, or any other substance, arose from the combustion of the inflammable and dephlogisticated airs. But *those* conclusions opened the way to Mr. Lavoisier's present theory, which perfectly agrees with that of Mr. Cavendish; only that Mr. Lavoisier accommodates it to his old theory, which banishes phlogiston. Mr. Monge's experiments (of which Mr. Lavoisier speaks as if made about the same time) were really not made until pretty long, I believe at least two months, later than Mr. Lavoisier's own, and were undertaken on receiving information of them.

The course of all this history will clearly convince you, that Mr. Lavoisier (instead of being led to the discovery by following up the experiments which he and Mr. Bucquet had commenced in 1777) was induced to institute again such experiments solely by the account he received from me, and of our English experiments; and that he really discovered nothing but what had before been pointed out to him to have been previously made out and demonstrated in England.

That at least one of Lavoisier's colleagues was aware of the real facts and merits of the case is evident from a letter which La Place addressed to Deluc on June 28th, 1783, which contains the following passage:

M. Lavoisier and I have repeated recently before Mr. Blagden and several other persons, the experiment of Mr. Cavendish upon the conversion into water of dephlogisticated and inflammable airs, by their combustion; with this difference, that we have burned them without the assistance of the electric spark, by bringing together two currents, the one of pure air, the other of inflammable air. We have obtained in this way more than 2½ drachms of pure water, or which, at least, had no character of acidity, and was insipid to the taste; but we do not yet know if this quantity of water represents the weight of the airs consumed. It is an experiment to be recommenced with all possible attention, and which appears to me of the greatest importance.

These statements, which have never been contradicted, would seem to leave no doubt on the question of priority as between Cavendish and Lavoisier concerning the experimental facts, or, indeed, as to the inference which each drew from them as to the non-elementary nature of water.

How Dr Crell regarded the matter is evident from a footnote which he appended to a translation of Cavendish's memoir which appeared in the *Chemische Annalen* for 1785, Part IV, p. 324.

This communication contains the substance of a paper presented to the Royal Society in London, by Henry Cavendish, Esq. and which has not only been inserted in the *Philosophical Transactions*; but has also been published separate under the title of "Experiments on Air" (London, J. Nichols, 1784, 4°, p. 37). Soon after, Sir Joseph Banks, Bart. (President of the Royal Society) was so obliging as to send me a copy, for the purpose of mentioning it in these *Chemical Annals*. This becomes a twofold duty upon me, because I have committed the same error as most of my compatriots and other men of letters, by ascribing to Mr. Lavoisier the discovery of the water resulting from the different kinds of inflamed air (see *Chem. Annals*, 1785, Part I, p. 48). Justice alone, therefore, demands of me, to return to Mr. Cavendish (whom I take this opportunity to assure of my most sincere esteem) the well earned honour of *The First Discovery* of this so very important and remarkable Phenomenon (which appears clearly from this paper) and at the same time to correct some other circumstances in mine above-mentioned publication[1].

We have now to consider the significance of the allusion to Watt. This has no reference to any experimental proof afforded by Watt himself but to certain inferences he deduced from the observations of his friend Priestley. The essential facts, stated shortly, are as follows. Watt, as we gather from his correspondence with Black, had long been of the opinion that "air" was a modification of water: he thought that as steam parts with its latent heat as it acquires sensible heat, or is more compressed, when it arrives at a certain point, it will have no latent heat, and may, under proper compression, be an elastic fluid nearly as specifically heavy as water; at which point it would again change its state and become air. He finds some confirmation of this belief in experiments on which Priestley was engaged at the period when Cavendish was occupied with the work which has just been described. Priestley's results on the seeming conversion of water into air were wholly fallacious, as he subsequently found.

Although Priestley's discovery of the source of his error may have shaken, and indeed did shake, Watt's belief in the experimental proof of the conversion of water into "air," it apparently had no influence on his

[1] Extract from a translation to be found among the Cavendish MSS.

conviction of the essential unity of all forms of "air." This is abundantly evident from the few chemical papers he published, and from the tenor of his correspondence with Black, Priestley, Kirwan and others of his contemporaries. Watt was no doubt familiar with the "mere random experiment" which Priestley made in conjunction with Warltire, but he seems to have attached no more importance than they did to the formation of the dew on detonation, but like them, to have regarded it as evidence that common air deposits its moisture when phlogisticated. Cavendish, as we have seen, communicated to Priestley the facts arising from his repetition of Warltire's experiments, as Priestley relates in a paper published in the *Phil. Trans.* for 1783. Priestley interpreted Cavendish's experiment as proving the conversion of air into water, thus strengthening his belief in the intimate connection between water and air of which hitherto he had been unable to acquire satisfactory proof. "Still hearing," he says, "of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr. Cavendish's concerning the *reconversion* of air into water by *decomposing* it in conjunction with inflammable air." He therefore repeated, as he thought, Cavendish's experiments but with certain modifications which he imagined, although quite erroneously, would remove objections which might be urged against them. He says:

> In order to be sure that the water I might *find in the air was really a constituent part of it,* and not what it might have imbibed after its formation, I made a quantity of both dephlogisticated and inflammable air, in such a manner as that neither of them should ever come into contact with water, receiving them as they were produced in mercury; the former from nitre, and in the middle of the process (long after the water of crystallisation was come over[1]), and the latter from perfectly made charcoal. The two kinds of air thus produced I decomposed by firing them together by the electric explosion and found a manifest deposition of water, and to appearance in the same quantity as if both the kinds of air had been previously confined by water...the result was such as to afford a strong presumption that the air was reconverted into water, and *therefore that the origin of it had been air.*

It is unnecessary to examine this passage very minutely. The better, or rather the seeming better, is here the enemy of the good. In the attempt to prepare pure dry gases Priestley only succeeded in making them more impure: it was physically impossible that he could have obtained, as he surmised, "the weight of the decomposed air in the moisture." But it was upon this wholly fallacious experiment that Watt theorised: it clearly proved to him that water and air are mutually convertible and are therefore essentially the same. Under date April 21st, 1783, he writes to Black: "Dr. Priestley has made many more experiments on the conversion of water into air, and I believe I have found out the cause of it; which I have

[1] Nitre contains no water of crystallisation.

put in the form of a letter to him which will be read at the Royal Society with his paper on the subject." He then gives Black a summary of the facts, or supposed facts, on which he bases his deductions:

In the deflagration of the inflammable and dephlogisticated airs, the airs unite with violence, become red hot, and, on cooling totally disappear. The only fixed matter which remains is *water*; and water, light and heat are all the products. Are we not then authorised to conclude that water is composed of dephlogisticated and inflammable air, or phlogiston, deprived of part of their latent heat, and that dephlogisticated, or pure air [oxygen] is composed of water deprived of its phlogiston, and united to heat and light; and if light be only a modification of heat, or a component part of phlogiston, then pure air [oxygen] consists of water deprived of its phlogiston and of latent heat.

On learning of the fallacy of Priestley's experimental proof of the conversion of water into air Watt desired that his letter to Priestley should not be publicly read and it was temporarily withdrawn on account of what Watt styled in a letter to Black, Priestley's "ugly experiment." In the meantime knowledge of Watt's letter, or of his views, seems to have been conveyed to Paris. In a letter from Watt to Deluc dated November 30th, 1783, we read:

I was at Dr. Priestley's last night. He thinks, as I do, that Mr. Lavoisier, having heard some imperfect account of the paper I wrote in the spring, has run away with the idea and made up a memoir hastily, without any satisfactory proof.... I, therefore, put the query to you of the propriety of sending my letter to pass through their hands to be printed: for even if this theory is Mr. Lavoisier's own, I am vain enough to think that he may get some hints from my letter, which may enable him to make experiments, and to improve his theory, and produce a memoir to the Academy before my letter can be printed, which may be so much superior as to eclipse my poor performance, and sink it into utter oblivion; nay, worse, I may be condemned as a plagiary, for I certainly cannot be heard in opposition to an Academician and a Financier....But, after all, I may be doing Mr. Lavoisier injustice.

Cavendish's paper was read to the Royal Society on January 15th, 1784, and some of Watt's friends, Deluc in particular, hastened to imply that its conclusions were framed in the light of knowledge derived from Watt's letter. In reply to Deluc Watt writes:

On the slight glance I have been able to give your extract of the paper, I think his theory very different from mine; which of the two is the right I cannot say; his is more likely to be so, as he has made many more experiments, and consequently has more facts to argue upon.

Watt's letter to Priestley, as well as one he wrote subsequently to Deluc, were by their author's direction subsequently merged into a single communication and published in the *Phil. Trans.* under the title "Thoughts on the Constituent Parts of Water, and of Dephlogisticated Air; with an account of some Experiments on that Subject. In a letter from Mr. James Watt, Engineer, to Mr. Deluc, F.R.S."

The controversy as regards priority died down during the lifetime of the parties principally concerned, and it seems to have made no difference in their friendly relations. It was however revived by the action of Arago, who as Perpetual Secretary of the French Academy read an *Eloge* on Watt who, like Cavendish, had been elected a member of the Institute. This provoked a reply from the Rev. W. Vernon Harcourt, who in the course of his Presidential Address to the British Association at the Birmingham Meeting in 1839, set out in detail all the facts in support of Cavendish's claims, including a lithographed reprint of the original laboratory notes, giving the dates and details of the experiments, thus occasioning what is known in the history of science as the Water Controversy, in which Brougham, Peacock, Muirhead, Whewell, Brewster and Jeffrey took part. The considerable body of literature to which this gave rise was critically examined by George Wilson and constitutes a section of his *Life of Cavendish.*

Time has now set its seal upon the matter and there is practical agreement as to its merits. As regards Lavoisier, it cannot be claimed that he was the first to obtain the facts. To Cavendish belongs the merit of having first supplied the true experimental basis upon which accurate knowledge could alone be founded. Watt, on the other hand, although reasoning from imperfect and indeed altogether erroneous data, was the first, so far as we can prove from documentary evidence, to state distinctly that water is not an element, but is composed, weight for weight, of two other substances, one of which he regarded as phlogiston and the other as dephlogisticated air. It would be a mistake, however, to suppose that Watt taught precisely the same doctrine of the true nature of water that we hold to-day. Nor did Cavendish utter a more certain sound. What we regard to-day as the expression of the truth, we owe to Lavoisier, who stated it with a directness and a precision that ultimately swept all doubt and hesitation aside—except in the mind of Priestley, whose "random experiment" gave the first glimmer of the truth[1].

As already stated, Cavendish's paper, or rather one section of it, was subjected to criticism immediately after it was communicated, and before it was printed, by Kirwan, Mr Cavendish, as he says, having had the politeness to permit him to read it. The criticism was directed not to the question of the formation of water but to that of fixed air in the various processes which Kirwan alleged it to be produced, but which Cavendish was unable to verify. As regards water Kirwan says "when inflammable air from metals and dephlogisticated air are fired, as a great diminution takes place, and yet no fixed air is found, I am nearly convinced, by Mr. Cavendish's experiments that water is really produced"; and he goes

[1] *Essays in Historical Chemistry*, James Watt, p. 120.

on to say that he is not surprised at this fact as he should have expected on *a priori* grounds that this particular method of phlogistication would have produced "a compound very different from that which it forms in other instances of phlogistication"; and he proceeds to develop his reasons with the dialectical skill and specious reasoning characteristic of his nimble intellect. Kirwan's paper is, in fact, a tissue of misstatements, false analogies, and loose reasoning, and is remembered only from the circumstance that it provoked a reply from Cavendish, and incidentally led to the production of his second paper on "Experiments on Air."

Cavendish's reply is characteristic of him. He begins by saying:

> In a paper lately read before this Society containing many experiments on air, I gave my reasons for supposing that the diminution which respirable air suffers by phlogistication, is not owing either to the generation or separation of fixed air from it; but without any arguments of a personal nature, or which related to any one person who espouses the contrary doctrine more than to another. This being contrary to the opinion maintained by Mr. Kirwan, he has written a paper in answer to it which was read on the fifth of February. As I do not like troubling the Society with controversy, I shall take no notice of the arguments used by him, but shall leave them for the reader to form his own judgement of; much less will I endeavour to point out any inconsistencies or false reasonings, should any such have crept into it; but as there are two or three experiments mentioned there, which may perhaps be considered as disagreeing with my opinion, I beg leave to say a few words concerning them.

The two or three experiments are then discussed and Kirwan's inferences from them refuted. The Irish chemist, who was certainly a well-read and accomplished man, a keen critic and remarkably familiar with the chemical literature of his time, both at home and abroad, returned to the attack, but without adducing any fresh facts, and Cavendish therefore took no notice of the rejoinder.

It is not improbable, however, that Kirwan's criticism of his remarks on the action of the electric spark on air, which it must be admitted are not very conclusive, induced him to undertake further inquiry on this matter. His results, which led to the discovery of the true nature of nitric acid, were communicated to the Royal Society in 1785, and are published in the *Phil. Trans.* for that year under the same title as his preceding paper, of which it is professedly a continuation.

He begins by a reference to his previous paper in which he gave reasons for his belief that the diminution produced in atmospheric air by phlogistication was not due to the formation of fixed air. As regards the action of the electric spark on air he admits that, as he had made no experiments himself on that subject, his opinion had been formed on the experiments of others. As the result of further inquiry he now finds that although he was right in supposing the diminution in volume of the air was not due to its phlogistication by the spark, and that no fixed air was formed by its action, the real cause was very different from what he expected, and that it depends upon the conversion of phlogisticated air [nitrogen] into nitrous [nitric] acid.

He then proceeds to give an account of the apparatus he employed to demonstrate this fact. A small quantity of the "air" to be experimented upon was introduced into a Λ-shaped tube of about $\frac{1}{10}$th of an inch in bore filled with mercury, the limbs of which were placed in separate vessels of mercury, the length of the column of the "air" being in general from $\frac{3}{4}$ to $1\frac{1}{2}$ inches. Various solutions, e.g. litmus, lime-water, soap-lees, etc. could be introduced into the two limbs.

When the electric spark was made to pass through common air, included between short columns of a solution of litmus, the solution acquired a red colour, and the air was diminished, conformably to what was observed by Dr. Priestley. When lime-water was used, instead of the solution of litmus, and the spark was continued till the air could be no further diminished, not the least cloud could be perceived in the lime-water; but the air was reduced to two-thirds of its original bulk; which is a greater diminution than it could have suffered by mere phlogistication, as that is very little more than one-fifth of the whole.

By continued passage of the spark Cavendish found that the whole of the lime could be neutralised, after which the free acid in the liquid began to attack the mercury. "When the air is confined by soap-lees [solution of caustic potash] the diminution proceeds rather faster than when it is confined by lime-water." Accordingly in the rest of the trials this solution was employed to absorb the acid produced. In the case of pure oxygen "the diminution was but small": in the case of nitrogen,

no sensible diminution took place; but when five parts of pure dephlogisticated air were mixed with three parts of common air, almost the whole of the air was made to disappear.

It must be considered, that common air consists of one part of dephlogisticated air, mixed with four of phlogisticated; so that a mixture of five parts of pure dephlogisticated air, and three of common air is the same thing as a mixture of seven parts of dephlogisticated air with three of phlogisticated....

As fast as the air was diminished by the electric spark, I continued adding more of the same kind, till no further diminution took place....The soap-lees being then poured out of the tube, and separated from the quicksilver, seemed to be perfectly neutralized, as they did not at all discolour paper tinged with the juice of blue flowers. Being evaporated to dryness, they left a small quantity of salt, which was evidently nitre, as appeared by the manner in which paper, impregnated with a solution of it, burned.

For more satisfaction, I tried this experiment over again on a larger scale.... the spark was continued till no more air could be made to disappear. The liquor when poured out of the tube, smelled evidently of phlogisticated nitrous acid [nitrous acid or nitric oxide], and being evaporated to dryness, yielded $1\frac{4}{10}$ grains of salt, which is pretty exactly equal in weight to the nitre which that amount of soap-lees would have afforded if saturated with nitrous [nitric] acid. This salt was found, by the manner in which paper dipped into a solution of it burned, to be true nitre. It appeared, by the test of *terra ponderosa salita* [barium chloride], to contain not more vitriolic acid than the soap-lees themselves contained, which was excessively little; and there is no reason to think that any other acid entered into it, except the nitrous [nitric].

In testing for the possible formation of hydrochloric acid Cavendish incidentally obtained a precipitate of silver nitrite: it is characteristic of his acute power of observation that he was not misled by it. He says:

A circumstance, however, occurred, which at first seemed to shew, that this salt [the nitre from the soap-lees] contained some marine acid; namely, an evident precipitation took place when a solution of silver was added to some of it dissolved in water; though the soap-lees used in its formation were perfectly free from marine acid, and though, to prevent all danger of any precipitate being formed by an excess of alkali in it, some purified nitrous [nitric] acid had been added to it, previous to the addition of the solution of silver. On consideration, however, I suspected that this precipitation might arise from the nitrous acid in it being phlogisticated; and therefore I tried whether nitre, much phlogisticated, would precipitate silver from its solution. For this purpose I exposed some nitre to the fire, in an earthen retort, till it had yielded a good deal of dephlogisticated air; and then, having dissolved it in water, and added to it some well purified spirit of nitre till it was sensibly acid, in order to be certain that the alkali did not predominate, I dropped into it some solution of silver, which immediately made a very copious precipitate. This solution however being deprived of some of its phlogiston by evaporation to dryness, and exposure for a few weeks to the air, lost the property of precipitating silver from its solution; a proof that this property depended only on its phlogistication, and not on its having absorbed sea-salt from the retort, or by any other means.

Hence it is certain that nitre, when much phlogisticated, is capable of making a precipitate with a solution of silver; and therefore there is no reason to think that the precipitate, which our salt [i.e. the salt formed in the soap-lees] occasioned with a solution of silver, proceeded from any other cause than that of its being phlogisticated; especially as it appeared by the smell, both on first taking it out of the tube, and on the addition of the spirit of nitre, previous to dropping in the solution of silver, that the acid in it was much phlogisticated. This property of phlogisticated nitre is worth the attention of chemists; as otherwise they may sometimes be led into mistakes, in investigating the presence of marine acid by a solution of silver.

No apology is needed for this somewhat lengthy extract. It is significant of the care, patience and skill with which Cavendish followed up and unravelled phenomena which to less cautious operators would have been so many pitfalls. All that is necessary to make the account consistent with modern terminology is to read "abstraction of oxygen" for addition of phlogiston: otherwise it is a perfectly accurate statement of the relation of nitrous acid to nitric acid; of the modes of their synthetical formation;

of the behaviour of nitre when heated; of the instability of the alkaline nitrites; and of the method of producing silver nitrite.

Having thus satisfactorily demonstrated that oxygen and nitrogen under the influence of heat and in presence of an alkali will unite to form nitrites and nitrates, Cavendish proceeds to explain what he considers to be the rationale of their production. As might be anticipated, his theory is obscured by the mists of phlogistonism. He had stated in his previous paper that when nitre is deflagrated with charcoal the acid [nitric acid] is converted into phlogisticated air identical with that contained in our atmosphere;

from which I concluded, that phlogisticated air is nothing else than nitrous acid united to phlogiston. According to this conclusion, phlogisticated air ought to be reduced to nitrous acid by being deprived of its phlogiston. But as dephlogisticated air is only water deprived of phlogiston, it is plain, that adding dephlogisticated air to a body, is equivalent to depriving it of phlogiston, and adding water to it; and therefore phlogisticated air ought also to be reduced to nitrous [nitric] acid, by being made to unite to, or form a chemical combination with, dephlogisticated air; only the acid formed this way will be more dilute, than if the phlogisticated air was simply deprived of phlogiston.

This inverted method of representing the facts is characteristic of the logic of phlogistonism. Cavendish implies that nitrogen is a compound of nitric acid and phlogiston, and as, according to him, phlogiston is a hydrate of inflammable air, it follows that in modern terminology nitrogen should be regarded as a *hydrated hydrogen nitrate*. When therefore oxygen acts upon this combination, it combines with the phlogiston forming water and liberating and then diluting the nitrous [nitric] acid. He then recapitulates the facts of his experiments already stated and interprets them in the light of his theory, and in like manner explains the formation of the nitric acid observed in the course of his observations on the synthesis of water.

He next reviews the properties of phlogisticated air as it exists in the atmosphere, noting their negative character, and then raises the question "whether there are not in reality many different substances confounded together by us under the name of phlogisticated air." This latter point has already been noted by the late Lord Rayleigh; it is of such special interest in the light of subsequent work that the passage merits quotation. Cavendish thus describes how he proceeds to find an answer to his query:

I therefore made an experiment to determine, whether the whole of a given portion of the phlogisticated air of the atmosphere could be reduced to nitrous acid, or whether there was not a part of a different nature from the rest, which would refuse to undergo that change. The foregoing experiments indeed in some measure decided this point, as much the greatest part of the air let up into the tube lost its elasticity; yet, as some remained unabsorbed, it did not appear for certain whether that was of the same nature as the rest or not. For this purpose

I diminished a similar mixture of dephlogisticated and common air, in the same manner as before, till it was reduced to a small part of its original bulk. I then, in order to decompound as much as I could of the phlogisticated air which remained in the tube, added some dephlogisticated air to it, and continued the spark till no further diminution took place. Having by these means condensed as much as I could of the phlogisticated air, I let up some solution of liver of sulphur to absorb the dephlogisticated air; after which only a small bubble of air remained unabsorbed, which certainly was not more than $\frac{1}{120}$ of the bulk of the phlogisticated air let up into the tube; so that if there is any part of the phlogisticated air of our atmosphere which differs from the rest, and cannot be reduced to nitrous acid, we may safely conclude, that it is not more than $\frac{1}{120}$ part of the whole.

No doubt, according to Newton's second rule that "to natural effects of the same kind the same causes are to be assigned, as far as it may be done," Cavendish might be warranted in concluding that the phlogisticated air of the atmosphere is of uniform character with the exception of at least the $\frac{1}{120}$th part. At the same time the conclusion implicitly assumes the absence of forms of phlogisticated air which might equally have the power to unite with oxygen under the conditions of the experiment.

The late Lord Rayleigh, as is well known, repeated the Cavendish experiment on a large scale and showed that the "small bubble" must have consisted substantially of argon, doubtless mixed with the other inert gases of the atmosphere subsequently discovered by Sir William Ramsay.

Cavendish then reverts to his original supposition that in the presence of "inflammable" [combustible or organic] matter "some of this matter might be burned by the spark, and thereby diminish the air." Oxygen confined over distilled water, soap-lees and litmus solution was therefore "sparked," but only a very slight diminution was observed, due probably to air in the solutions or to the formation of ozone. In the case of the litmus "the solution soon acquired a red colour, which became paler and paler as the spark was continued, till at last it was quite colourless and transparent." Fixed air was formed, as shown by lime-water becoming cloudy.

In this experiment therefore the litmus was, if not burnt, at least decompounded, so as to lose entirely its purple colour, and to yield fixed air...and so very likely might the solutions of many other combustible [organic] substances. But there is nothing, in any of these experiments, which favours the opinion of the air being at all diminished by means of phlogiston communicated to it by the electric spark.

There is also nothing in Cavendish's electrical researches to show that he ever associated phlogiston with electricity, and although he attempted to explain some of the principal phenomena of electricity by the assumption of an elastic fluid, no mention of phlogiston occurs in his memoirs, nor can we gather how he supposed that phlogiston could be communicated to oxygen by means of the electric spark. We are again met with the ever-recurrent difficulty: what exactly was Cavendish's conception of phlogiston?

This paper, as throwing light upon the true nature of nitric acid, and its relations to oxygen and nitrogen, naturally attracted considerable attention, and became the subject of further inquiry. Other observers "of distinguished ability" in attempting to repeat Cavendish's experiment were not equally successful, and accordingly he "thought it right to take some measures to authenticate the truth of it." He therefore requested Mr Gilpin, Clerk to the Royal Society, to repeat the experiment "in presence of some of the Gentlemen most conversant with these subjects." It appeared that the chemists who had endeavoured to repeat the synthesis of the nitric acid were Van Marum and Paets Van Trootswyk in Holland; and Lavoisier, Hassenfratz and Monge in France. "I am not acquainted," says Cavendish, "with the method which the three latter Gentlemen employed, and am at a loss to conceive what could prevent such able philosophers from succeeding, except want of patience."

The details of the repetition are set forth in a paper read to the Royal Society on April 17th, 1788, and published in the *Phil. Trans.* for that year under the title "On the Conversion of a Mixture of dephlogisticated and phlogisticated Air into nitrous Acid, by the Electric Spark." We learn from this paper that the electrical machine employed by Cavendish

was one of Mr. Nairne's patent machines, the cylinder of which is 12½ inches long, and 7 in diameter. A conductor of 5 feet long, and 6 inches in diameter, was adapted to it, and the ball which received the spark was placed at two or three inches from another ball, fixed to the end of the conductor. Now when the machine worked well, Mr. Gilpin supposes he got about two or three hundred sparks a minute, and the diminution of the air during the half hour which he continued working at a time, varied in general from 40 to 120 measures, but was usually greatest when there was most air in the tube, provided the quantity was not so great as to prevent the spark from passing readily.

The experiment was repeated twice and the formation of nitric acid was fully confirmed. As Van Marum had described the details of his method Cavendish was able to point out the cause of his want of success. A diminution in volume of the included gas was actually observed by him, but the alkali was only imperfectly neutralised and the touch-paper prepared from it was not sufficiently quick-burning. The experiment has been frequently repeated and with simpler apparatus. Faraday showed that if paper moistened with caustic potash solution be suspended between two brass balls from which a spark discharge is passing, nitre is rapidly formed and the paper becomes touch-paper.

It is not necessary to dwell upon the importance of Cavendish's discovery simply as a scientific fact, or to point out the manifold results which have flowed from it in connection with the natural occurrence of nitrates, and the nutrition of plants. His method of synthesis is to-day the basis of an industry which bids fair to revolutionise large and important departments of chemical and agricultural procedure.

This paper constitutes the last of Cavendish's published chemical researches. In point of time they range from 1766 to 1788. The Chatsworth manuscripts contain results of other chemical inquiries which will be referred to later.

We now proceed to give some account of his published labours in other departments of science with the exception of his electrical researches which have already been fully dealt with by Professor Clerk Maxwell.

As already stated, Cavendish, as regards science, was a remarkably many-sided man. Practically every field of scientific inquiry opened up in his time attracted him, and he carried on more or less simultaneously investigations in very different branches. As his published work in departments other than electricity and chemistry does not allow itself to be conveniently classified, it may be desirable to treat of these papers in the order of their appearance.

Cavendish's memorable paper on electricity, published in the *Phil. Trans.* for 1771, established his position as an authority on that branch of physical science, and no doubt led to his inclusion on a committee appointed by the Royal Society at the request of the Board of Ordnance, to consider the best method of protecting the powder magazine at Purfleet from lightning. This matter has already been dealt with by Professor Clerk Maxwell in the earlier volume of this work and need not therefore be further referred to.

During the latter half of the eighteenth century meteorological observations began to attract an increasing amount of attention and the early volumes of the *Phil. Trans.* contain numerous communications on the subject. In 1773 the Royal Society instituted under Cavendish's superintendence and direction systematic and regular observations on atmospheric temperature, pressure, humidity, rain-fall, and wind, as well as on magnetic variation and inclination, at their house in Crane Court and subsequently at Somerset Place. These records were tabulated and discussed in successive volumes of the *Transactions* down to 1843 when on the recommendation of the Council of the Society, the Government established a meteorological and magnetic observatory in association with the Royal Observatory at Greenwich.

A couple of years after their installation in Crane Court the Council of the Society requested Cavendish to examine the condition and mode of working of the instruments, and his report was published in the *Phil. Trans.* for 1776 under the title of "An Account of the Meteorological Instruments used at the Royal Society's House." It contains a description of the thermometers, barometer, rain-gauge, hygrometer, variation-compass and dipping-needle.

Cavendish, like his father, had paid considerable attention to thermometry and he took advantage of the opportunity afforded by the report to indicate certain sources of error in the mode of graduation of the thermometer and in the manner of its use. He was the first to draw attention to the necessity of correcting for the emergent column; that is, for the portion of mercury in the stem not heated to the temperature it is desired to ascertain. He points out that a thermometer,

dipped into a liquor of the heat of boiling-water, will stand at least 2° higher, if it is immersed to such a depth that the quicksilver in the tube is heated to the same degree as that in the ball, than if it is immersed no lower than the freezing-point, and the rest of the tube is not much warmer than the air. The only accurate method is, to take care that all parts of the quicksilver should be heated equally. For this reason, in trying the heat of liquor much hotter or colder than the air, the thermometer ought, if possible, to be immersed almost as far as to the top of the column of quicksilver in the tube.

But as this procedure is not always practicable, Cavendish gives a table showing the amount to be added or subtracted for the number of degrees not immersed "owing to the supposed difference of heat of the quicksilver in that part of the tube and in the ball," based on the assumption "that quicksilver expands one 11500th part of its bulk by each degree [F.] of heat."

This table has long since been superseded by others based upon similar principles, but even subsequent tables, although more accurate, are affected, like that of Cavendish, by uncertainty as to the true temperature of the mercury in the emergent column.

In spirit thermometers, as he points out, the error of the emergent column may be much greater owing to the greater expansibility by heat of spirits of wine.

He indicates the necessity of immersing the whole of the mercury in the steam from boiling water when determining the upper fixed point of a thermometer and describes a simple apparatus for this purpose.

"At present," he says, "there is so little uniformity observed in the manner of adjusting thermometers, that the boiling-point, in instruments made by our best artists, differ from each other by not less than 2½°; owing partly to a difference in the height of the barometer at which they were adjusted, and partly to the quicksilver in the tube being more heated in the method used by some persons, than in that used by others. It is very much to be wished therefore, that some means were used to establish a uniform method of proceeding; and there are none which seem more proper, or more likely to be effectual, than that the Royal Society should take it into consideration, and recommend that method of proceeding which shall appear to them to be most expedient."

The barometer in use by the Royal Society was of the "cistern kind," and Cavendish discusses the best method of reading it, and he states his reasons for preferring it to the syphon barometer. He gives a table of corrections for capillary depression depending upon the diameter of the tube, based upon observations made by his father, Lord Charles Cavendish, which as Professor Clerk Maxwell has pointed out "have furnished the basis not only for the correction of the reading of barometers, etc. but for the verification of the theory of capillary action by Young, Laplace, Poisson and Ivory." From notes and memoranda to be found among his papers it would appear that the actual measurements made to determine the degree of depression were made by Cavendish himself.

The results of the barometric observations were published by the Society as monthly means. It would seem to have been the practice of the observer to reduce each observation, taken twice a day, to the standard temperature, and to take the mean of the whole. Cavendish points out that

it is sufficient to take the mean height of the barometer, and correct that according to the mean heat of the thermometer; the result will be exactly the same as if each observation had been corrected separately, and a mean of the corrected observations taken.

Cavendish seems to have been satisfied with the character and position of the "rain-gage," and notes that the strength of the wind is registered as "gentle, brisk, and violent or stormy, which are distinguished by the figures 1, 2, and 3. When there is no sensible wind, it is distinguished by a cypher." Observations of humidity were made by Smeaton's hygrometer (*Phil. Trans.* 61. 198), depending upon the variations in length of a stretched string.

The construction and mode of use of the variation-compass and dipping-needle are discussed at considerable length, and the sources of error and methods of eliminating them are set out in detail. Cavendish would appear to have been familiar with magnetic observations of this kind, and to have assisted his father in making and recording them. The variation-compass was constructed by Nairne after the pattern designed by Knight with certain modifications introduced by Lord Charles Cavendish and Sisson. An attempt was made to determine the error due to local attraction in the Society's house by comparison with an instrument at Great Marlborough Street, presumably belonging to Lord Charles Cavendish.

The dipping-needle was made by Nairne "after a plan of the Rev. Mr. Michell, F.R.S., Rector of Thornhill[1]" and is described in *Phil. Trans.* 1772, 62. 476. The method of observation was precisely the same as that in use to-day, viz. reading the needle east and west and reversing the poles. Cavendish discusses the theory of this procedure and points out its practical limitations due to mechanical difficulties in construction, more particularly to the ends of the axis not being truly cylindrical—an imperfection not infrequently still met with in modern dipping-needles. In his report he gives the result of comparisons between needles of different construction, made

[1] Cf. the short memoir of Michell by Sir A. Geikie, Camb. Univ. Press, 1918

in the garden of his house in Great Marlborough Street, "partly with a view to determine the true dip at this time [1775] in a place out of reach of the influence of any iron work, and partly to see how nearly different needles would agree."

This report to the Society displays Cavendish at his best. It reveals the range of his knowledge, his painstaking care, his sense of accuracy, his perspicacity and the thoroughness with which he studied any problem he attacked. Instruments designed for measurement had always a special attraction for him, and he seems to have taken a peculiar pleasure in working with them and in studying their behaviour with a view to getting the best results out of them. He would appear to have had no great interest in the technical side of invention; at all events his name is not now associated with any instrument of precision, or any formal piece of apparatus now in use[1]. His main concern seemed to be to make such instruments as he could construct out of rough material, or as the "artists" of his day provided, serve his purpose by skilful and intelligent use. With him it was a case of "the man behind the gun."

His suggestion that the Royal Society should take steps to standardise the method of determining the fixed-points of mercurial thermometers led the Council to appoint a committee consisting of himself, Dr Heberden, Mr Alex. Aubert, Dr Deluc, Rev. Nevil Maskelyne, Dr Horsley and Mr Planta to consider further and advise them on the matter. The report, which was presented in 1777 was published in the *Phil. Trans.* 67. 816. The committee adopted substantially Cavendish's methods of ascertaining the upper fixed point, after a careful experimental inquiry, the details of which are fully described and the notes of which are still preserved among his papers. In regard to the correction for the emergent column, it is suggested that its mean temperature may be ascertained with sufficient accuracy by attaching a second thermometer to the stem, in the manner still practised. The standard atmospheric pressure adopted is 29·8 inches, and full instructions are given as to the corrections to be applied to the observed boiling-point when the barometer differs from this height. The report, which seems to have been largely drawn up by Cavendish,—considerable sections of it in his own handwriting are to be found among his MSS. papers,—is a notable contribution to thermometry; it exercised an immediate influence on the construction and use of the mercurial thermometer and incidentally on the development of the science of heat.

Cavendish's interest in mercury as a thermometric agent was doubtless the reason that induced him to occupy himself with the question of its solidification. That mercury could be frozen was first clearly demonstrated

[1] As might be supposed, Cavendish paid considerable attention to the improvement of the chemical balance and he possessed good instruments of Ramsden's construction. We owe to him the first suggestion to use agate planes for the bearing of the central and terminal knife-edges.

in 1759 by Braun of St Petersburg who effected its solidification by means of a freezing-mixture of snow and nitric acid, when he "obtained a solid, shining metallic mass, which extended under the strokes of a pestle; in hardness rather inferior to lead, and yielding a dull dead sound like that metal." This observation excited great interest, mercury being regarded from the time of the alchemists as a substance of quite peculiar properties, and possessed in a preeminent degree of the "essential principle of fluidity." At the instance of the Royal Society, Braun's experiments were repeated and confirmed in 1775 by Mr Thomas Hutchins, the Governor of Albany Fort, in Hudson's Bay, but the most exaggerated estimates of the degree of cold necessary for the solidification of the metal were current. The proper method of ascertaining the freezing-point was pointed out, independently, by Black and Cavendish, the latter of whom furnished Hutchins with thermometers and a simple apparatus in which the experiment might be repeated, and the temperature of solidification accurately determined. The second series of experiments were made in 1781–1782 and the results were communicated to the Royal Society in 1783 and are published in the *Phil. Trans.* 73. 303. They were commented upon by Cavendish in a paper published in the same volume, entitled "Observations on Mr. Hutchins's Experiments for determining the Degree of Cold at which Quicksilver freezes." He begins by explaining the apparatus he suggested, and which was employed by Hutchins.

It consisted of a small mercurial thermometer, the bulb of which reached about 2½ inches below the scale, and was inclosed in a glass cylinder swelled at bottom into a ball, which, when used, was filled with quicksilver, so that the bulb of the thermometer was intirely surrounded with it. If this cylinder is immersed in a freezing mixture till great part of the quicksilver in it is frozen, it is evident, that the degree shewn at that time by the inclosed thermometer, is the precise point at which mercury freezes.

Cavendish then points out the fallacy in the preceding attempts in which the degree of cold was estimated by noting the degree of contraction in the thermometer itself containing the frozen mercury, as no account was taken of the diminution in volume which the mercury suffers in passing from the liquid to the solid state. He compares the phenomena of freezing mercury with those observed on the solidification of water and of melted tin or lead. He draws attention to the super-cooling of water, explains the rise of temperature at the moment of solidification and points out its natural effect. His explanation agrees with that of Black;

only instead of using the expression, heat is generated or produced, he [Black] says, latent heat is evolved or set free; but as this expression relates to an hypothesis depending on the supposition, that the heat of bodies is owing to their containing more or less of a substance called the matter of heat; and as I think Sir Isaac Newton's opinion, that heat consists in the internal motion of the particles of bodies, much the most probable, I chose to use the expression, heat is generated.

Cavendish then discusses *seriatim* the accounts furnished by Hutchins of his several experiments; and after correcting the results so as to accord with a thermometer

adjusted in the manner recommended by the Committee of the Royal Society, it follows, that all the experiments agree in shewing that the true point at which quicksilver freezes is $38\frac{2}{3}°$, or in whole numbers 39° [F.] below 0[1].

With regard to the contraction of mercury in freezing, Cavendish, after discussing certain observations by Hutchins and Braun, concludes that it is "almost $\frac{1}{23}$ of its whole bulk," which agrees with the value deduced from Mallet's determination of its specific gravity at its melting point (*Proc. Roy. Soc.* 1877, 26. 71).

The remainder of the paper consists of a discussion on the cold produced by the freezing mixture of snow and nitric acid employed. This he says is owing to the melting and solution of the snow in the acid.

Now, in all probability, there is a certain degree of cold in which the spirit of nitre, so far from dissolving snow, will yield out part of its own water, and suffer that to freeze, as is the case with solutions of common salt; so that if the cold of the materials before mixing be equal to this, no additional cold can be produced. If the cold of the materials is less, some increase of cold will be produced; but the total cold will be less than in the former case, since the additional cold cannot be generated without some of the snow being dissolved, and thereby weakening the acid, and making it less able to dissolve more snow; but yet the less the cold of the materials is, the greater will be the additional cold produced....

However extraordinary it may at first appear, there is the utmost reason to think, that a rather greater degree of cold would have been obtained if the spirit of nitre had been weaker.

[1] The following are recorded determinations of the freezing point of mercury (*Science Abstracts*, 20, 11, 1917, 58):

Authority	Date	Thermometer	Value °C.
Hutchins	1776		− 39·44
Cavendish	1783		− 39·26
Regnault	1862		− 38·50
B. Stewart	1863	Gas thermometer	− 38·85
Vicentini and Omodei	1888	Mercury thermometer previously compared with the gas thermometer	− 38·80
Chappuis	1896		− 38·85
Chree	1898		− 38·86
Henning	1914	Platinum thermometer	− 38·89
Wilhelm	1916	Resistance thermometer	− 38·87

As Blagden states in his "History of the Congelation of Quicksilver" (*Phil. Trans.* 73. 1783, 329) Cavendish was the first to effect its solidification in England, at his house at Hampstead, on February 26th, 1783 by a freezing mixture of pounded ice or snow and dilute nitric acid.

He points out that strong nitric acid generates heat by combining with water and that it is only when a certain amount of water has been added that this generation of heat ceases, when the addition of snow produces cold. The amount of water which needs to be added to the strong acid before the generation of heat ceases was found by Cavendish to be about $\frac{1}{4}$ the weight of the acid, which points to the formation of a monohydrate: this when mixed with snow appears to form the most effective of the freezing mixtures afforded by mixtures of this acid and snow.

This paper was followed by a lengthy communication from Blagden on a "History of the Congelation of Quicksilver" (*Phil. Trans.* 73. 329) which gives an account of all the observations on the subject previously published, with an examination of the statements concerning extraordinary low temperatures which had been recorded by travellers and others in Siberia, Lapland and elsewhere, and which he shows to be fallacious on grounds stated by Cavendish. These papers taken together are of importance as refuting the exaggerated conceptions of the intensity of cold in the neighbourhood of the polar regions and as putting an end to erroneous speculations concerning the influence of low temperatures on animal and vegetable life.

Cavendish's interest in the subject of freezing mixtures, and in the theory of their action, induced him to institute a further series of experiments at Hudson's Bay with the assistance of Mr John McNab, to whom he sent solutions of nitric and sulphuric acids of various strengths, as well as of ordinary alcohol, with "accurately adjusted" thermometers, with instructions for their use. The results of the observations were communicated by Cavendish to the Royal Society in 1786 and are published in the *Phil. Trans.* for that year (Vol. 76, p. 241) under the title "An Account of Experiments made by Mr John McNab, at Henley House, Hudson's Bay, relating to freezing Mixtures."

In connection with these experiments Cavendish furnishes a table showing the specific gravities and corresponding strengths of the acids which were to be employed, the specific gravities being taken at 60° F., compared with water at the same temperature, and the strengths ascertained by determining the weight of marble which 1 part by weight of the acid would dissolve. The numbers he gives enable us to gain an idea of the accuracy with which he worked. Spirit of nitre of the specific gravity he states, viz. 1·4371 at 60° F., would contain 73·5 per cent. of real nitric acid and 1 part of it by weight would dissolve 0·583 parts of pure marble: Cavendish found 0·582. Similarly, spirit of nitre of specific gravity 1·4043 at 60°/60° F. would contain 66 per cent. of nitric acid, and 1 part by weight would dissolve 0·524 parts of marble: Cavendish found 0·525. Strong oil of vitriol of specific gravity 1·8437 at 60°/60° F. would contain 97·35 per cent. H_2SO_4, 1 part by weight of which would be theoretically capable of dissolving 0·990 parts of marble: Cavendish found indirectly 0·98. These numbers are a striking exemplification of the care, patience and manipulative skill which he spent upon all quantitative determinations.

Cavendish had a two-fold object in instituting these experiments. It appeared from some observations of Fahrenheit, who did a considerable amount of work on freezing mixtures, that nitric acid could be frozen, and that the frozen acid when mixed with ice produced cold, a result confirmed by Braun. It seemed doubtful, however, "whether it was the whole acid, or only the watery part, which froze," and to clear up this point Cavendish "desired Mr. McNab to expose it to the cold, and if it froze, to ascertain the temperature, and decant the fluid part into another bottle, and send both home to be examined." Similar experiments were to be made with the solutions of sulphuric acid and spirits of wine. His second object was to ascertain whether by proceeding as he directed, "a greater degree of cold might be produced than had been done hitherto."

In the experiments with the spirit of nitre it was found that this acid was "capable of a kind of congelation, in which the whole, and not merely the watery part, freezes." The freezing point "also differs greatly according to the strength and varies according to a very unexpected law." The acid, like water, may be supercooled without solidification: white crystals are formed on solidification *which are heavier than the still liquid portion.*

The difference indeed is so great, that in one case where it froze into solid crystals on the surface, these crystals, when detached by agitation, fell with force enough to make a tinkling noise against the bottom of the glass....It is this contraction of the acid in freezing which makes the frozen part subside in the fluid part; as it was found, in the undiluted acid, that the latter [the fluid part] consisted of a stronger and consequently heavier acid than the former [the frozen part]. But still the subsidence of the frozen part shows that the ice [the frozen part] is not mere water, or even a very dilute acid; which indeed was proved by the examination of the liquors sent home.

Neglecting the observations with the "dephlogisticated spirit of nitre," owing to the uncertainty as to its real composition, and confining our attention to the "common spirit of nitre" the results are thus summarised by Cavendish:

	Strength	Freezing point
Common spirit of nitre	0·54	$-31\frac{1}{2}°$ F. ($-35·3°$ C.)
" " "	0·411	$-1\frac{1}{2}°$ F. ($-18·6°$ C.)

The first corresponds fairly closely as regards strength and freezing point with the acid $N(OH)_5$ or $HNO_3 . 2H_2O$, which according to Erdmann (*Zeitsch. Anorg. Chem.* 1902, 32. 431) crystallises in needles melting at $-35°$ C. The strength of the acid does not correspond with that of the monohydrate which is said to freeze at $-38°$ C. The second would seem to be identical with the trihydrate $HNO_3 . 3H_2O$, which according to Pickering (*Chem. Soc. Trans.* 1893, 63. 436) melts at $-18·2°$ C. and according to Küster and Kremann (*Zeitsch. anorg. Chem.* 1904, 41. 1) at $-18·5°$ C.

The trihydrate may be formed by adding snow little by little to the cooled acid so long as a rise of temperature is noted. The maximum point observed was $-1\frac{1}{4}$° F. [$-18{\cdot}5$° C.].

The snow did not appear to dissolve, but formed thin white cakes, which however did not float on the surface, but fell to the bottom, and when broke by the spatula formed a gritty sediment; so that it appears, that these cakes are not simply undissolved snow, but that the adjoining acid absorbed so much of the snow in contact with it as to become diluted sufficiently to freeze with that degree of cold and then congealed into these cakes. The quantity of congealed matter seems to have kept increasing till the end of the experiment.

From the minute description he gives "of the phenomena observed on mixing snow with the acid" there can be no doubt that he also obtained the cryohydrate Ice + $HNO_3 . 3H_2O$, or $HNO_3 . 3H_2O + HNO_3 . H_2O$, the melting points of which (-43° C. and -42° C.) agree closely with that noted by him, viz. $-45\frac{1}{4}$° F. ($-42{\cdot}9$° C.).

From these experiments it appears that spirit of nitre is subject to two kinds of congelation, which we may call the aqueous and spirituous; as in the first it is chiefly, if not intirely, the watery part which freezes, and in the latter the spirit itself. Accordingly, when the spirit is cooled to the point of aqueous congelation, it has no tendency to dissolve snow and produce cold thereby, but on the contrary is disposed to part with its own water; whereas its tendency to dissolve snow and produce cold, is by no means destroyed by being cooled to the point of spirituous congelation, or even by being actually congealed. When the acid is excessively dilute, the point of aqueous congelation must necessarily be very little below that of freezing water; when the strength is ·21 it is at -17°, and at the strength of ·243 it seems from Art. 16 to be at $-44°\frac{1}{4}$. Spirit of nitre, of the foregoing degrees of strength, is liable only to the aqueous congelation, and it is only in greater strengths that the spirituous congelation can take place. This seems to be performed with the least degree of cold, when the strength is ·411 in which case the freezing point is at $-1°\frac{1}{2}$. When the acid is either stronger or weaker, it requires a greater degree of cold; and in both cases the frozen part seems to approach nearer to the strength of ·411 than the unfrozen part; it certainly does so when the strength is greater than ·411, and there is little doubt but what it does so in the other case. At the strength of ·54, the point of spirituous congelation is $-31°\frac{1}{2}$ and at ·33 probably $-45°\frac{1}{4}$; at least one kind of congelation takes place at that point, and there is little doubt but that it is of the spirituous kind. In order to present this matter more at one view, I have added the following table of the freezing point of common spirit of nitre answering to different strengths:

Strength	Freezing point	
·54	$-31\frac{1}{2}$° F.	spirituous congelation
·411	$-1\frac{1}{2}$[1]	spirituous congelation
·38	$-45\frac{1}{4}$	spirituous congelation
·243	$-44\frac{1}{4}$	aqueous congelation
·21	-17	aqueous congelation

[1] The point of easiest freezing.

From the conditions under which these experiments were made they are necessarily not of a very high degree of accuracy; thus Cavendish had to calculate the degree of dilution of the acid in one or two cases from the weight of snow Mr McNab added, and this obviously could only be approximately known. It seemed however worth while to compare the results with those obtained independently by Pickering and Küster and Kremann, representing them in the form of curves so far as the observations are comparable. The general character of the curves is strikingly similar. An examination of the *Phil. Trans.* paper of 1786 and of the subsequent one in 1788 leaves little room for doubt that Cavendish was actually the first to indicate the existence of these particular hydrates of nitric acid.

Observations were then made upon the vitriolic acid. Strong oil of vitriol (sp. gr. 1·8437 at 60° F. = 97 per cent. strength) froze "to the colour and consistence of hog's-lard," contracting on solidification. It was not completely melted until the temperature rose to 20° F. (− 6·6° C.). According to Pictet and Knietsch pure (100 per cent.) H_2SO_4 melts at 10° C. The difference is due to the slight quantity of water in Cavendish's acid. He points out, as already observed by the Duc d'Ayen and De Morveau, that sulphuric acid "freezes with a less degree of cold when strong than when much diluted." Nevertheless

it is not certain whether it has any point of easiest freezing, like spirit of nitre, or whether the cold required to freeze it does not continually diminish as the strength increases, without limitation; but the latter opinion is the most probable.

Cavendish's "points of easiest freezing" correspond to the points of inflection in the curves shown on plotting his results. As will be seen subsequently he found reason to modify this opinion: further experiments showed that

oil of vitriol has not only a strength of easiest freezing, but that at a strength superior to this, has another point of contrary flexure [the expression is Cavendish's], beyond which, if the strength be increased, the cold necessary to freeze it again begins to diminish.

He seems to have suspected that the observations in the present paper might possibly be affected by the formation of what was known as glacial oil of vitriol (Nordhausen acid).

It appears also, both from Art. 21 and from M. De Morveau's experiment, that during the congelation of the oil of vitriol, some separation of its parts takes place, so that the congealed part differs in some respect from the rest, in consequence of which it freezes with a less degree of cold; and as there is reason to think from Art. 21 that these two parts do not differ much in strength, it seems as if the difference between them depended on some less obvious quality, and probably on that, whatever it is, which forms the difference between glacial and common oil of vitriol. The oil of vitriol prepared from green vitriol, has sometimes been obtained in such a state as to remain constantly congealed, except when exposed to a heat considerably greater than that of the atmosphere, whence it acquired its name of *glacial*. It is not known indeed upon what this property depends, but it is certainly something else than its strength; for oil of vitriol of this kind is always smoking, and the fumes it emits are particularly oppressive and suffocating, though very different from those of the volatile sulphureous acid [sulphur dioxide]. On rectification likewise it yields, with the gentlest heat, a peculiar concrete substance, in the form of saline crystals [sulphur trioxide]; and after this volatile part has been driven off, the remainder is no longer smoking, and has lost its glacial character.

The mixture of oil of vitriol and spirit of nitre when mixed with snow was found to offer no advantages over oil of vitriol alone and no phenomena of importance were noticed concerning it. Cavendish contents himself with the remark: "as the Society will most likely have less curiosity about the disposition to freeze of this mixture than of the simple acids, I shall spare the particulars."

Nor did the experiments with spirits of wine afford any very definite information. The snow seemed to be dissolved much less readily by spirits of wine than by nitric and sulphuric acids and no great degree of cold could be produced by its addition.

Cavendish was well aware that several questions might be raised concerning which his experiments afford no adequate answer. "But," he says, "as this would lead me into disquisitions of considerable length, without my being able to say anything very satisfactory on the subject, I shall forbear entering into it."

Nevertheless he was not content to remain satisfied with his work and Mr McNab was commissioned to institute further experiments.

As some of these properties were deduced from reasoning not sufficiently easy to strike the generality of readers with much conviction, Mr. McNab was desired to try some more experiments to ascertain the truth of it.

Fresh samples of acids of different strengths were sent out to him with a new set of instructions.

He was desired to expose each of these liquors to the cold till they froze; then to try their temperature by a thermometer; afterwards to keep them in a warm room till the ice [the solid portion] was almost melted, and then again expose them to the cold, and when a considerable part of the acid had frozen, to try the temperature a second time; then to decant the unfrozen part into another bottle, and send both parts back to England, that their strength might be examined.... The intent of decanting the fluid part, and sending both parts back, that their strength might be determined, was partly to examine the truth of the supposition laid down in my former Paper, that the strength of the frozen part approaches nearer to ·411 than that of the unfrozen; but it is also a necessary step towards determining the freezing point answering to a given strength of the acid; for as the frozen part is commonly of a different strength from the unfrozen, the strength of the fluid part, and the cold necessary to make it freeze, is continually altering during the progress of the congelation. In consequence of this, the temperature of the liquor is not that with which the frozen part congealed; but it is that necessary to make the remainder, or the fluid part, begin to freeze, or, in other words, it is the freezing point of the fluid part. This is the reason that a thermometer, placed in spirit of nitre, continually sinks during the progress of congelation; which is contrary to what is observed in pure water, and other fluids in which no separation of parts is produced by freezing.

The results of this second series of observations were communicated by Cavendish to the Royal Society in 1788 and are printed in *Phil. Trans.* 78. 166 under the title of "An Account of Experiments made by Mr. John McNab, at Albany Fort, Hudson's Bay, relative to the Freezing of Nitrous and Vitriolic Acids." From the results of these observations Cavendish deduces the following table showing the freezing point of aqueous solutions of nitric acid of various strengths:

Strength	Freezing point
·561	− 41·6° F.
·445	− 3·8
·390	− 4
·353	− 11
·343	− 13·8
·310	− 23
·276	− 40·3

By interpolation from these data, according to Newton's method (*Princip. Math.* Lib. 3, prop. 40, lem. 5) it appears that the strength at which the acid freezes with the least cold is ·418, and that the freezing point answering to that strength is $-2\frac{4}{10}°$.... These experiments confirm the truth of the conclusions I drew from Mr. McNab's former experiments; for, first, there is a certain degree of strength at which spirit of nitre freezes with a less degree of cold than when it is either stronger or weaker; and when spirit of nitre, of a different strength from that, is made to congeal, the frozen part approaches nearer to the foregoing degree of strength than the unfrozen. Likewise this strength, as well as the freezing point corresponding thereto, and the freezing point answering to the strength of ·54 come out very nearly the same as I concluded from those experiments; for by the present experiments they come out ·418, $-2\frac{4}{10}°$ and − 31°, and by the former ·411, $-1\frac{1}{2}°$ and − 31°.

If these observations are plotted, the strengths of the acid being expressed in terms of molecular percentages, it will be found that the character of the curve is identical with that, over the same range, representing the values obtained by Pickering (*Chem. Soc. Trans.* 1893, 63. 436) and by Küster and Kremann (*Zeitsch. anorg. Chem.* 1904, 41. 1). The points of "contrary flexure," as Cavendish calls them, occur at substantially the same temperatures and the corresponding strengths are not very dissimilar. Cavendish found the substance crystallising at −18·8° C. to contain, as the mean of the two determinations of strength, 52·3 per cent. of nitric acid. The trihydrate which crystallises at − 18·5° C. contains 53·9 per cent. The coupled hydrate $HNO_3 . 3H_2O + HNO_3 . H_2O$, said to melt at − 42° C., has the same composition as the acid $N(OH)_5$ which according to Erdmann (*Zeitsch. anorg. Chem.* 1902, 32. 431) crystallises in needles melting at − 35° C. and contains 63·6 per cent. HNO_3.

In the interval between the publication of Cavendish's first and second papers on freezing mixtures, the subject of the freezing points of aqueous solutions of sulphuric acid had been attacked by Keir, a chemical manufacturer living near Birmingham, a friend of Priestley, and a Fellow of the Royal Society, who contributed a paper "On the Congelation of the Vitriolic Acid" to the *Phil. Trans.* 77. 267. Keir, from experiments made during the severe frost of 1784–5,

was led to believe that there must be some certain strength at which the vitriolic acid was more disposed to freeze than at any other, greater or less.... I have found that the point of strength most favourable to congelation is very determinate, and that a very small variation above or below that point renders the acid incapable of freezing without a considerable augmentation of cold.

The sulphuric acid of "easiest freezing" was found by Keir to have a density of 1·78, and the freezing and melting points of the acid were identical, viz. 46° F. (7·8° C.).

These numbers agree with those subsequently found by Lunge (*Ber.* 14. 1881, 2649) and by Knietsch (*Ber.* 34. 1901, 4069). Mr McNab's experiments confirmed Keir's observations. From his experiments "it would seem," says Cavendish, "that the freezing point of oil of vitriol, answering to different strengths, is nearly as annexed":

Strength	Freezing point
·977	+ 1° F.
·918	− 26°
·846	+ 42°
·758	− 45°

From hence we may conclude, that oil of vitriol has not only a strength of easiest freezing, as Mr Keir has shown; but that, at a strength superior to this, it has another point of contrary flexure, beyond which, if the strength be increased, the cold necessary to freeze it again begins to diminish.

The strength answering to this latter point of contrary flexure must in all probability be rather more than ·918, as the decanted or unfrozen part of No. 2 seemed rather stronger than the undecanted part; and for a like reason the strength of easiest freezing is rather more than ·846.

As already stated, the "strengths" of the acid solutions express the weight of marble which 1 part by weight of the liquid is theoretically capable of dissolving. As a matter of fact, Cavendish, in the case of sulphuric acid solutions,

> did not find their strength by actually trying how much marble they would dissolve; as that method is too uncertain, on account of the selenite [calcium sulphate] formed in the operation, and which in good measure defends the marble from the action of the acid. The method I used was, to find the weight of the plumbum vitriolatum [lead sulphate] formed by the addition of sugar of lead, and from thence to compute the strength, on the supposition that a quantity of oil of vitriol, sufficient to produce 100 parts of plumbum vitriolatum, will dissolve 33 of marble; as I found by experiment that so much oil of vitriol would saturate as much fixed alkali as a quantity of nitrous acid [nitric acid] sufficient to dissolve 33 of marble.

This estimation by Cavendish that 100 parts of lead sulphate may be produced from as much oil of vitriol as would be equivalent to 33 parts of marble, that is, that 100 of lead sulphate are equivalent to 33 of marble, is perfectly accurate.

We are now in a position to see how far subsequent work confirms Cavendish's observations. Of modern observations the most accurate are probably those of Knietsch (*Ber.* 34. 1901, 4069). The following table contains the results of the comparison, so far as it is applicable:

	Cavendish		Knietsch	
Strength	SO_3 p. c.	M. P.	SO_3 p. c.	M. P.
·977	78·1	− 17°·2 C.	78	− 16°·5 C.
·918	74·5	− 32°·3	74	− 25°
·846	67	+ 6°	67	+ 8°
·758	61	− 42°·8	61	below − 40°

Considering the circumstances, it is nothing short of marvellous that Cavendish should have succeeded in getting results so closely approximating to the truth. He not only determined the points of "easiest freezing" and of "contrary flexure" with precision, but his estimations of the corresponding strengths and temperatures are a remarkable testimony to his skill and accuracy of work, in spite of his limited means and the imperfections of his appliances.

It would appear from the absence of all reference to it on the part of later observers that Cavendish's work on the freezing of aqueous solutions of nitric and sulphuric acid was either unknown to them or that its significance was not recognised.

As regards the reasoning by which Cavendish deduced the "strengths" of his acid solutions, Dr Wilson has already pointed out that he was not only cognisant of what we now know as the "law of constant proportion," and acted upon it, but in the special case cited, he was practically applying also the "law of reciprocal proportion," thus showing that although the principles underlying these laws were not actually formulated, they were clearly recognised by him as at the basis of all quantitative analytical work. If he had only pursued the path of inquiry which this recognition opened up we might not have had to wait twenty years for the promulgation of the new departure we associate with the name and fame of Dalton.

Cavendish was now in his fifty-seventh year. Chemical inquiry continued to interest him, as may be proved by notes among his MSS., but he published no further contributions towards it. It has been surmised that the revolution effected by Lavoisier and his followers repelled him from its further prosecution. There would seem to be no adequate ground for this supposition. Biased as he might be towards Stahl's doctrine, he was not so prejudiced as to neglect the study of chemical phenomena because of its seeming insufficiency to explain them. Such an assumption would be wholly opposed to what we know to be his character as a natural philosopher. At no period of his activity as an experimentalist was his energy entirely absorbed by chemical pursuits. His published work, and still more his unpublished manuscripts, show that meteorology, astronomy, electricity, heat, geology, geodesy, mathematics—in fact nearly every branch of natural science known in his time—in addition to chemistry—attracted him, and he seemed to turn from one to the other with equal zeal as their several problems interested him.

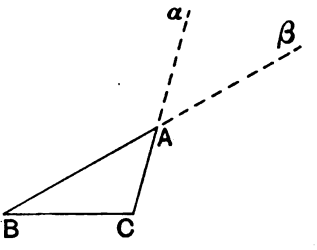
In 1790 he communicated a paper "On the Height of the Luminous Arch that was seen Feb. 23, 1784," to the Royal Society which is printed in *Phil. Trans.* 80. 101. The measurements were based upon observations of an aurora made almost simultaneously by the Rev. F. J. H. Wollaston at Cambridge, the Rev. B. Hutchinson at Kimbolton, and Mr J. Franklin at Blockley in Worcestershire (*Phil. Trans.* 90. 43–46) and communicated to the Royal Society in 1786. It would appear from Professor Loomis's paper in *Silliman's Journ.* 1873 that manifestations of the aurora were particularly frequent at about this period. After discussing and correcting the data, Cavendish calculates from the geographical position of the places of observation that the height of the aurora "could hardly be less than 52 miles, and is not likely to have much exceeded 71." Many like calculations made since Cavendish's time have given similar results, and the estimates of Dalton, Backhouse, H. A. Newton, Alex. S. Herschel and others point to values of the same order as that found by Cavendish. With reference to this paper Dr Chree, F.R.S., of the Kew Observatory, writes as follows:

There is a very full historical discussion of ideas on Aurora by the late Prof. Cleveland Abbe in *Terrestrial Magnetism*, Vol. 3 (1898) in three parts, p. 5, p. 53 and p. 149. The first name he gives is Halley (*Phil. Trans.* 1716) but does not say he calculated heights. The first names he gives as calculators are: Mairan, *Traité de l'Aurore Boreale*, Paris 1731 to 1733; Maier, St Petersburg read 1728, published 1735. He also (p. 12) refers to the work by Cavendish (*Phil. Trans.*

5—2

1790) as quoted by Dr Thomas Young. Abbe again refers on p. 54 to Cavendish in connection with the fact that his views appeared unknown to Dalton when calculating auroral heights in 1791 and 1793.

Cavendish's height seems more in accordance with present ideas than those given by Mairan and Maier. Cavendish's method seems practically that used by Prof. Størmer of Christiania, now the leading authority on the subject. Størmer was the first who managed to photograph auroras. He takes simultaneous photographs from the ends of a base (say 25 kilometres long). The photograph includes stars as well as aurora, and the relative positions of the aurora among the stars from the two ends of the base give the parallax. From end B of the base the aurora A is seen in direction of star β, from the other end C it is seen in direction of star α. The angle $\alpha B\beta$ (or $\alpha C\beta$) is known from astronomical data, and hence the parallax angle BAC. This I fancy is how Cavendish actually worked it out. The only difference is that the apparent star positions of the aurora were observed (and not at absolutely identical times, the *assumption* being made that the arch remained stationary which may not, of course, have been true). Cavendish seems to have had more critical views upon questions of perspective and other uncertainties than his predecessors, and though I should hardly regard the paper as an epoch-making one it seems deserving of attention in any compendious treatment of Cavendish's writings.



Størmer in his earlier work got aurora as low, I think, as 40 kms. In his more recent work he gets a great number of heights of 90 to under 100 kms. and less numerous instances of heights up to several hundred kms. Thus the height Cavendish got is of the same order as the heights Størmer gets as usual for the lower borders of Aurora.

With regard to Professor Abbe's reference to Dalton's supposed ignorance of Cavendish's prior work, we read in the First Edition (1793) of the *Meteorological Essays*, p. 155:

A very moderate skill in optics was sufficient to convince the author [Dalton], that as the luminous beams at all places appear to tend towards one point about the zenith, they must in reality be straight beams, parallel to each other, and nearly perpendicular to the horizon; and from the appearance of their breadth, they must be cylindrical.

To this sentence there is a footnote. "The author did not see before May 1793, the *Philosophical Transactions* for 1790 in which he finds this idea is suggested by H. Cavendish, Esq., F.R.S. and A.S."

The following letter from his younger brother Frederick, found among Cavendish's papers, may be worth reproduction, as evidence of their joint interest in its subject-matter, and as throwing a little light upon their personal relations, referred to in Wilson's *Life*.

Market Street
Wed. Mar. 1st 1780.

Dear Brother,

As I know you observe the Aurora Borealis with much attention, I send you an account of one which appeared last Night, and which in some respects was the most remarkable I have known. It had the most perfect Corona I ever beheld, with Radii streaming down on all sides, and overspreading *the whole* Hemisphere. The Corona was situated almost close to the hinder foot of Ursa Major, very near to the two stars ♏ and ♈; but rather on that side which is nearest to the Stars ψ and β and in Line with them. The Aurora was of a pale colour, tho' I am inform'd that before I observ'd it, the Sky was very Red in the Eastern quarter (as describ'd to me) sometimes in a Flush, and sometimes darting up the Heavens, and I myself occasionally observ'd a Flush of Red, in the West, and in other directions; 'tho it was not the general tenour of its' appearance. It was a little tremulous in its' motion, by no means darting quick, as I have sometimes observ'd it; but varying its Figure sometimes, and sometimes disappearing. The Situation of the Corona was always the same, its' Radii always concentering in the same Point. Sometimes the Space within the Corona was pretty clear; at other times fill'd nearly, with irregular Streams of luminous matter, hurried confusedly together, darting quick, and again instantly disappearing.

It was near 10 o'clock when I first perceiv'd it (it had been observ'd by others, an hour or two sooner) but it disappearing soon after, I did not attend to it, 'till looking at the Thermometer a little before 12 o'clock, I found the Aurora exceeding bright. I accordingly took my Plan of the Stars, in order to determine the precise situation of the Corona. I attended to it for near an hour, and am certain as to the Situation I have describ'd.

Give my Duty to my Father. I hope ye are both in good Health.

I am your affectionate
Brother

FREDERICK CAVENDISH.

Honble Henry Cavendish
at Lord Charles Cavendish's
Great Marlborough Street
London

At the foot of this letter Henry Cavendish has written

$$\left.\begin{array}{l}\text{Altitude}\\ \text{Azim.}\end{array}\right\}\ \text{♈ about}\ \begin{array}{l}72^\circ\\ 11^\circ\ \text{East}\end{array}\ \text{at 12 o'clock.}$$

Alt. ψ about 85°. All 4 Stars nearly in the same vertical circle.

In 1792 Cavendish contributed a short paper to the Royal Society "On the Civil Year of the Hindoos, and its Divisions," which is printed in

Vol. 82, 1792, of the *Philosophical Transactions*. This paper is now, doubtless, only of historical value, but to the biographer of Cavendish it is not without significance as evidence of its author's catholicity and the wide range of his studies and interests. Dr E. Denison Ross, of the School of Oriental Studies, was good enough to consult Dr Barnett of the British Museum concerning its merits as a contribution to our knowledge of Indian chronology. Having regard to the date of its appearance, Dr Barnett writes that on the whole "it marks a distinct advance." There are, he adds, "naturally errors in it,...but he is on the right way, and pursues it intelligently."

In the *Philosophical Transactions* for 1797 appears a paper by Mendoza y Rios (Vol. 87, 1797, p. 43) entitled "Researches on the Chief Problems of Nautical Astronomy. From the French" in which there is given an extract of a letter from Cavendish to the author, dated January, 1795. Concerning this paper, and Cavendish's contribution to it, Sir Frank Dyson the Astronomer Royal reports:

> It seems that in 1805 very elaborate tables were published by Mendoza, the expenses being partly defrayed by Sir Joseph Banks and other Englishmen interested in navigation. In this work no reference is made to Cavendish. As far as I can see he did not use the suggestions put forward by Cavendish, but adopted a wholly different method. Mendoza's work had several editions; we have at the Observatory the one of 1805 and one published at Madrid in 1850. I do not think that Cavendish's method was put to practical use. He advocates what looks at first sight the better method, finding the apparent distance between the sun and a star by a series of corrections to the so-called true distance. But the calculation, owing to refraction and especially parallax, is very complex and the corrections are not very small. Consequently instead of this differential method, Mendoza preferred to calculate the apparent distance directly and his procedure has been generally followed.

In 1798 appeared the famous memoir on "Experiments to determine the Density of the Earth" (*Phil. Trans.* 1798, 88. 469) which, by its appeal to wider interests, greatly enlarged the scientific fame of its author. There is little doubt the subject had been in Cavendish's mind for many years previously. He had taken much interest in Maskelyne's work on Schehallien[1], and, as his unpublished papers show, had furnished memoranda and calculations relating to it, as a member of the "Committee of Attraction" of the Royal Society appointed "to consider of a proper hill on which to try the experiment," to supervise generally the conduct of the inquiry, as well as the expenditure of the royal bounty which defrayed its cost.

[1] "An Account of Observations made on the Mountain Schehallien for finding its Attraction," by the Rev. Nevil Maskelyne, B.D., F.R.S., and Astronomer Royal, *Phil. Trans.* 1775, 65. p. 500.

The principle of the method employed by Cavendish was suggested by his friend the Rev. John Michell, F.R.S., Rector of Thornhill, near Dewsbury, who also contrived an apparatus for carrying it into effect. Michell[1], who had been a Fellow of Queens' College, Cambridge, was fourth in the Tripos List for 1748–49 and was made Woodwardian Professor of Geology in 1762, holding the office for two years. He was elected into the Royal Society in 1760, the same year as Cavendish, and his signature appears in the Charter-book under that of Sir Joshua Reynolds who was made a Fellow in 1761. He was interested in astronomy and geodesy, and made important communications on these subjects to the *Philosophical Transactions*. He was a good geometrician and skilled in the use of instruments, and was the first to suggest the use of Hadley's quadrant in surveying and pilotage. He was also the author of a noteworthy paper on the cause and phenomena of earthquakes (*Phil. Trans.* 1760) and was an acute and skilful geological observer. "In his generalizations, derived in great part from his own observations on the geological structure of Yorkshire, he anticipated many of the views more fully developed by later naturalists." (Lyell, *Principles of Geology*, 7th ed., p. 43.)

The apparatus devised by Michell for determining the mean density of the earth by observing the attraction of small masses of matter was based upon a principle which he had suggested and used as far back as 1768. It was subsequently employed for measuring small attractions and repulsions by Coulomb with whose name it is usually associated. Owing to Michell's other engagements its adaptation for measuring the density of the earth was not completed until a short time before his death and no actual observations were made by him with it.

The arrangement came into the possession of the Rev. Francis John Hyde Wollaston, Jacksonian Professor at Cambridge—one of the observers who furnished Cavendish with data for his computation of the height of the aurora. As Wollaston was unable to make use of it, he transferred it to Cavendish who proceeded to erect it, after very considerable modifications, in an outhouse in his garden at Clapham. The principle of the method consisted in measuring the angle of torsional deflection of a horizontal beam, suspended at the centre by a long thin wire, and provided at each extremity with a small leaden ball, when a much larger ball of the same metal is brought near so as to attract it. The main difficulty of the experiment consists in eliminating the various disturbing factors which interfere with trustworthy measurements of the torsion due to the force of the attracting masses. The sources of disturbance are due, partly to faults inherent in the arrangement of the apparatus, but more especially to the difficulty of securing absolute uniformity in temperature and freedom from air currents. From Cavendish's account of his experiments he would appear to have thought out pretty completely the theory of the method, and to have

[1] See short memoir by Sir A. Geikie, 1918.

striven, so far as his means permitted, to obviate or to correct for such sources of error as he perceived. In all seventeen series of observations were made. From the table giving the results it "appears" says Cavendish

that though the experiments agree pretty well together, yet the difference between them, both in quantity of motion of the arm and in the time of vibration, is greater than can proceed merely from the error of observation. As to the difference in the motion of the arm, it may very well be accounted for, from the current of air produced by the difference of temperature; but whether this can account for the difference in the time of vibration is doubtful. If the current of air was regular, and of the same swiftness in all parts of the vibration of the ball, I think it could not; but as there will most likely be much irregularity in the current, it may very likely be sufficient to account for the difference.

Two different wires were used to suspend the beam.

By a mean of the experiments made with the wire first used, the density of the earth comes out 5·48 times greater than that of water; and by a mean of those made with the latter wire, it comes out the same, and the extreme difference of the results of the 23 observations made with this wire, is only ·75; so that the extreme results do not differ from the mean by more than ·38 or $\frac{1}{14}$ of the whole, and therefore the density should seem to be determined hereby, to great exactness.

The "Cavendish experiment," as it is called, has been frequently repeated, viz. by Reich in 1837 and again in 1852, by Baily in 1841, by Cornu and Baille in 1872, by Boys in 1894, by Braun in 1894, by Eötvös in 1896, and by Burgess in 1901.

It is, of course, possible that Cavendish's method, as, indeed, he points out, may be affected by some error, which, as he says, "may perhaps act always, or commonly, in the same direction," thus making it desirable to use different methods and other instruments. Accordingly attempts have been made to obtain an independent value by means of the chemical balance. This instrument was employed for this purpose by von Jolly of Munich in 1878–80, by Poynting in 1890, by Richarz and Krigar-Menzel in 1898. Wilsing, at Potsdam, has also made a series of observations by causing a pendulum 1 metre in length armed at each end with balls of 540 grams in weight to oscillate between moveable cylinders weighing 325,000 grams. The values obtained by these several experimenters are as follows:

By the torsion-balance:

1798	Cavendish, recalculated by Baily	5·45
1837	Reich	5·49
1841	Baily	5·674
1852	Reich	5·583
1872	Cornu and Baille	5·56
1894	Boys	5·527

1894	Braun	5·527
1896	Eötvös	5·55
1901	Burgess	5·44–5·74

By the chemical balance:

1878–80	von Jolly	5·692
1890	Poynting	5·49
1898	Richarz and Krigar-Menzel ...	5·505

By the pendulum:

1886–88	Wilsing	5·579

Burgess, in a paper published in the *Phys. Rev.* 14. 1902, pp. 257–264, has given a summary of the most trustworthy determinations of the mean density of the earth and calculates that the most probable value is 5·5247, which is remarkably close to the concordant and independent values of Boys and Braun.

Hutton's calculations from Maskelyne's observations of the deviation of the plumb-line at Schehallien had shown that the ratio of the density of the earth to that of the mountain is as 9 to 5. From a lithological examination of the mountain, Playfair concluded that its mean density was between 2·7 and 2·8, which gives about 5 for the mean density of the earth, a result confirmed by the observations, in 1856, of James and Clarke on Arthur's Seat (5·3–5·4), and of Mendenhall on Fusiyama in 1880 (5·77).

Although these observations are of very unequal value, as regards accuracy those obtained by the Cavendish method being the more trustworthy, they are all in remarkable accordance with Newton's "guess" that

> the super-stratum of the Earth being about twice as dense as the water, and the sub-strata becoming, in proportion to their depth, three, four, and even five times more dense, it is probable that the whole mass of the Earth is five or six times more dense than if it were formed of water[1].

The last paper published by Cavendish was "On an improvement in the manner of dividing Astronomical Instruments," printed in the *Phil. Trans.* for 1809, pp. 221–231—the year before his death.

Edward Troughton, the well-known mathematical instrument maker, had communicated to the Royal Society an account of a method of dividing astronomical and other instruments (*Phil. Trans.*, 1809, 105) which attracted considerable attention at the time of its appearance on account of its ingenuity and the excellence of its performance, and as a distinct advance on the methods of Sisson, Bird and Ramsden—all eminent craftsmen in their day as instrument makers. Cavendish, for

[1] For an account of the observations on the determination of the mean density of the earth made prior to 1894, see Poynting's essay on that subject (Adams Prize), London, Griffin & Co. Also C. V. Boys, "La Constante de la gravitation," *Congrès Internationale de Physique*, Paris, 1900.

whom all instruments of precision had a sort of fascination, had evidently studied Troughton's method with care, and in the paper referred to he suggests a method of obviating certain disadvantages in the mode of dividing as at that time practised. Sir Horace Darwin reports concerning this paper:

Cavendish suggests a method which he has not actually tried, but which would seem to be an improvement on the method that appeared to have been used at that time for dividing large circles of astronomical instruments. In his method the circle was first divided into 6 parts by setting a beam compass with the points apart at a distance equal to the radius. These spaces were divided again by the beam compass, sometimes into two equal parts, and sometimes into three and five equal parts, and so on till quite small spaces were left. Errors have to be calculated and allowed for, and the process is most laborious and slow.

I do not think Cavendish's paper is of any practical value to anyone now dividing circles, as they are always made on a dividing engine, which is a method of copying an existing divided circle. But if anyone wished to make an original divided circle he should certainly read Cavendish's paper.

THE ELECTRICAL RESEARCHES

OF THE

HONOURABLE HENRY CAVENDISH, F.R.S.

PHILOSOPHICAL TRANSACTIONS. VOL. 61, 1771, pp. 584–677

An attempt to explain some of the Principal Phænomena of Electricity, by means of an Elastic Fluid

Read Dec. 19, 1771 and Jan. 9, 1772.

{See Table of Contents at the beginning of this volume.}

[PART I.]

1] Since I first wrote the following paper, I find that this way of accounting for the phænomena of electricity is not new. Æpinus, in his *Tentamen Theoriæ Electricitatis et Magnetismi**, has made use of the same, or nearly the same hypothesis that I have; and the conclusions he draws from it agree nearly with mine, as far as he goes. However, as I have carried the theory much farther than he has done, and have considered the subject in a different, and, I flatter myself, in a more accurate manner, I hope the Society will not think this paper unworthy of their acceptance.

2] The method I propose to follow is, first, to lay down the hypothesis; next, to examine by strict mathematical reasoning, or at least, as strict reasoning as the nature of the subject will admit of, what consequences will flow from thence; and lastly, to examine how far these consequences agree with such experiments as have yet been made on this subject. In a future paper, I intend to give the result of some experiments I am making, with intent to examine still further the truth of this hypothesis, and to find out the law of the electric attraction and repulsion.

HYPOTHESIS.

3] There is a substance, which I call the electric fluid, the particler of which repel each other and attract the particles of all other mattes with a force inversely as some less power of the distance than the cube: the particles of all other matter also, repel each other, and attract those of the electric fluid, with a force varying according to the same power of the distances. Or, to express it more concisely, if you look upon the electric fluid as matter of a contrary kind to other matter, the particles of all matter, both those of the electric fluid and of other matter, repel particles of the same kind, and attract those of a contrary kind, with a force inversely as some less power of the distance than the cube.

* [Petropoli, 1759.]

4] For the future, I would be understood never to comprehend the electric fluid under the word matter, but only some other sort of matter.

5] It is indifferent whether you suppose all sorts of matter to be indued in an equal degree with the foregoing attraction and repulsion, or whether you suppose some sorts to be indued with it in a greater degree than others; but it is likely that the electric fluid is indued with this property in a much greater degree than other matter; for in all probability the weight of the electric fluid in any body bears but a very small proportion to the weight of the matter; but yet the force with which the electric fluid therein attracts any particle of matter must be equal to the force with which the matter therein repels that particle; otherwise the body would appear electrical, as will be shewn hereafter.

To explain this hypothesis more fully, suppose that 1 grain of electric fluid attracts a particle of matter, at a given distance, with as much force as n grains of any matter, lead for instance, repel it: then will 1 grain of electric fluid repel a particle of electric fluid with as much force as n grains of lead attract it; and 1 grain of electric fluid will repel 1 grain of electric fluid with as much force as n grains of lead repel n grains of lead*.

6] All bodies in their natural state with regard to electricity, contain such a quantity of electric fluid interspersed between their particles, that the attraction of the electric fluid in any small part of the body on a given particle of matter shall be equal to the repulsion of the matter in the same small part on the same particle. A body in this state I call saturated with electric fluid: if the body contains more than this quantity of electric fluid, I call it overcharged: if less, I call it undercharged. This is the hypothesis; I now proceed to examine the consequences which will flow from it.

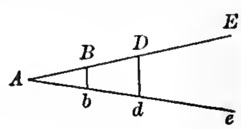


Fig. 1.

7] Lemma I. Let EAe (Fig. 1) represent a cone continued infinitely; let A be the vertex, and Bb and Dd planes parallel to the base; and let the cone be filled with uniform matter, whose particles repel each other with a force inversely as the n power of the distance. If n is greater than 3, the force with which a particle at A is repelled by $EBbe$ or all that part of the cone beyond Bb is as $\frac{1}{AB^{n-3}}$.

For supposing AB to flow, the fluxion of $EBbe$ is proportional to $-\dot{AB} \times AB^2$, and the fluxion of its repulsion on A is proportional to $\frac{-\dot{AB}}{AB^{n-2}}$; the fluent of which is $\frac{1}{(n-3)\,AB^{n-3}}$; which when AB is infinite is equal to nothing; consequently the repulsion of $EBbe$ is proportional to

$$\frac{1}{(n-3)\,AB^{n-3}} \text{ or to } \frac{1}{AB^{n-3}}.$$

* [Note 1, p. 352.]

8] Cor. If AB is infinitely small, $\frac{1}{AB^{n-3}}$ is infinitely great; therefore the repulsion of that part of the cone between A and Bb, on A, is infinitely greater than the repulsion of all that beyond it.

9] Lemma II. By the same method of reasoning it appears, that if n is equal to 3, the repulsion of the matter between Bb and Dd on a particle at A, is proportional to the logarithm of $\frac{AD}{AB}$; consequently, the repulsion of that part is infinitely small in respect of that between A and Bb, and also infinitely small in respect of that beyond Dd.

10] Lemma III. In like manner, if n is less than 3, the repulsion of the part between A and Bb on A is proportional to AB^{3-n}: consequently the repulsion of the matter between A and Bb, on A, is infinitely small in respect of that beyond it.

11] Cor. It is easy to see from these three lemmata, that, if the electric attraction and repulsion had been supposed to be inversely as some higher power of the distance than the cube, a particle could not have been sensibly affected by the repulsion of any fluid, except what was placed close to it. If the repulsion was inversely as the cube of the distance, a particle could not be sensibly affected by the repulsion of any finite quantity of fluid, except what was close to it. But as the repulsion is supposed to be inversely as some power of the distance less than the cube, a particle may be sensibly affected by the repulsion of a finite quantity of fluid, placed at any finite distance from it.

12] Def. If the electric fluid in any body is by any means confined in such manner that it cannot move from one part of the body to the other, I call it immoveable: if it is able to move readily from one part to another, I call it moveable.

13] Prop. I. A body overcharged with electric fluid attracts or repels a particle of matter or fluid, and is attracted or repelled by it, with exactly the same force as it would, if the matter in it, together with so much of the fluid as is sufficient to saturate it, was taken away, or as if the body consisted only of the redundant fluid in it. In like manner an undercharged body attracts or repels with the same force, as if it consisted only of the redundant matter; the electric fluid, together with so much of the matter as is sufficient to saturate it, being taken away.

This is evident from the definition of saturation.

14] Prop. II. Two over or undercharged bodies attract or repel each other with just the same force that they would, if each body consisted only of the redundant fluid in it, if overcharged, or of the redundant matter in it, if undercharged.

For, let the two bodies be called A and B; by the last proposition the redundant substance in B impels each particle of fluid and matter in A, and consequently impels the whole body A, with the same force that the whole body B impels it: for the same reason the redundant substance in A impels the redundant substance in B, with the same force that the whole body A impels it. It is shewn therefore, that the whole body B impels the whole body A, with the same force that the redundant substance in B impels the whole body A, or with which the whole body A impels the redundant substance in B; and that the whole body A impels the redundant substance in B, with the same force that the redundant substance in A impels the redundant substance in B; therefore the whole body B impels the whole body A, with the same force with which the redundant substance in A impels the redundant substance in B, or with which the redundant substance in B impels the redundant substance in A.

15] COR. Let the matter in all the rest of space, except in two given bodies, be saturated with immoveable fluid; and let the fluid in those two bodies be also immoveable. Then, if one of the bodies is saturated, and the other either over or undercharged, they will not at all attract or repel each other.

If the bodies are both overcharged, they will repel each other.

If they are both undercharged, they will also repel each other.

If one is overcharged and the other undercharged, they will attract each other.

N.B. In this corollary, when I call a body overcharged, I would be understood to mean, that it is overcharged in all parts, or at least nowhere undercharged: in like manner, when I call it undercharged, I mean that it is undercharged in all parts, or at least nowhere overcharged.

16] PROP. III. If all the bodies in the universe are saturated with electric fluid, it is plain that no part of the fluid can have any tendency to move.

17] PROP. IV. If the quantity of electric fluid in the universe is exactly sufficient to saturate the matter therein, but unequally dispersed, so that some bodies are overcharged and others undercharged; then, if the electric fluid is not confined, it will immediately move till all the bodies in the universe are saturated.

For supposing that any body is overcharged, and the bodies near it are not, a particle at the surface of that body will be repelled from it by the redundant fluid within; consequently some fluid will run out of that body; but if the body is undercharged, a particle at its surface will be attracted towards the body by the redundant matter within, so that some fluid will run into the body.

N.B. In Prob. IV, Case 3 {p. 42}, there will be shewn an exception to this proposition; there may perhaps be some other exceptions to it: but

I think there can be no doubt, but what this proposition must hold good in general.

18] LEMMA IV. Let *BDE*, *bde*, and $\beta\delta\epsilon$ (Fig. 2), be concentric spherical surfaces, whose center is *C*: if the space* *Bb* is filled with uniform matter, whose particles repel with a force inversely as the square of the distance, a particle placed anywhere within the space *Cb*, as at *P*, will be repelled with as much force in one direction as another, or it will not be impelled in any direction. This is demonstrated in Newton, *Princip.* Lib. I, Prop. 70. It follows also from his demonstration, that if the repulsion is inversely as some higher power of the distance than the square, the particle *P* will be impelled towards the center; and if the repulsion is inversely as some lower power than the square, it will be impelled from the center†.

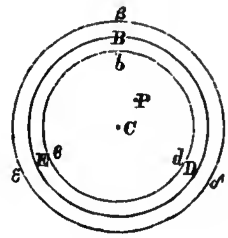


Fig. 2.

19] LEMMA V. If the repulsion is inversely as the square of the distance, a particle placed anywhere without the sphere *BDE*, is repelled by that sphere, and also by the space *Bb*, with the same force that it would if all the matter therein was collected in the center of the sphere; provided the density of the matter therein is everywhere the same at the same distance from the center. This is easily deduced from Prop. 71, of the same book, and has been demonstrated by other authors.

20] PROP. V. PROBLEM I. Let the sphere *BDE* be filled with uniform solid matter, overcharged with electric fluid: let the fluid therein be moveable, but unable to escape from it: let the fluid in the rest of infinite space be moveable, and sufficient to saturate the matter therein; and let the matter in the whole of infinite space, or at least in the space $B\beta$, whose dimensions will be given below, be uniform and solid; and let the law of the electric attraction and repulsion be inversely as the square of the distance: it is required to determine in what manner the fluid will be disposed both within and without the globe.

Take the space *Bb* such, that the interstices between the particles of matter therein shall be just sufficient to hold a quantity of electric fluid, whose particles are pressed close together, so as to touch each other, equal to the whole redundant fluid in the globe, besides the quantity requisite to saturate the matter in *Bb*; and take the space $B\beta$ such, that the matter therein shall be just able to saturate the redundant fluid in the globe:

* By the space *Bb* or $B\beta$, I mean the space comprehended between the spherical surfaces *BDE* and *bde*, or between *BDE* and $\beta\delta\epsilon$: by the space *Cb* or $C\beta$, I mean the spheres *bde* or $\beta\delta\epsilon$.

{† Hence the only law according to a power of the distances which permits absence of force inside a charged shell is the inverse square.}

then, in all parts of the space Bb, the fluid will be pressed close together, so that its particles shall touch each other; the space $B\beta$ will be intirely deprived of fluid; and in the space Cb, and all the rest of infinite space, the matter will be exactly saturated.

For, if the fluid is disposed in the above-mentioned manner, a particle of fluid placed anywhere within the space Cb will not be impelled in any direction by the fluid in Bb, or the matter in $B\beta$, and will therefore have no tendency to move: a particle placed anywhere without the sphere $\beta\delta\epsilon$ will be attracted with just as much force by the matter in $B\beta$, as it is repelled by the redundant fluid in Bb, and will therefore have no tendency to move: a particle placed anywhere within the space Bb, will indeed be repelled towards the surface, by all the redundant fluid in that space, which is placed nearer the center than itself; but as the fluid in that space is already pressed as close together as possible, it will not have any tendency to move; and in the space $B\beta$ there is no fluid to move, so that no part of the fluid can have any tendency to move.

Moreover, it seems impossible for the fluid to be at rest, if it is disposed in any other form; for if the density of the fluid is not everywhere the same at the same distance from the center, but is greater near b than near d, a particle placed anywhere between those two points will move from b towards d; but if the density is everywhere the same at the same distance from the center, and the fluid in Bb is not pressed close together, the space Cb will be overcharged, and consequently a particle at b will be repelled from the center, and cannot be at rest: in like manner, if there is any fluid in $B\beta$, it cannot be at rest: and, by the same kind of reasoning, it might be shewn, that, if the fluid is not spread uniformly within the space Cb, and without the sphere $\beta\delta\epsilon$, it cannot be at rest.

21] Cor. I. If the globe BDE is undercharged, everything else being the same as before, there will be a space Bb, in which the matter will be intirely deprived of fluid, and a space $B\beta$, in which the fluid will be pressed close together; the matter in Bb being equal to the whole redundant matter in the globe, and the redundant fluid in $B\beta$, being just sufficient to saturate the matter in Bb: and in all the rest of space the matter will be exactly saturated. The demonstration is exactly similar to the foregoing.

22] Cor. II. The fluid in the globe BDE will be disposed in exactly the same manner, whether the fluid without is immoveable, and disposed in such manner, that the matter shall be everywhere saturated, or whether it is disposed as above described; and the fluid without the globe will be disposed in just the same manner, whether the fluid within is disposed uniformly, or whether it is disposed as above described.

23] Prop. VI. Problem 2. To determine in what manner the fluid will be disposed in the globe BDE, supposing everything as in the last problem, except that the fluid on the outside of the globe is immoveable, and disposed in such manner as everywhere to saturate the matter, and that the electric attraction and repulsion is inversely as some other power of the distance than the square.

I am not able to answer this problem accurately; but I think we may be certain of the following circumstances.

24] CASE 1. Let the repulsion be inversely as some power of the distance between the square and the cube, and let the globe be overcharged.

It is certain that the density of the fluid will be everywhere the same, at the same distance from the center. Therefore, first, There can be no space as *Cb*, within which the matter will be everywhere saturated; for a particle at *b* is impelled towards the center, by the redundant fluid in *Bb*, and will therefore move towards the center, unless *Cb* is sufficiently overcharged to prevent it. Secondly, The fluid close to the surface of the sphere will be pressed close together; for otherwise a particle so near to it, that the quantity of fluid between it and the surface should be very small, would move towards it; as the repulsion of the small quantity of fluid between it and the surface would be unable to balance the repulsion of the fluid on the other side. Whence, I think, we may conclude, that the density of the fluid will increase gradually from the center to the surface, where the particles will be pressed close together: whether the matter exactly at the center will be overcharged, or only saturated, I cannot tell.

25] COR. For the same reason, if the globe be undercharged, I think we may conclude, that the density of the fluid will diminish gradually from the center to the surface, where the matter will be intirely deprived of fluid.

26] CASE 2. Let the repulsion be inversely as some power of the distance less than the square; and let the globe be overcharged.

There will be a space *Bb*, in which the particles of the fluid will be everywhere pressed close together; and the quantity of redundant fluid in that space will be greater than the quantity of redundant fluid in the whole globe *BDE*; so that the space *Cb*, taken all together, will be undercharged: but I cannot tell in what manner the fluid will be disposed in that space.

For it is certain, that the density of the fluid will be everywhere the same, at the same distance from the center. Therefore, let *b* be any point where the fluid is not pressed close together, then will a particle at *b* be impelled towards the surface, by the redundant fluid in the space *Bb*; therefore, unless the space *Cb* is undercharged, the particle will move towards the surface.

27] Cor. For the same reason, if the globe is undercharged, there will be a space *Bb*, in which the matter will be intirely deprived of fluid, the quantity of matter therein being more than the whole redundant matter in the globe; and, consequently, the space *Cb*, taken all together, will be overcharged*.

28] Lemma VI. Let the whole space comprehended between two parallel planes, infinitely extended each way, be filled with uniform matter, the repulsion of whose particles is inversely as the square of the distance; the plate of matter formed thereby will repel a particle of matter with exactly the same force, at whatever distance from it it be placed.

For suppose that there are two such plates, of equal thickness, placed parallel to each other, let *A* (Fig. 3) be any point not placed in or between the two plates: let *BCD* represent any part of the nearest plate: draw the lines *AB*, *AC*, and *AD*, cutting the furthest plate in *b*, *c*, and *d*; for it is plain that if they cut one plate, they must, if produced, cut the other: the triangle *BCD* is to the triangle *bcd*, as AB^2 to Ab^2; therefore a particle of matter at *A* will be repelled with the same force by the matter in the triangle *BCD*, as by that in *bcd*. Whence it appears, that a particle at *A* will be repelled with as much force by the nearest plate, as by the more distant; and consequently, will be impelled with the same force by either plate, at whatever distance from it it be placed.

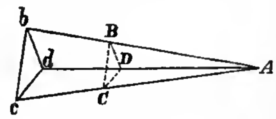


Fig. 3.

29] Cor. If the repulsion of the particles is inversely as some higher power of the distance than the square, the plate will repel a particle with more force, if its distance be small than if it be great; and if the repulsion is inversely as some lower power than the square, it will repel a particle with less force, if its distance be small than if it be great.

30] Prop. VII. Prob. 3. In Fig. 4, let the parallel lines *Aa*, *Bb*, &c. represent parallel planes infinitely extended each way: let the spaces† *AD* and *EH* be filled with uniform solid matter: let the electric fluid in each of those spaces be moveable and unable to escape: and let all the rest of the matter in the universe be saturated with immoveable fluid; and let the electric attraction and repulsion be inversely as the square of the distance. It is required to determine in what manner the fluid will be disposed in the spaces *AD* and *EH*, according as one or both of them are over or undercharged.

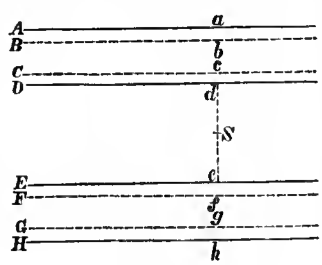


Fig. 4.

* [Note 2, p. 358.]

† By the space *AD* or *AB*, &c. I mean the space comprehended between the planes *Aa* and *Dd*, or between *Aa* and *Bb*.

Let AD be that space which contains the greatest quantity of redundant fluid, if both spaces are overcharged, or which contains the least redundant matter, if both are undercharged; or, if one is overcharged, and the other undercharged, let AD be the overcharged one. Then, first, There will be two spaces, AB and GH, which will either be intirely deprived of fluid, or in which the particles will be pressed close together; namely, if the whole quantity of fluid in AD and EH together, is less than sufficient to saturate the matter therein, they will be intirely deprived of fluid; the quantity of redundant matter in each being half the whole redundant matter in AD and EH together: but if the fluid in AD and EH together is more than sufficient to saturate the matter, the fluid in AB and GH will be pressed close together; the quantity of redundant fluid in each being half the whole redundant fluid in both spaces. Secondly, In the space CD the fluid will be pressed close together; the quantity of fluid therein being such, as to leave just enough fluid in BC to saturate the matter therein. Thirdly, The space EF will be intirely deprived of fluid; the quantity of matter therein being such that the fluid in FG shall be just sufficient to saturate the matter therein: consequently, the redundant fluid in CD will be just sufficient to saturate the redundant matter in EF; for as AB and GH together contain the whole redundant fluid or matter in both spaces, the spaces BD and EG together contain their natural quantity of fluid; and therefore, as BC and FG each contain their natural quantity of fluid, the spaces CD and EF together contain their natural quantity of fluid. And fourthly, The spaces BC and FG will be saturated in all parts.

For, first, If the fluid is disposed in this manner, no particle of it can have any tendency to move: for a particle placed anywhere in the spaces BC and FG, is attracted with just as much force by EF, as it is repelled by CD; and it is repelled or attracted with just as much force by AB, as it is in a contrary direction by GH, and, consequently, has no tendency to move. A particle placed anywhere in the space CD, or in the spaces AB and GH, if they are overcharged, is indeed repelled with more force towards the planes Dd, Aa and Hh, than it is in the contrary direction; but as the fluid in those spaces is already as much compressed as possible, the particle will have no tendency to move.

Secondly, It seems impossible that the fluid should be at rest, if it is disposed in any other manner: but as this part of the demonstration is exactly similar to the latter part of that of Problem the first, I shall omit it.

31] Cor. I. If the two spaces AD and EH are both overcharged, the redundant fluid in CD is half the difference of the redundant fluid in those spaces: for half the difference of the redundant fluid in those spaces, added to the quantity in AB, which is half the sum, is equal to the whole quantity in AD. For a like reason, if AD and EH are both undercharged, the redundant matter in EF is half the difference of the redundant matter in those spaces; and if AD is overcharged, and EH undercharged, the redundant fluid in CD exceeds half the redundant fluid in AD, by a quantity sufficient to saturate half the redundant matter in EH.

32] COR. II. It was before said, that the fluid in the spaces AB and GH (when there is any fluid in them) is repelled against the planes Aa and Hh; and, consequently, would run out through those planes, if there was any opening for it to do so. The force with which the fluid presses against the planes Aa and Hh, is that with which the redundant fluid in AB is repelled by that in GH; that is, with which half the redundant fluid in both spaces is repelled by an equal quantity of fluid. Therefore, the pressure against Aa and Hh depends only on the quantity of redundant fluid in both spaces together, and not at all on the thickness or distance of those spaces, or on the proportion in which the fluid is divided between the two spaces. If there is no fluid in AB and GH, a particle placed on the outside of the spaces AD and EH, contiguous to the planes Aa or Hh, is attracted towards those planes by all the matter in AB and GH, *id est*, by all the redundant matter in both spaces; and, consequently, endeavours to insinuate itself into the space AD or EH; and the force with which it does so depends only on the quantity of redundant matter in both spaces together. The fluid in CD also presses against the plane Dd, and the force with which it does so is that with which the redundant fluid in CD is attracted by the matter in EF.

33] COR. III. If AD is overcharged, and EH undercharged: and the redundant fluid in AD is exactly sufficient to saturate the redundant matter in EH, all the redundant fluid in AD will be collected in the space CD, where it will be pressed close together: the space EF will be intirely deprived of fluid, the quantity of matter therein being just sufficient to saturate the redundant fluid in CD, and the spaces AC and FH will be everywhere saturated. Moreover, if an opening is made in the planes Aa or Hh, the fluid within the spaces AD or EH will have no tendency to run out thereat, nor will the fluid on the outside have any tendency to run in at it: a particle of fluid too placed anywhere on the outside of both spaces, as at P, will not be at all attracted or repelled by those spaces, any more than if they were both saturated; but a particle placed anywhere between those spaces, as at S, will be repelled from d towards e; and if a communication was made between the two spaces, by the canal de, the fluid would run out of AD into EH, till they were both saturated.

34] PROP. VIII. PROB. 4. To determine in what manner the fluid will be disposed in the space AD, supposing that all the rest of the universe is saturated with immoveable fluid, and that the electric attraction and repulsion is inversely as some other power of the distance than the square.

I am not able to answer this Problem accurately, except when the repulsion is inversely as the simple or some lower power of the distance; but I think we may be certain of the following circumstances.

35] Case 1. Let the repulsion be inversely as some power of the distance between the square and the cube, and let AD be overcharged.

First, It is certain that the density of the fluid must be everywhere the same, at the same distance from the planes Aa and Dd. Secondly, There can be no space as BC, of any sensible breadth, in which the matter will not be overcharged. And thirdly, The fluid close to the planes Aa and Dd will be pressed close together. Whence, I think, we may conclude, that the density of the fluid will increase gradually from the middle of the space to the outside, where it will be pressed close together. Whether the matter exactly in the middle will be overcharged, or only saturated, I cannot tell.

36] Case 2. Let the repulsion be inversely as some power of the distance between the square and the simple power, and let AD be overcharged.

There will be two spaces AB and DC, in which the fluid will be pressed close together, and the quantity of redundant fluid in each of those spaces will be more than half the redundant fluid in AD; so that the space BC, taken all together, will be undercharged; but I cannot tell in what manner the fluid will be disposed in that space. The demonstrations of these two cases are exactly similar to those of the two cases of Prob. 2.

37] Case 3. If the repulsion is inversely as the simple or some lower power of the distance, and AD is overcharged, all the fluid will be collected in the spaces AB and CD, and BC will be intirely deprived of fluid. If AD contains just fluid enough to saturate it, and the repulsion is inversely as the distance, the fluid will remain in equilibrio, in whatever manner it is disposed; provided its density is everywhere the same at the same distance from the planes Aa and Dd: but if the repulsion is inversely as some less power than the simple one, the fluid will be in equilibrio, whether it is either spread uniformly, or whether it is all collected in that plane which is in the middle between Aa and Dd, or whether it is all collected in the spaces AB and CD; but not, I believe, if it is disposed in any other manner.

The demonstration depends upon this circumstance; namely, that if the repulsion is inversely as the distance, two spaces AB and CD, repel a particle placed either between them, or on the outside of them, with the same force as if all the matter of those spaces was collected in the middle plane between them.

It is needless mentioning the three cases in which AD is undercharged, as the reader will easily supply the place.

38] Though the four foregoing problems do not immediately tend to explain the phænomena of electricity, I chose to insert them; partly because they seem worth engaging our attention in themselves; and partly because they serve, in some measure, to confirm the truth of some of the following propositions, in which I am obliged to make use of a less accurate kind of reasoning.

39] In the following propositions, I shall always suppose the bodies I speak of to consist of solid matter, confined to the same spot, so as not to be able to alter its shape or situation by the attraction or repulsion of other bodies on it: I shall also suppose the electric fluid in these bodies to be moveable, but unable to escape, unless when otherwise expressed. As for the matter in all the rest of the universe, I shall suppose it to be saturated with immoveable fluid. I shall also suppose the electric attraction and repulsion to be inversely as any power of the distance less than the cube, except when otherwise expressed.

40] By a canal, I mean a slender thread of matter, of such kind that the electric fluid shall be able to move readily along it, but shall not be able to escape from it, except at the ends, where it communicates with other bodies. Thus, when I say that two bodies communicate with each other by a canal, I mean that the fluid shall be able to pass readily from one body to the other by that canal*.

41] Prop. IX. If any body at a distance from any over or undercharged body be overcharged, the fluid within it will be lodged in greater quantity near the surface of the body than near the center. For, if you suppose it to be spread uniformly all over the body, a particle of fluid in it, near the surface, will be repelled towards the surface by a greater quantity of fluid than that by which it is repelled from it; consequently, the fluid will flow towards the surface, and make it denser there: moreover, the particles of fluid close to the surface will be pressed close together; for otherwise, a particle placed so near it, that the quantity of redundant fluid between it and the surface should be very small, would move towards it; as the small quantity of redundant fluid between it and the surface would be unable to balance the repulsion of that on the other side.

From the four foregoing problems it seems likely, that if the electric attraction or repulsion is inversely as the square of the distance, almost all the redundant fluid in the body will be lodged close to the surface, and there pressed close together, and the rest of the body will be saturated. If the repulsion is inversely as some power of the distance between the square and the cube, it is likely that all parts of the body will be overcharged: and if it is inversely as some less power than the square, it is likely that all parts of the body, except those near the surface, will be undercharged.

* [Note 3, p. 365.]

42] Cor. For the same reason, if the body is undercharged, the deficiency of fluid will be greater near the surface than near the center, and the matter near the surface will be entirely deprived of fluid. It is likely too, if the repulsion is inversely as some higher power of the distance than the square, that all parts of the body will be undercharged: if it is inversely as the square, that all parts, except near the surface, will be saturated: and if it is inversely as some less power than the square, that all parts, except near the surface, will be overcharged.

43] Prop. X. Let the bodies A and D (Fig. 5) communicate with each other by the canal EF; and let one of them, as D, be overcharged; the other body A will be so also.

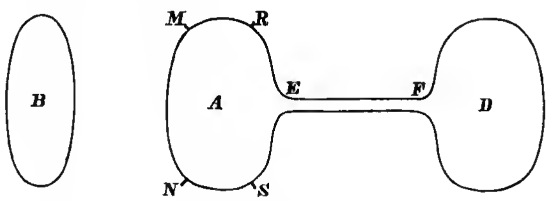


Fig. 5.

For as the fluid in the canal is repelled by the redundant fluid in D, it is plain, that unless A was overcharged, so as to balance that repulsion, the fluid would run out of D into A.

In like manner, if one is undercharged, the other must be so too.

44] Prop. XI. Let the body A (Fig. 6) be either saturated or over or undercharged; and let the fluid within it be in equilibrio. Let now the body B, placed near it, be rendered overcharged, the fluid within it being supposed immoveable, and disposed in such manner, that no part of it shall be undercharged; the fluid in A will no longer be in equilibrio, but will be repelled from B: therefore, the fluid will flow from those parts of A which are nearest to B, to those which are more distant from it; and, consequently, the part adjacent to MN (that part of the surface of A which is turned towards B) will be made to contain less electric fluid than it did before, and that adjacent to the opposite surface RS will contain more than before.

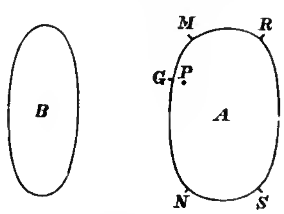


Fig. 6.

It must be observed, that when a sufficient quantity of fluid has flowed from MN towards RS, the repulsion which the fluid in the part adjacent to MN exerts on the rest of the fluid in A, will be so much weakened, and the repulsion of that in the part near RS will be so much increased, as to compensate the repulsion of B, which will prevent any more fluid flowing from MN to RS.

The reason why I suppose the fluid in *B* to be immoveable is, that otherwise a question might arise, whether the attraction or repulsion of the body *A* might not cause such an alteration in the disposition of the fluid in *B*, as to cause some parts of it to be undercharged; which might make it doubtful, whether *B* did on the whole repel the fluid in *A*. It is evident, however, that this proposition would hold good, though some parts of *B* were undercharged, provided it did on the whole repel the fluid in *A*.

45] Cor. If *B* had been made undercharged, instead of overcharged, it is plain that some fluid would have flowed from the further part *RS* to the nearer part *MN*, instead of from *MN* to *RS*.

46] Prop. XII. Let us now suppose that the body *A* communicates by the canal *EF*, with another body *D*, placed on the contrary side of it from *B*, as in Fig. 5; and let these two bodies be either saturated, or over or undercharged; and let the fluid within them be in equilibrio. Let now the body *B* be overcharged: it is plain that some fluid will be driven from the nearer part *MN* to the further part *RS*, as in the former proposition; and also some fluid will be driven from *RS*, through the canal, to the body *D*; so that the quantity of fluid in *D* will be increased thereby, and the quantity in *A*, taking the whole body together, will be diminished; the quantity in the part near *MN* will also be diminished; but whether the quantity in the part near *RS* will be diminished or not, does not appear for certain; but I should imagine it would be not much altered.

47] Cor. In like manner, if *B* is made undercharged, some fluid will flow from *D* to *A*, and also from that part of *A* near *RS*, to the part near *MN*.

48] Prop. XIII. Suppose now that the bodies *A* and *D* communicate by the bent canal *MPNnpm* (Fig. 7) instead of the straight one *EF*: let the bodies be either saturated or over or undercharged as before; and let

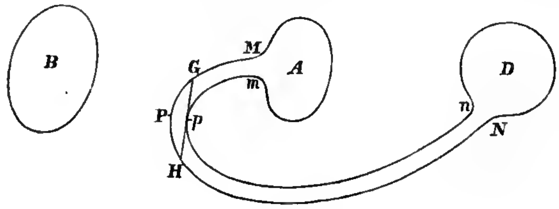


Fig. 7.

the fluid be at rest; then if the body *B* is made overcharged, some fluid will still run out of *A* into *D*; provided the repulsion of *B* on the fluid in the canal is not too great.

The repulsion of *B* on the fluid in the canal will at first drive some fluid out of the leg *MPpm* into *A*, and out of *NPpn* into *D*, till the quantity of fluid in that part of the canal which is nearest to B is so much diminished, and its repulsion on the rest of the fluid in the canal is so much diminished also as to compensate the repulsion of B: but as the leg $NPpn$ is longer than the other, the repulsion of B on the fluid in it will be greater; consequently some fluid will run out of A into D, on the same principle that water is drawn out of a vessel through a siphon.

49] But if the repulsion of B on the fluid in the canal is so great, as to drive all the fluid out of the space $GPHpG$, so that the fluid in the leg $MGpm$ does not join to that in $NHpn$; then it is plain that no fluid can run out of A into D; any more than water will run out of a vessel through a siphon, if the height of the bend of the siphon above the water in the vessel, is greater than that to which water will rise in vacuo.

50] Cor. If B is made undercharged, some fluid will run out of D into A; and that though the attraction of B on the fluid in the canal is ever so great.

51] Prop. XIV. Let ABC (Fig. 8) be a body overcharged with immoveable fluid, uniformly spread; let the bodies near ABC on the outside be saturated with immoveable fluid; and let D be a body inclosed within ABC, and communicating by the canal DG with other distant bodies saturated with fluid; and let the fluid in D and the canal and those bodies be moveable; then will the body D be rendered undercharged.

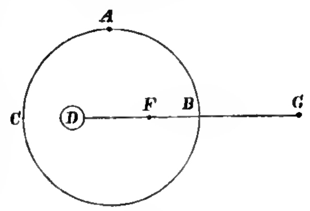


Fig. 8.

For let us first suppose that D and the canal are saturated, and that D is nearer to B than to the opposite part of the body, C; then will all the fluid in the canal be repelled from C by the redundant fluid in ABC; but if D is nearer to C than to B, take the point F, such that a particle placed there would be repelled from C with as much force as one at D is repelled towards C; the fluid in DF, taking the whole together, will be repelled with as much force one way as the other; and the fluid in FG is all of it repelled from C: therefore in both cases the fluid in the canal, taking the whole together, is repelled from C; consequently some fluid will run out of D and the canal, till the attraction of the unsaturated matter therein is sufficient to balance the repulsion of the redundant fluid in ABC.

52] Prop. XV. If we now suppose that the fluid on the outside of ABC is moveable; the matter adjacent to ABC on the outside will become undercharged. I see no reason however to think that that will prevent the body D from being undercharged; but I cannot say exactly what effect it will have, except when ABC is spherical and the repulsion is inversely as the square of the distance; in this case it appears by Prob. I that the fluid in the part DB of the canal will be repelled from C, with just a smuch force as in the last proposition; but the fluid in the part BG will not be repelled at all: consequently D will be undercharged, but not so much as in the last proposition.

53] Cor. If ABC is now supposed to be undercharged, it is certain that D will be overcharged, provided the matter near ABC on the outside is saturated with immoveable fluid; and there is great reason to think that it will be so, though the fluid in that matter is moveable.

54] Prop. XVI. Let $AEFB$ (Fig. 9) be a long cylindric body, and D an undercharged body; and let the quantity of fluid in $AEFB$ be such, that the part near EF shall be saturated. It appears from what has been said before, that the part near AB will be overcharged; and moreover there will be a certain space, as $AabB$, adjoining to the plane AB, in which the fluid will be pressed close together; and the fluid in that space will press against the plane AB, and will endeavour to escape from it; and by Prop. II the two bodies will attract each other: now I say that the force with which the fluid presses against the plane AB, is very nearly the same with which the two bodies attract each other in the direction EA; provided that no part of $AEFB$ is undercharged.

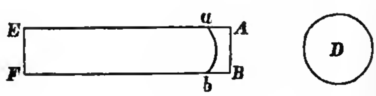


Fig. 9.

Suppose so much of the fluid in each part of the cylinder as is sufficient to saturate the matter in that part, to become solid; the remainder, or the redundant fluid remaining fluid as before. In this case the pressure against the plane AB must be exactly equal to that with which the two bodies attract each other in the direction EA: for the force with which D attracts that part of the fluid which we supposed to become solid, is exactly equal to that with which it repels the matter in the cylinder; and the redundant fluid in $EabF$ is at liberty to move, if it had any tendency to do so, without moving the cylinder; so that the only thing which has any tendency to impel the cylinder in the direction EA is the pressure of the redundant fluid in $AabB$ against AB; and as the part near EF is saturated, there is no redundant fluid to press against the plane EF, and thereby to counteract the pressure against AB. Suppose now all the electric fluid in the cylinder to become fluid; the force with which the two bodies attract each other will remain exactly the same; and the only alteration in the pressure against AB, will be, that that part of the fluid in $AabB$, which we at first supposed solid and unable to press against the plane, will now be at liberty to press against it; but as the density of the fluid when its particles are pressed close together may be supposed many times greater than when it is no denser than sufficient to saturate the matter in the cylinder, and consequently the quantity of redundant fluid in $AabB$ many times greater than that which is required to saturate the matter therein, it follows that the pressure against AB will be very little more than on the first supposition.

N.B. If any part of the cylinder is undercharged, the pressure against AB is greater than the force with which the bodies attract. If the electric repulsion is inversely as the square or some higher power of the distance, it seems very unlikely that any part of the cylinder should be undercharged; but if the repulsion is inversely as some lower power than the square, it is not improbable but some part of the cylinder may be undercharged.

55] LEMMA VII. Let AB (Fig. 10) represent an infinitely thin flat circular plate, seen edgeways, so as to appear to the eye as a straight line; let C be the center of the circle; and let DC passing through C, be perpendicular to the plane of the plate; and let the plate be of uniform thickness, and consist of uniform matter, whose particles repel with a force inversely as the n power of the distance; n being greater than one, and less than three: the repulsion of the plate on a particle at D is proportional to

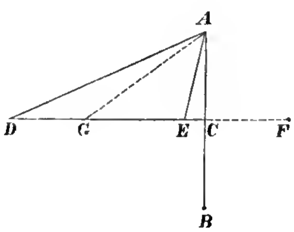


Fig. 10.

$$\frac{DC}{DC^{n-1}} - \frac{DC}{DA^{n-1}};$$

provided the thickness of the plate and size of the particle D is given.

For if CA is supposed to flow, the corresponding fluxion of the quantity of matter in the plate is proportional to $CA \times \dot{CA}$; and the corresponding fluxion of the repulsion of the plate on the particle D, in the direction DC, is proportional to

$$\frac{CA \times \dot{CA}}{DA^n} \times \frac{DC}{DA}, = \frac{\dot{DA} \times DC}{DA^n};$$

for $\dot{DA} : \dot{CA} :: CA : DA$; the variable part of the fluent of which is $\frac{-DC}{(n-1)\,DA^{n-1}}$: whence the repulsion of the plate on the particle D is proportional to $\frac{DC}{(n-1)\,DC^{n-1}} - \frac{DC}{(n-1)\,DA^{n-1}}$, or to $\frac{DC}{DC^{n-1}} - \frac{DC}{DA^{n-1}}$.

56] COR. If DC^{n-1} is very small in respect of CA^{n-1}, the particle D is repelled with very nearly the same force as if the diameter of the plate was infinite.

57] LEMMA VIII. Let L and l represent the two legs of a right-angled triangle, and h the hypothenuse; if the shorter leg l is so much less than the other, that l^{n-1} is very small in respect of L^{n-1}, $h^{3-n} - L^{3-n}$ will be very small in respect of l^{3-n}.

For $h^{3-n} = (L^2 + l^2)^{\frac{3-n}{2}} = L^{3-n}\left(1 + \frac{l^2}{L^2}\right)^{\frac{3-n}{2}}$

$$= L^{3-n}\left[1 + \frac{(3-n)\,l^2}{2L^2} - \frac{(3-n)\,(n-1)\,l^4}{8L^4}, \text{ \&c.}\right]$$

therefore

$$h^{3-n} - L^{3-n} = \frac{(3-n)\,l^2}{2L^{n-1}} - \frac{3-n \times n-1 \times l^4}{8L^{n+1}}, \text{ \&c.}$$

$$= \frac{l^{3-n} \times 3-n \times l^{n-1}}{2L^{n-1}} - \frac{l^{3-n} \times 3-n \times n-1 \times l^{n+1}}{8L^{n+1}}, \text{ \&c.}$$

which is very small in respect of l^{3-n}; as l^{n-1} is by the supposition very small in respect of L^{n-1}.

58] Lemma IX. Let DG now represent the axis of a cylindric or prismatic column of uniform matter; and let the diameter of the column be so small, that the repulsion of the plate AB on it shall not be sensibly different from what it would be, if all the matter in it was collected in the axis: the force with which the plate repels the column is proportional to

$$DC^{3-n} + AC^{3-n} - DA^{3-n};$$

supposing the thickness of the plate and base of the column to be given.

For, if DC is supposed to flow, the corresponding fluxion of the repulsion is proportional to

$$\frac{D\dot{C}}{DC^{n-2}} - \frac{DC \times D\dot{C}}{DA^{n-1}} = \frac{D\dot{C}}{DC^{n-2}} - \frac{D\dot{A}}{DA^{n-2}};$$

the fluent of which, $\dfrac{AC^{3-n} + DC^{3-n} - DA^{3-n}}{3-n}$, vanishes when DC vanishes.

59] Cor. I. If the length of the column is so great that AC^{n-1} is very small in respect of DC^{n-1}, the repulsion of the plate on it is very nearly the same as if the column was infinitely continued.

For by Lemma VIII $AC^{3-n} + DC^{3-n} - DA^{3-n}$ differs very little in this case from AC^{3-n}; and if DC is infinite, it is exactly equal to it.

60] Cor. II. If AC^{n-1} is very small in respect of DC^{n-1}, and the point E be taken in DC such that EC^{n-1} shall be very small in respect of AC^{n-1}, the repulsion of the plate on the small part of the column EC, is to its repulsion on the whole column DC, very nearly as EC^{3-n} to AC^{3-n}.

61] Lemma X. If we now suppose all the matter of the plate to be collected in the circumference of the circle, so as to form an infinitely slender uniform ring, its repulsion on the column DC will be less than when the matter is spread uniformly all over the plate, in the ratio of

$$\frac{(3-n)\,AC^2}{2} \times \left(\frac{1}{AC^{n-1}} - \frac{1}{DA^{n-1}}\right) \text{ to } DC^{3-n} + AC^{3-n} - DA^{3-n}.$$

For it was before said, that if the matter of the plate be spread uniformly, its repulsion on the column will be proportional to

$$DC^{3-n} + AC^{3-n} - DA^{3-n},$$

or may be expressed thereby; let now AC, the semidiameter of the plate, be increased by the infinitely small quantity $A\dot{C}$; the quantity of matter in the plate will be increased by a quantity, which is to the whole, as $2A\dot{C}$ to AC; and the repulsion of the plate on the column will be increased by

$$(3-n)\, A\dot{C} \times AC^{2-n} - AC \times \frac{AC}{DA} \times (3-n) \times DA^{2-n},$$

$$= (3-n) \times A\dot{C} \times AC \times \left(\frac{1}{AC^{n-1}} - \frac{1}{DA^{n-1}}\right):$$

therefore if a quantity of matter, which is to the whole quantity in the plate as $2A\dot{C}$ to AC, be collected in the circumference, its repulsion on the column DC will be to that of the whole plate as

$$3 - n \times A\dot{C} \times AC \times \left(\frac{1}{AC^{n-1}} - \frac{1}{DA^{n-1}}\right), \text{ to } DC^{3-n} + AC^{3-n} - DA^{3-n};$$

and consequently the repulsion of the plate when all the matter is collected in its circumference, is to its repulsion when the matter is spread uniformly, as

$$\frac{3 - n \times AC^2}{2} \times \left(\frac{1}{AC^{n-1}} - \frac{1}{DA^{n-1}}\right), \text{ to } DC^{3-n} + AC^{3-n} - DA^{3-n}.$$

62] Cor. I. If the length of the column is so great, that AC^{n-1} is very small in respect of DC^{n-1}, the repulsion of the plate, when all the matter is collected in the circumference, is to its repulsion when the matter is spread uniformly, very nearly as $\frac{3 - n \times AC^{3-n}}{2}$ to AC^{3-n}, or as $3 - n$ to 2.

63] Cor. II. If EC^{n-1} is very small in respect of AC^{n-1}, the repulsion of the plate on the short column EC, when all the matter in the plate is collected in its circumference, is to its repulsion when the matter is spread uniformly, very nearly as

$$\frac{3 - n \times n - 1 \times EC^2}{4AC^{n-1}} \text{ to } EC^{3-n},$$

or as $3 - n \times n - 1 \times EC^{n-1}$ to $4AC^{n-1}$; and is therefore very small in comparison of what it is when the matter is spread uniformly.

For by the same kind of process as was used in Lemma VIII, it appears, that if EC^2 is very small in respect of AC^2,

$$AC^2 \times \left(\frac{1}{AC^{n-1}} - \frac{1}{EA^{n-4}}\right)$$

differs very little from $\frac{n - 1 \times EC^2}{2EA^{n-1}}$, or from $\frac{n - 1 \times EC^2}{2AC^{n-1}}$; and if EC^{n-1} is very small in respect of AC^{n-1}, EC^2 is *à fortiori* very small in respect of AC^2.

64] Cor. III. Suppose now that the matter of the plate is denser near the circumference than near the middle, and that the density at and near the middle is to the mean density, or the density which it would everywhere be of if the matter was spread uniformly, as δ to 1; the repulsion of the plate on EC will be less than if the matter was spread uniformly, in a ratio approaching much nearer to that of δ to 1, than to that of equality.

65] Cor. IV. Let everything be as in the last corollary, and let π be taken to one, as the force with which the plate actually repels the column DC, (DC^{n-1} being very great in respect of AC^{n-1}), is to the force with which it would repel it, if the matter was spread uniformly; the repulsion of the plate on EC will be to its repulsion on DC, in a ratio between that of $EC^{3-n} \times \delta$ to $AC^{3-n} \times \pi$, and that of EC^{3-n} to $AC^{3-n} \times \pi$, but will approach much nearer to the former ratio than to the latter.

66] Lemma XI. In the line DC produced, take CF equal to CA: if all the matter of the plate AB is collected in the circumference, its repulsion on the column CD, infinitely continued, is equal to the repulsion of the same quantity of matter collected in the point F, on the same column.

For the repulsion of the plate on the column in the direction CD, is the same, whether the matter of it be collected in the whole circumference, or in the point A. Suppose it therefore to be collected in A; and let an equal quantity of matter be collected in F; take FG constantly equal to AD; and let AD and FG flow: the fluxion of CD is to the fluxion of FG, as AD to CD; and the repulsion of A on the point D, in the direction CD, is to the repulsion of F on G, as CD to AD; and therefore the fluxion of the repulsion of A on the column CD, in the direction CD, is equal to the fluxion of the repulsion of F on CG; and when AD equals AC, the repulsion of both A and F on their respective columns vanishes; and therefore the repulsion of A on the whole column CD equals that of F on CG; and when CD and CG are both infinitely extended, they may be looked upon as the same column.

67] Prop. XVII. Let two similar bodies, of different sizes, and consisting of different sorts of matter, be both overcharged, or both undercharged, but in different degrees; and let the redundance or deficience of fluid in each be very small in respect of the whole quantity of fluid in them: it is impossible for the fluid to be disposed accurately in a similar manner in both of them*; as it has been shewn that there will be a space,

* By the fluid being disposed in a similar manner in both bodies, I mean that the quantity of redundant or deficient fluid in any small part of one body, is to that in the corresponding small part of the other, as the whole quantity of redundant or deficient fluid in one body, to that in the other. By the quantity of deficient fluid in a body, I mean the quantity of fluid wanting to saturate it. Notwithstanding the impropriety of this expression, I must beg leave to make use of it, as it will frequently save a great deal of circumlocution. [See Note 1.]

close to the surface, which will either be as full of fluid as it can hold, or will be entirely deprived of fluid; but it will be disposed as nearly in a similar manner in both, as is possible. To explain this, let *BDE* and *bde* (Fig. 11) be the two similar bodies; and let the space comprehended between the surfaces *BDE* and *FGH* (or the space *BF* as I shall call it for shortness) be that part of *BDE*, which is either as full of fluid as it can hold, or entirely deprived of it: draw the surface *fgh*, such that the space *bf* shall be to the space *BF*, as the quantity of redundant or deficient fluid in *bde*, to that in *BDE*, and that the thickness of the space *bf* shall everywhere bear the same proportion to the corresponding thickness of *BF*: then will the space *bf* be either as full of fluid as it can hold, or entirely deprived of it; and the fluid within the space *fgh* will be disposed very nearly similarly to that in the space *FGH*.

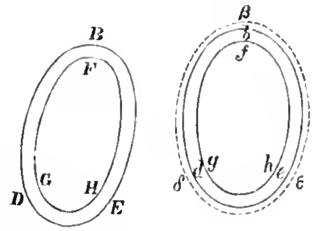


Fig. 11.

For it is plain, that if the fluid could be disposed accurately in a similar manner in both bodies, the fluid would be in equilibrio in one body, if it was in the other: therefore draw the surface $\beta\delta\epsilon$, such that the thickness of the space βf shall be everywhere to the corresponding thickness of *BF*, as the diameter of *bde* to the diameter of *BDE*; and let the redundant fluid or matter in *bf* be spread uniformly over the space βf; then if the fluid in the space *fgh* is disposed exactly similarly to that in *FGH*, it will be in equilibrio; as the fluid will then be disposed exactly similarly in the spaces $\beta\delta\epsilon$ and *BDE*: but as by the supposition, the thickness of the space βf is very small in respect of the diameter of *bde*, the fluid or matter in the space *bf* will exert very nearly the same force on the rest of the fluid, whether it is spread over the space βf, or whether it is collected in *bf*.

68] Prop. XVIII. Let two bodies, *B* and *b*, be connected to each other by a canal of any kind, and be either over or undercharged: it is plain that the quantity of redundant or deficient fluid in *B*, would bear exactly the same proportion to that in *b*, whatever sort of matter *B* consisted of, if it was possible for the redundant or deficient fluid in any body to be disposed accurately in the same manner, whatever sort of matter it consisted of. For suppose *B* to consist of any sort of matter; and let the fluid in the canal and two bodies be in equilibrio: let now *B* be made to consist of some other sort of matter, which requires a different quantity of fluid to saturate it; but let the quantity and disposition of the redundant or deficient fluid in it remain the same as before: it is plain that the fluid will still be in equilibrio; as the attraction or repulsion of any body depends only on the quantity and disposition of the redundant and deficient fluid in it. Therefore, by the preceding proposition, the quantity of redundant or deficient fluid in *B*, will actually bear very nearly the same proportion to that in *b*, whatever sort of matter *B* consists of; provided the quantity of redundant or deficient fluid in it is very small in respect of the whole. [See Exp. IV, Art. 269.]

69] PROP. XIX. Let two bodies *B* and *b* (Fig. 12) be connected together by a very slender canal *ADda*, either straight or crooked: let the canal be everywhere of the same breadth and thickness; so that all sections of this canal made by planes perpendicular to the direction of the canal in that part, shall be equal and similar: let the canal be composed of uniform matter; and let the electric fluid therein be supposed incompressible, and of such density as exactly to saturate the matter therein; and let it, nevertheless, be able to move readily along the canal; and let each particle of fluid in the canal be attracted and repelled by the matter and fluid in the canal and in the bodies *B* and *b*, just in the same manner that it would be if it was not incompressible*; and let the bodies *B* and *b* be either over or undercharged. I say that the force with which the whole quantity of fluid in the canal is impelled from *A* towards *D*, in the direction of the axis of the canal, by the united attractions and repulsions of the two bodies, must be nothing; as otherwise the fluid in the canal could not be at rest: observing that by the force with which the whole quantity of fluid is impelled in the direction of the axis of the canal, I mean the sum of the forces, with which the fluid in each part of the canal is impelled in the direction of the axis of the canal in that place, from *A* towards *D*; and observing also, that an impulse in the contrary direction from *D* towards *A* must be looked upon as negative.

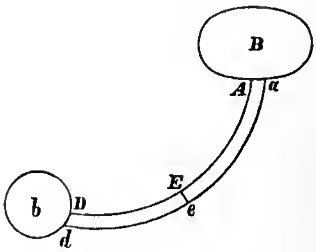


Fig. 12.

For as the canal is exactly saturated with fluid, the fluid therein is attracted or repelled only by the redundant matter or fluid in the two bodies. Suppose now that the fluid in any section of the canal, as *Ee*, is impelled with any given force in the direction of the canal at that place, the section *Dd* would, in consequence thereof, be impelled with exactly the same force in the direction of the canal at *D*, if the fluid between *Ee* and *Dd* was not at all attracted or repelled by the two bodies; and, consequently, the section *Dd* is impelled in the direction of the canal, with the sum of the forces, with which the fluid in each part of the canal is impelled by the attraction or repulsion of the two bodies in the direction of the

* This supposition of the fluid in the canal being incompressible, is not mentioned as a thing which can ever take place in nature, but is merely imaginary; the reason for making of which will be given hereafter.

axis in that part; and consequently, unless this sum was nothing, the fluid in Dd could not be at rest.

70] Cor. Therefore, the force with which the fluid in the canal is impelled one way in the direction of the axis, by the body B, must be equal to that with which it is impelled by b in the contrary direction.

71] Prop. XX. Let two similar bodies B and b (Fig. 13) be connected by the very slender cylindric or prismatic canal Aa, filled with incompressible fluid, in the same manner as described in the preceding proposition: let the bodies be overcharged; but let the quantity of redundant fluid in each bear so small a proportion to the whole, that the fluid may be considered as disposed in a similar manner in both; let the bodies also be similarly situated in respect of the canal Aa; and let them be placed at an infinite distance from each other, or at so great an one, that the repulsion of either body on the fluid in the canal shall not be sensibly less than if they were at an infinite distance: then, if the electric attraction and repulsion is inversely as the n power of the distance, n being greater than 1, and less than 3, the quantity of redundant fluid in the two bodies will be to each other as the $n-1$ power of their corresponding diameters AF and af.

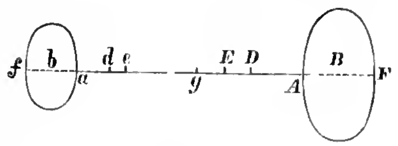


Fig. 13.

For if the quantity of redundant fluid in the two bodies is in this proportion, the repulsion of one body on the fluid in the canal will be equal to that of the other body on it in the contrary direction; and, consequently, the fluid will have no tendency to flow from one body to the other, as may thus be proved. Take the points D and E very near to each other; and take da to DA, and ea to EA, as af to AF; the repulsion of the body B on a particle at D, will be to the repulsion of b on a particle at d, as $\frac{1}{AF}$ to $\frac{1}{af}$; for, as the fluid is disposed similarly in both bodies, the quantity of fluid in any small part of B, is to the quantity in the corresponding part of b, as AF^{n-1} to af^{n-1}; and consequently the repulsion of that small part of B, on D, is to the repulsion of the corresponding part of b, on d, as $\frac{AF^{n-1}}{AF^n}$, or $\frac{1}{AF}$, to $\frac{1}{af}$. But the quantity of fluid in the small part DE of the canal, is to that in de, as DE to de, or as AF to af; therefore the repulsion of B on the fluid in DE, is equal to that of b on the fluid in de: therefore, taking ag to Aa, as af to AF, the repulsion of b on the fluid in ag, is equal to that of B on the fluid in Aa; but the repulsion of b on ag may be considered as the same as its repulsion on Aa; for, by the supposition, the repulsion of B on Aa may be considered as the same as if it was continued infinitely; and therefore, the repulsion of b on ag may be considered as the same as if it was continued infinitely.

N.B. If n was not greater than 1, it would be impossible for the length of Aa to be so great, that the repulsion of B on it might be considered as the same as if it was continued infinitely; which was my reason for requiring n to be greater than 1.

72] COR. By just the same method of reasoning it appears, that if the bodies are undercharged, the quantity of deficient fluid in b will be to that in B, as af^{n-1} to AF^{n-1}.

73] PROP. XXI. Let a thin flat plate be connected to any other body, as in the preceding proposition, by a canal of incompressible fluid, perpendicular to the plane of the plate; and let that body be overcharged, the quantity of redundant fluid in the plate will bear very nearly the same proportion to that in the other body, whatever the thickness of the plate may be, provided its thickness is very small in proportion to its breadth, or smallest diameter.

For there can be no doubt, but what, under that restriction, the fluid will be disposed very nearly in the same manner in the plate, whatever its thickness may be; and therefore its repulsion on the fluid in the canal will be very nearly the same, whatever its thickness may be. [See Exp. IV, Art. 272.]

74] PROP. XXII. Let AB and DF (Fig. 14) represent two equal and parallel circular plates, whose centers are C and E; let the plates be placed so, that a right line joining their centers shall be perpendicular to the plates; let the thickness of the plates be very small in respect of their distance CE; let the plate AB communicate with the body H, and the plate DF with the body L, by the canals CG and EM of incompressible fluid, such as are described in Prop. XIX; let these canals meet their

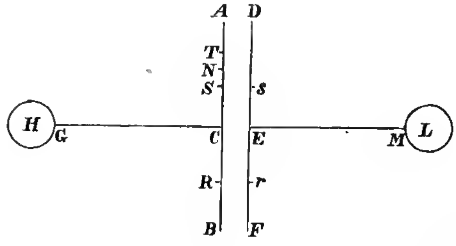


Fig. 14.

respective plates in their centers C and E, and be perpendicular to the plane of the plates; and let their length be so great, that the repulsion of the plates on the fluid in them may be considered as the same as if they were continued infinitely; let the body H be overcharged, and let L be saturated. It is plain, from Prop. XII, that DF will be undercharged, and AB will be more overcharged than it would otherwise be. Suppose, now, that the redundant fluid in AB is disposed in the same manner as the deficient fluid is in DF; let P be to one as the force with which the plate AB would repel the fluid in CE, if the canal ME was continued to C, is to the force with which it would repel the fluid in CM; and let the force with which AB repels the fluid in CG, be to the force with which it would repel it, if the redundant fluid in it was spread uniformly, as π to 1; and let the force with which the body H repels the fluid in CG, be the same with which a quantity of redundant fluid, which we will call B, spread uniformly over AB, would repel it in the contrary direction. Then will the redundant fluid in AB be equal to $\frac{B}{2P\pi - P^2\pi}$, and therefore, if P is very small, will be very nearly equal to $\frac{B}{2P\pi}$; and the deficient fluid in DF will be to the redundant fluid in AB, as $1 - P$ to 1, and therefore, if P is very small, will be very nearly equal to the redundant fluid in AB.

For it is plain, that the force with which AB repels the fluid in EM, must be equal to that with which DF attracts it; for otherwise, some fluid would run out of DF into L, or out of L into DF: for the same reason, the excess of the repulsion of AB on the fluid in CG, above the attraction of FD thereon, must be equal to the force with which a quantity of redundant fluid equal to B, spread uniformly over AB, would repel it, or it must be equal to that with which a quantity equal to $\frac{B}{\pi}$, spread in the manner in which the redundant fluid is actually spread in AB, would repel it. By the supposition, the force with which AB repels the fluid in EM, is to the force with which it would repel the fluid in CM, supposing EM to be continued to C, as $1 - P$ to 1; but the force with which any quantity of fluid in AB would repel the fluid in CM, is the same with which an equal quantity similarly disposed in DF, would repel the fluid in EM; therefore the force with which the redundant fluid in AB repels the fluid in EM, is to that with which an equal quantity similarly disposed in DF, would repel it, as $1 - P$ to 1: therefore, if the redundant fluid in AB be called A, the deficient fluid in DF must be $A \times 1 - P$: for the same reason, the force with which DF attracts the fluid in CG, is to that with which AB repels it, as $A \times 1 - P \times 1 - P$, or $A \times (1 - P)^2$, to A; therefore, the excess of the force with which AB repels CG above that with which DF attracts it, is equal to that with which a quantity of redundant fluid equal to $A - A \times (1 - P)^2$, or $A \times (2P - P^2)$, spread over AB, in the manner in which the redundant fluid therein is actually spread, would repel it: therefore $A \times (2P - P^2)$ must be equal to $\frac{B}{\pi}$, or A must be equal to $\frac{B}{2P\pi - P^2\pi}$.

75] COR. I. If the density of the redundant fluid near the middle of the plate AB, is less than the mean density, or the density which it would everywhere be of, if it was spread uniformly, in the ratio of δ to 1; and if the distance of the two plates is so small, that EC^{n-1} is very small in respect of AC^{n-1}, and that EC^{3-n} is very small in respect of AC^{3-n}, the quantity of redundant fluid in AB will be greater than $\frac{B}{2} \times \left.\frac{AC}{EC}\right|^{3-n}$, and less than $\frac{B}{2\delta} \times \left.\frac{AC}{EC}\right|^{3-n}$, but will approach much nearer to the latter value than the former. For, in this case, $P\pi$ is, by Lemma X, Corol. IV, less than $\left.\frac{EC}{AC}\right|^{3-n}$, and greater than $\left.\frac{EC}{AC}\right|^{3-n} \times \delta$, but approaches much nearer to the latter value than the former; and if EC^{3-n} is very small in respect of AC^{3-n}, P is very small.

76] REMARKS. If DF was not undercharged, it is certain that AB would be considerably more overcharged near the circumference of the circle than near the center; for if the fluid was spread uniformly, a particle placed anywhere at a distance from the center, as at N, would be repelled with considerably more force towards the circumference than it would towards the center. If the plates are very near together, and, consequently, DF nearly as much undercharged as AB is overcharged, AB will still be more overcharged near the circumference than near the center, but the difference will not be near so great as in the former case: for, let NR be many times greater than CE, and NS less than CE; and take Er and Es equal to CR and CS; there can be no doubt, I think, but that the deficient fluid in DF will be lodged nearly in the same manner as the redundant fluid in AB; and therefore, the repulsion of the redundant fluid at R, on a particle at N, will be very nearly balanced by the attraction of the redundant matter at r, for R is not much nearer to N than r is; but the repulsion of S will not be near balanced by that of s; for the distance of S from N is much less than that of s. Let now a small circle, whose diameter is ST, be drawn round the center N, on the plane of the plate; as the density of the fluid is greater at T than at S, the repulsion of the redundant fluid within the small circle tends to impel the point N towards C; but as there is a much greater quantity of fluid between N and B, than between N and A, the repulsion of the fluid without the small circle tends to balance that; but the effect of the fluid within the small circle is not much less than it would be, if DF was not undercharged; whereas much the greater part of the effect of that part of the plate on the outside of the circle, is taken off by the effect of the corresponding part of DF: consequently, the difference of density between T and S will not be near so great as if DF was not undercharged. Hence I should imagine, that if the two plates are very near together, the density of the redundant fluid near the center will not be much less than the mean density, or δ will not be much less than 1; moreover, the less the distance of the plates, the nearer will δ approach to 1.

77] Cor. II. Let now the body H consist of a circular plate, of the same size as AB, placed so, that the canal CG shall pass through its center, and be perpendicular to its plane; by the supposition, the force with which H repels the fluid in the canal CG, is the same with which a quantity of fluid, equal to B, spread uniformly over AB, would repel it in the contrary direction: therefore, if the fluid in the plate H was spread uniformly, the quantity of redundant fluid therein would be B, and if it was all collected in the circumference, would be $\frac{2B}{3-n}$; and therefore the real quantity will be greater than B, and less than $\frac{2B}{3-n}$.

78] Cor. III. Therefore, if we suppose δ to be equal to 1, the quantity of redundant fluid in AB will exceed that in the plate H, in a greater ratio than that of $\overline{\frac{AC}{CE}}\Big|^{3-n} \times \frac{3-n}{4}$ to 1, and less than that of $\overline{\frac{AC}{CE}}\Big|^{3-n} \times \frac{1}{2}$ to 1; and from the preceding remarks it appears that the real quantity of redundant fluid in AB can hardly be much greater than it would if δ was equal to 1.

79] Cor. IV. Hence, if the electric attraction and repulsion is inversely as the square of the distance, the redundant fluid in AB, supposing δ to be equal to 1, will exceed that in the plate H, in a greater ratio than that of AC to $4CE$, and less than that of AC to $2CE$.

80] Cor. V. Let now the body H consist of a globe, whose diameter equals AB; the globe being situated in such a manner, that the canal CG, if continued, would pass through its center; and let the electric attraction and repulsion be inversely as the square of the distance, the quantity of redundant fluid in the globe will be $2B$: for the fluid will be spread uniformly over the surface of the globe, and its repulsion on the canal will be the same as if it was all collected in the center of the sphere, and will therefore be the same with which an equal quantity, disposed in the circumference of AB, would repel it in the contrary direction, or with which half that quantity, or B, would repel it, if spread uniformly over the plate. [See Art. 140.]

81] Cor. VI. Therefore, if δ was equal to 1, the redundant fluid in AB would exceed that in the globe, in the ratio of AC to $4CE$; and therefore, it will in reality exceed that in the globe, in a rather greater ratio than that of AC to $4CE$; but if the plates are very near together, it will approach very near thereto, and the nearer the plates are, the nearer it will approach thereto.

82] Cor. VII. Whether the electric repulsion is inversely as the square of the distance or not, if the body H is as much undercharged, as it was before overcharged, AB will be as much undercharged as it was before overcharged, and DF as much overcharged as it was before undercharged.

83] Cor. VIII. If the size and distance of the plates be altered, the quantity of redundant or deficient fluid in the body H remaining the same, it appears, by comparing this proposition with the 20th and 21st propositions, that the quantity of redundant and deficient fluid in AB will be as $AC^{n-1} \times \left|\frac{\overline{AC}}{EC}\right|^{3-n}$, or as $\frac{AC^2}{EC^{3-n}}$, supposing the value of δ to remain the same*.

84] Prop. XXIII. Let AE (Fig. 15) be a cylindric canal, infinitely continued beyond E; and let AF be a bent canal, meeting the other at A, and infinitely continued beyond F: let the section of this canal, in all parts of it, be equal to that of the cylindric canal, and let both canals be filled with uniform fluid of the same density: the force with which a particle of fluid P, placed anywhere at pleasure, repels the whole quantity of fluid in AF, in the direction of the canal, is the same with which it repels the fluid in the canal AE, in the direction AE.

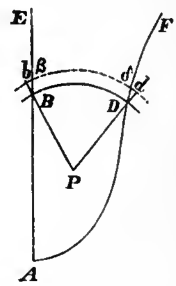


Fig. 15.

On the center P, draw two circular arches BD and bd, infinitely near to each other, cutting AE in B and β, and AF in D and δ, and draw the radii Pb and Pd. As $PB = PD$, the force with which P repels a particle at B, in the direction $B\beta$, is to that with which it repels an equal particle at D, in the direction $D\delta$, as $\frac{Bb}{B\beta}$ to $\frac{Dd}{D\delta}$, or as $\frac{1}{B\beta}$ to $\frac{1}{D\delta}$; and therefore, the force with which it repels the whole fluid in $B\beta$, in the direction $B\beta$, is the same with which it repels the whole fluid in $D\delta$, in the direction $D\delta$, that is in the direction of the canal; and therefore, the force with which it repels the whole fluid in AE, in the direction AE, is the same with which it repels the whole fluid in AF, in the direction of the canal.

85] Cor. If the bent canal ADF, instead of being infinitely continued, meets the cylindric canal in E, as in Fig. 16, the repulsion of P on the fluid in the bent canal ADE, in the direction of the canal, will still be equal to its repulsion on that in the cylindric canal AE, in the direction AE.

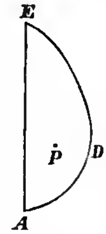


Fig. 16.

86] Prop. XXIV. If two bodies, for instance the plate AB, and the body H, of Prop. XXII, communicate with each other, by a canal filled with incompressible fluid, and are either over or undercharged, the quantity of redundant fluid in them will bear the same proportion to each other, whether the canal by which they communicate is straight or crooked, or

[* Note 4, p. 368.]

into whatever part of the bodies the canal is inserted, or in whatever manner the two bodies are situated in respect of each other; provided that their distance is infinite, or so great that the repulsion of each body on the fluid in the canal shall not be sensibly less than if it was infinite.

Let the parallelograms *AB* and *DF* (Fig. 17) represent the two plates, and *H* and *L* the bodies communicating with them: let now *H* be removed to *h*; and let it communicate with *AB* by the bent canal *gc*; the quantity of fluid in the plates and bodies remaining the same as before; and let us, for the sake of ease in the demonstration, suppose the canal *gc* to be everywhere of the same thickness as the canal *GC*; though the proposition will evidently hold good equally, whether it is or not: the fluid will still be in equilibrio. For let us first suppose the canal *gc* to be

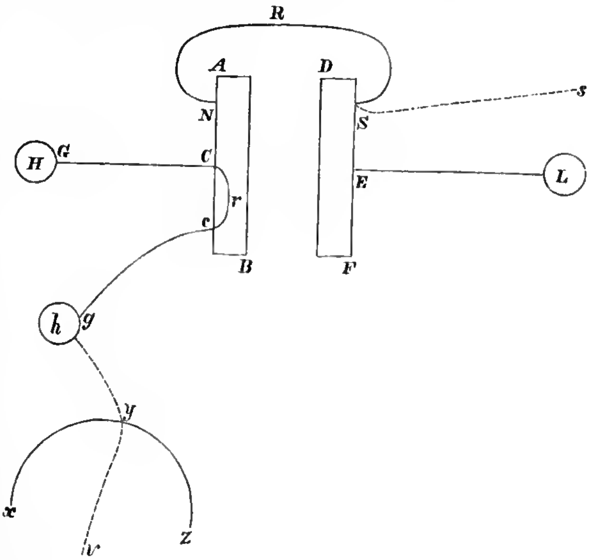


Fig. 17

continued through the substance of the plate *AB*, to *C*, along the line *crC*; the part *crC* being of the same thickness as the rest of the canal, and the fluid in it of the same density: by the preceding proposition, the repulsion or attraction of each particle of fluid or matter in the plates *AB* and *DF*, on the fluid in the whole canal *Crcg*, in the direction of that canal, is equal to its repulsion or attraction on the fluid in the canal *CG*, in the direction *CG*; and therefore the whole repulsion or attraction of the two plates on the canal *Crcg*, is equal to their repulsion or attraction on *CG*: but as the fluid in the plate *AB* is in equilibrio, each particle of fluid in the part *Crc* of the canal is impelled by the plates with as much force in one direction as the other; and consequently the plates impel the fluid in the canal *cg* with as much force as they do that in the whole canal *Crcg*, that is, with the same force that they impel the fluid in *CG*. In like manner the body *h* impels the fluid in *cg* with the same force that *H* does the fluid in *CG*; and consequently *h* impels the fluid in *cg* one way in the direction of the canal, with the same force that the two plates impel it the contrary way; and therefore the fluid in *cg* has no tendency to flow from one body to the other.

87] COR. By the same method of reasoning, with the help of the corollary to the 23rd proposition, it appears, that if *AB* and *H* each communicate with a third body by canals of incompressible fluid, and a communication is made between *AB* and *H* by another canal of incompressible fluid, the fluid will have no tendency to flow from one to the other through this canal; supposing that the fluid was in equilibrio before this communication was made. In like manner if *AB* and *H* communicate with each other, or each communicate with a third body, by canals of real fluid, instead of the imaginary canals of incompressible fluid used in these propositions, and a communication is also made between them by a canal of incompressible fluid, the fluid can have no tendency to flow from one to the other. The truth of the latter part of this corollary will appear by supposing an imaginary canal of incompressible fluid to be continued through the whole length of the real one.

88] PROP. XXV. Let now a communication be made between the two plates *AB* and *DF*, by the canal *NRS* of incompressible fluid, of any length; and let the body *H* and the plate *AB* be overcharged. It is plain that the fluid will flow through that canal from *AB* to *DF*. Now the whole force with which the fluid in the canal is impelled along it by the joint action of the two plates is the same with which the whole quantity of fluid in the canal *CG* or *cg* is impelled by them; supposing the canal *NRS* to be everywhere of the same breadth and thickness as *CG* or *cg*.

For suppose that the canal *NRS*, instead of communicating with the plate *DF*, is bent back just before it touches it, and continued infinitely along the line *Ss*; the force with which the two plates impel the fluid in *Ss*, is the same with which they impel that in *EL*, supposing *Ss* to be of the same breadth and thickness as *EL*; and is therefore nothing; therefore the force with which they impel the fluid in *NRS*, is the same with which they impel that in *NRSs*; which is the same with which they impel that in *CG*.

89] PROP. XXVI. Let now *xyz* [Fig. 17] be a body of an infinite size, containing just fluid enough to saturate it; and let a communication be made between *h* and *xyz*, by the canal *hy* of incompressible fluid, of the same breadth and thickness as *gc* or *GC*; the fluid will flow through it from *h* to *xyz*; and the force with which the fluid in that canal is impelled along it, is equal to that with which the fluid in *NRS* is impelled by the two plates.

If the canal *hy* is of so great a length, that the repulsion of *h* thereon is the same as if it was continued infinitely, then the thing is evident: but if it is not, let the canal *hy*, instead of communicating with *xyz*, so that the fluid can flow out of the canal into *xyz*, be continued infinitely through its substance, along the line *yv*: now it must be observed that a small part of the body *xyz*, namely, that which is turned towards *h*, will by the action of *h* upon it, be rendered undercharged; but all the rest of the body will be saturated; for the fluid driven out of the undercharged part will not make the remainder, which is supposed to be of an infinite size, sensibly overcharged: now the force with which the fluid in the infinite canal *hyv* is impelled by the body *h* and the undercharged part of *xyz*, is the same with which the fluid in *gc* is impelled by them; but as the fluid in all parts of *xyz* is in equilibrio, a particle in any part of *yv* cannot be impelled in any direction; and therefore the fluid in *hy* is impelled with as much force as that in *hyv*; and therefore the fluid in *hy* is impelled with as much force as that in *gc*; and is therefore impelled with as much force as the fluid in *NRS* is impelled by the two plates.

90] It perhaps may be asked, whether this method of demonstration would not equally tend to prove that the fluid in *hy* was impelled with the same force as that in *NRS*, though *xyz* did not contain just fluid enough to saturate it. I answer not; for this demonstration depends on the canal *yv* being continued, within the body *xyz*, to an infinite distance beyond any over or undercharged part; which could not be if *xyz* contained either more or less fluid than that*.

91] PROP. XXVII. Let two bodies *B* and *b* (Fig. 13) be joined by a cylindric or prismatic canal *Aa*, filled with real fluid; and not by any imaginary canal of incompressible fluid as in the 20th proposition; and let the fluid therein be in equilibrio: the force with which the whole or any given part of the fluid in the canal is impelled in the direction of its axis by the united repulsions and attractions of the redundant fluid or matter in the two bodies and the canal, must be nothing; or the force with which it is impelled one way in the direction of the axis of the canal, must be equal to that with which it is impelled the other way.

For as the canal is supposed cylindric or prismatic, no particle of fluid therein can be prevented from moving in the direction of the axis of it, by the sides of the canal; and therefore the force with which each particle is impelled either way in the direction of the axis, by the united attractions and repulsions of the two bodies and the canal, must be nothing, otherwise it could not be at rest; and therefore the force with which the whole, or any given part of the fluid in the canal, is impelled in the direction of the axis, must be nothing.

92] COR. I. If the fluid in the canal is disposed in such manner, that the repulsion or attraction of the redundant fluid or matter in it, on the

[* Note 5, p. 369.]

whole or any given part of the fluid in the canal, has no tendency to impel it either way in the direction of the axis; then the force with which that whole or given part is impelled by the two bodies must be nothing; or the force with which it is impelled one way in the direction of the axis, by the body B, must be equal to that with which it is impelled in the contrary direction by the other body; but not if the fluid in the canal is disposed in a different manner.

93] COR. II. If the bodies, and consequently the canal, is overcharged; then, in whatever manner the fluid in the canal is disposed, the force with which the whole quantity of redundant fluid in the canal is repelled by the body B in the direction Aa, must be equal to that with which it is repelled by b in the contrary direction. For the force with which the redundant fluid is impelled in the direction Aa by its own repulsion, is nothing; for the repulsions of the particles of any body on each other have no tendency to make the whole body move in any direction.

94] REMARKS. When I first thought of the 20th and 22nd propositions, I imagined that when two bodies were connected by a cylindric canal of real fluid, the repulsion of one body on the whole quantity of fluid in the canal, in one direction, would be equal to that of the other body on it in the contrary direction, in whatever manner the fluid was disposed in the canal; and that therefore those propositions would have held good very nearly, though the bodies were joined by cylindric canals of real fluid; provided the bodies were so little over or undercharged, that the quantity of redundant or deficient fluid in the canal should be very small in respect of the quantity required to saturate it; and consequently that the fluid therein should be very nearly of the same density in all parts. But from the foregoing proposition it appears that I was mistaken, and that the repulsion of one body on the fluid in the canal is not equal to that of the other body on it, unless the fluid in the canal is disposed in a particular manner: besides that, when two bodies are both joined by a real canal, the attraction or repulsion of the redundant matter or fluid in the canal has some tendency to alter the disposition of the fluid in the two bodies; and in the 22nd proposition, the canal CG exerts also some attraction or repulsion on the canal EM: on all which accounts the demonstration of those propositions is defective, when the bodies are joined by real canals. I have good reason however to think, that those propositions actually hold good very nearly when the bodies are joined by real canals; and that, whether the canals are straight or crooked, or in whatever direction the bodies are situated in respect of each other: though I am by no means able to prove that they do: I therefore chose still to retain those propositions, but to demonstrate them on this ideal supposition, in which they are certainly true, in hopes that some more skilful mathematician may be able to shew whether they really hold good or not. [See Note 3.]

95] What principally makes me think that this is the case, is that as far as I can judge from some experiments I have made*, the quantity of fluid in different bodies agrees very well with those propositions, on a supposition that the electric repulsion is inversely as the square of the distance. It should also seem from those experiments, that the quantity of redundant or deficient fluid in two bodies bore very nearly the same proportion to each other, whatever is the shape of the canal by which they are joined, or in whatever direction they are situated in respect of each other.

96] Though the above propositions should be found not to hold good when the bodies are joined by real canals, still it is evident, that in the 22nd proposition, if the plates AB and DF are very near together, the quantity of redundant fluid in the plate AB will be many times greater than that in the body H, supposing H to consist of a circular plate of the same size as AB, and DF will be near as much undercharged as AB is overcharged.

97] Sir Isaac Newton supposes that air consists of particles which repel each other with a force inversely as the distance: but it appears plainly from the foregoing pages, that if the repulsion of the particles was in this ratio, and extended indefinitely to all distances, they would compose a fluid extremely different from common air. If the repulsion of the particles was inversely as the distance, but extended only to a given very small distance from their centers, they would compose a fluid of the same kind as air, in respect of elasticity, except that its density would not be in proportion to its compression: if the distance to which the repulsion extends, though very small, is yet many times greater than the distance of the particles from each other, it might be shewn, that the density of the fluid would be nearly as the square root of the compression. If the repulsion of the particles extended indefinitely, and was inversely as some higher power of the distance than the cube, the density of the fluid would be as some power of the compression less than $\frac{3}{5}$. The only law of repulsion, I can think of, which will agree with experiment, is one which seems not very likely; namely, that the particles repel each other with a force inversely as the distance; but that, whether the density of the fluid is great or small, the repulsion extends only to the nearest particles: or, what comes to the same thing, that the distance to which the repulsion extends, is very small, and also is not fixed, but varies in proportion to the distance of the particles†.

[* Exp. III, Art. 265.] [† Note 6, p. 370.]

An attempt to explain some of the Principal Phænomena of Electricity, by means of an Elastic Fluid—Part II

Containing a Comparison of the Foregoing Theory with Experiment.

98] § 1. It appears from experiment, that some bodies suffer the electric fluid to pass with great readiness between their pores; while others will not suffer it to do so without great difficulty; and some hardly suffer it to do so at all. The first sort of bodies are called conductors, the others non-conductors. What this difference in bodies is owing to I do not pretend to explain.

It is evident that the electric fluid in conductors may be considered as moveable, or answers to the definition given of that term in page 6. As to the fluid contained in non-conducting substances, though it does not absolutely answer to the definition of immoveable, as it is not absolutely confined from moving, but only does so with great difficulty; yet it may in most cases be looked upon as such without sensible error.

99] Air does in some measure permit the electric fluid to pass through it; though, if it is dry, it lets it pass but very slowly, and not without difficulty; it is therefore to be called a non-conductor.

It appears that conductors would readily suffer the fluid to run in and out of them, were it not for the air which surrounds them: for if the end of a conductor is inserted into a vacuum, the fluid runs in and out of it with perfect readiness; but when it is surrounded on all sides by the air, as no fluid can run out of it without running into the air, the fluid will not do so without difficulty.

100] If any body is surrounded on all sides by the air, or other non-conducting substances, it is said to be insulated: if on the other hand it anywhere communicates with any conducting body, it is said to be not insulated. When I say that a body communicates with the ground, or any other body, I would be understood to mean that it does so by some conducting substance.

101] Though the terms positively and negatively electrified are much used, yet the precise sense in which they are to be understood seems not well ascertained; namely, whether they are to be understood in the same sense in which I have used the words over or undercharged, or whether, when any number of bodies, insulated and communicating with each other by conducting substances, are electrified by means of excited glass, they are all to be called positively electrified (supposing, according to the usual opinion, that excited glass contains more than its natural quantity of electricity); even though some of them, by the approach of a stronger electrified body, are made undercharged. I shall use the words in the latter sense; but as it will be proper to ascertain the sense in which I shall use them more accurately, I shall give the following definition.

102] In order to judge whether any body, as A, is positively or negatively electrified: suppose another body B, of a given shape and size, to be placed at an infinite distance from it, and from any other over or undercharged body; and let B contain the same quantity of electric fluid as if it communicated with A by a canal of incompressible fluid: then, if B is overcharged, I call A positively electrified; and if it is undercharged, I call A negatively electrified; and the greater the degree in which B is over or undercharged, the greater is the degree in which A is positively or negatively electrified.

103] It appears from the corollary to the 24th proposition, that if several bodies are insulated, and connected together by conducting substances, and one of these bodies is positively or negatively electrified, all the other bodies must be electrified in the same degree: for supposing a given body B to be placed at an infinite distance from any over or undercharged body, and to contain the same quantity of fluid as if it communicated with one of those bodies by a canal of incompressible fluid, all the rest of those bodies must by that corollary contain the same quantity of fluid as if they communicated with B by canals of incompressible fluid: but yet it is possible that some of those bodies may be overcharged, and others undercharged: for suppose the bodies to be positively electrified, and let an overcharged body D be brought near one of them, that body will become undercharged, provided D is sufficiently overcharged; and yet by the definition it will still be positively electrified in the same degree as before.

Moreover, if several bodies are insulated and connected together by conducting substances, and one of these bodies is electrified by excited glass, there can be no doubt, I think, but what they will all be positively electrified; for if there is no other over or undercharged body placed near any of these bodies, the thing is evident; and though some of these bodies may, by the approach of a sufficiently overcharged body, be rendered undercharged; yet I do not see how it is possible to prevent a body placed at an infinite distance, and communicating with them by a canal of incompressible fluid, from being overcharged.

In like manner if one of these bodies is electrified by excited sealing wax, they will all be negatively electrified*.

104] It is impossible for any body communicating with the ground to be either positively or negatively electrified: for the earth, taking the

[* Note 7, p. 372.]

whole together, contains just fluid enough to saturate it, and consists in general of conducting substances; and consequently though it is possible for small parts of the surface of the earth to be rendered over or undercharged, by the approach of electrified clouds or other causes; yet the bulk of the earth, and especially the interior parts, must be saturated with electricity. Therefore assume any part of the earth which is itself saturated, and is at a great distance from any over or undercharged part; any body communicating with the ground, contains as much electricity as if it communicated with this part by a canal of incompressible fluid, and therefore is not at all electrified.

105] If any body A, insulated and saturated with electricity, is placed at a great distance from any over or undercharged body, it is plain that it cannot be electrified; but if an overcharged body is brought near it, it will be positively electrified; for supposing A to communicate with any body B, at an infinite distance, by a canal of incompressible fluid, it is plain that unless B is overcharged, the fluid in the canal could not be in equilibrio, but would run from A to B. For the same reason a body insulated and saturated with fluid, will be negatively electrified if placed near an undercharged body.

106] § 2. The phænomena of the attraction and repulsion of electrified bodies seem to agree exactly with the theory; as will appear by considering the following cases.

107] Case I. Let two bodies, A and B, both conductors of electricity, and both placed at a great distance from any other electrified bodies, be brought near each other. Let A be insulated, and contain just fluid enough to saturate it; and let B be positively electrified. They will attract each other; for as B is positively electrified, and at a great distance from any overcharged body, it will be overcharged; therefore, on approaching A and B to each other, some fluid will be driven from that part of A which is nearest to B to the further part: but when the fluid in A was spread uniformly, the repulsion of B on the fluid in A was equal to its attraction on the matter therein; therefore, when some fluid is removed from those parts where the repulsion of B is strongest to those where it is weaker, B will repel the fluid in A with less force than it attracts the matter; and consequently the bodies will attract each other.

108] Case II. If we now suppose that the fluid is at liberty to escape from out of A, if it has any disposition to do so, the quantity of fluid in it before the approach of B being still sufficient to saturate it; that is, if A is not insulated and not electrified, B being still positively electrified, they will attract with more force than before: for in this case, not only some fluid will be driven from that part of A which is nearest to B to the opposite part, but also some fluid will be driven out of A.

It must be observed, that if the repulsion of B on a particle at E, (Fig. 19) the farthest part of A, is very small in respect of its repulsion on an equal particle placed at D, the nearest part of A, the two bodies will attract with very nearly the same force, whether A is insulated or not; but if the repulsion of B, on a particle at E, is very near as great as on one at D, they will attract with very little force if A is insulated. For instance, let a small overcharged ball be brought near one end of a long conductor not electrified; they will attract with very near the same force, whether the conductor be insulated or not; but if the conductor be overcharged, and brought near a small unelectrified ball, they will not attract with near so much force, if the ball is insulated, as if it is not.

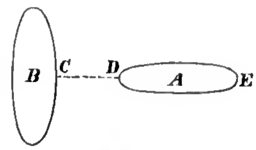


Fig. 19.

109] CASE III. If we now suppose that A is negatively electrified, and not insulated, it is plain that they will attract with more force than in the last case; as A will be still more undercharged in this case, than in the last.

110] N.B. In these three cases, we have not as yet taken notice of the effect which the body A will have in altering the quantity and disposition of the fluid in B; but in reality this will make the bodies attract each other with more force than they would otherwise do; for in each of these cases the body A attracts the fluid in B; which will cause some fluid to flow from the farther parts of B to the nearer, and will also cause some fluid to flow into it, if it is not insulated, and will consequently cause B to act upon A with more force than it would otherwise do.

111] CASES IV, V, VI. Let us now suppose that B is negatively electrified; and let A be insulated, and contain just fluid enough to saturate it; they will attract each other; for B will be undercharged; it will therefore attract the fluid in A, and will cause some fluid to flow from the farthest part of A, where it is attracted with less force, to the nearer part, where it is attracted with more force; so that B will attract the fluid in A with more force than it repels the matter.

If A is now supposed to be not insulated and not electrified, B being still negatively electrified, it is plain that they will attract with more force than in the last case: and if A is positively electrified, they will attract with still more force.

In these three last cases also, the effect which A has in altering the quantity and disposition of the fluid in B, tends to increase the force with which the two bodies attract.

112] CASE VII. It is plain that a non-conducting body saturated with fluid, is not at all attracted or repelled by an over or undercharged body, until, by the action of the electrified body on it, it has either acquired some additional fluid from the air, or had some driven out of it, or till some fluid is driven from one part of the body to the other.

113] CASE VIII. Let us now suppose that the two bodies A and B are both positively electrified in the same degree. It is plain, that were it not for the action of one body on the other, they would both be overcharged, and would repel each other. But it may perhaps be said, that one of them as A may, by the action of the other on it, be either rendered undercharged on the whole, or at least may be rendered undercharged in that part nearest to B; and that the attraction of this undercharged part on a particle of the fluid in B, may be greater than the repulsion of the more distant overcharged part; so that on the whole the body A may attract a particle of fluid in B. If so, it must be affirmed that the body B repels the fluid in A; for otherwise, that part of A which is nearest to B could not be rendered undercharged. Therefore, to obviate this objection, let the bodies by joined by the straight canal DC of incompressible fluid (Fig. 19). The body B will repel the fluid in all parts of this canal; for as A is supposed to attract the fluid in B, B will not only be more overcharged than it would otherwise be, but it will also be more overcharged in that part nearest to A than in the opposite part. Moreover, as the near undercharged part of A is supposed to attract a particle of fluid in B with more force than the more distant overcharged part repels it; it must, *a fortiori*, attract a particle in the canal with more force than the other repels it; therefore the body A must attract the fluid in the canal; and consequently some fluid must flow from B to A, which is impossible; for as A and B are both electrified in the same degree, they contain the same quantity of fluid as if they both communicated with a third body at an infinite distance, by canals of incompressible fluid; and therefore, by the corollary to Prop. 24, if a communication is made between them by a canal of incompressible fluid, the fluid would have no disposition to flow from one to the other.

114] CASE IX. But if one of the bodies as A is positively electrified in a less degree than B, then it is possible for the bodies to attract each other; for in this case the force with which B repels the fluid in A may be so great, as to make the body A either intirely undercharged, or at least to make the nearest part of it so much undercharged, that A shall on the whole attract a particle of fluid in B.

It may be worth remarking with regard to this case, that when two bodies, both electrified positively but unequally, attract each other, you may by removing them to a greater distance from each other, cause them to repel; for as the stronger electrified body repels the fluid in the weaker with less force when removed to a greater distance, it will not be able to drive so much fluid out of it, or from the nearer to the further part, as when placed at a less distance.

115] Cases X and XI. By the same reasoning it appears, that if the two bodies are both negatively electrified in the same degree, they must repel each other: but if they are both negatively electrified in different degrees, it is possible for them to attract each other. .

All these cases are exactly conformable to experiment.

116] Case XII. Let two cork balls be suspended by conducting threads from the same positively electrified body, in such manner that if they did not repel, they would hang close together: they will both be equally electrified, and will repel each other: let now an overcharged body, more strongly electrified than them, be brought under them; they will become less overcharged, and will separate less than before: on bringing the body still nearer, they will become not at all overcharged, and will not separate at all: and on bringing the body still nearer, they will become undercharged, and will separate again.

117] Case XIII. Let all the air of a room be overcharged, and let two cork balls be suspended close to each other by conducting threads communicating with the wall. By Prop. 15, it is highly probable that the balls will be undercharged; and therefore they should repel each other.

These two last cases are experiments of Mr Canton's, and are described in *Philosophical Transactions* 1753, p. 350, where are other experiments of the same kind, all readily explicable by the foregoing theory.

I have now considered all the principal or fundamental cases of electric attractions and repulsions which I can think of; all of which appear to agree perfectly with the theory*.

118] § 3. On the cases in which bodies receive electricity from or part with it to the air.

Lemma I. Let the body A (Fig. 6) either stand near some over or undercharged body, or at a distance from any. It seems highly probable, that if any part of its surface, as MN, is overcharged, the fluid will endeavour to run out through that part, provided the air adjacent thereto is not overcharged.

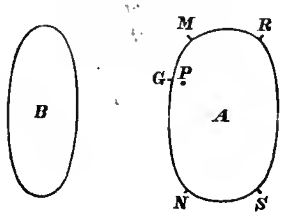


Fig. 6.

For let G be any point in that surface, and P a point within the body, extremely near to it; it is plain that a particle of fluid at P must be repelled with as much force in one direction as another (otherwise it could not be at rest) unless all the fluid between P and G is pressed close together, in which case it may be repelled with more force towards G than it is in the contrary direction: now a particle at G is repelled in the direction PG, *i.e.* from P to G, by all the redundant fluid between P and G; and a particle at P is repelled by the

[* Note 8, p. 373.]

same fluid in the contrary direction; so that as the particle at P is repelled with not less force in the direction PG than in the contrary, I do not see how a particle at G can help being repelled with more force in that direction than the contrary, unless the air on the outside of the surface MN was more overcharged than the space between P and G.

In like manner, if any part of the surface is undercharged, the fluid will have a tendency to run in at that part from the air.

The truth of this is somewhat confirmed by the third problem; as in all the cases of that problem, the fluid was shewn to have a tendency to run out of the spaces AD and EH, at any surface which was overcharged, and to run in at any which was undercharged.

119] Cor. I. If any body at a distance from other over or undercharged bodies be positively electrified, the fluid will gradually run out of it from all parts of its surface into the adjoining air; as it is plain that all parts of the surface of that body will be overcharged: and if the body is negatively electrified, the fluid will gradually run into it at all parts of its surface from the adjoining air.

120] Cor. II. Let the body A (Fig. 6) insulated and containing just fluid enough to saturate it, be brought near the overcharged body B; that part of the surface of A which is turned towards B will by Prop. II be rendered undercharged, and will therefore imbibe electricity from the air; and at the opposite surface RS, the fluid will run out of the body into the air.

121] Cor. III. If we now suppose that A is not insulated, but communicates with the ground, and consequently that it contained just fluid enough to saturate it before the approach of B, it is plain that the surface MN will be more undercharged than before; and therefore the fluid will run in there with more force than before; but it can hardly have any disposition to run out at the opposite surface RS; for if the canal by which A communicates with the ground is placed opposite to B, as in figure 5, then the fluid will run out through that canal till it has no longer any tendency to run out at RS; and by the remarks at the end of Prop. 27, it seems probable, that the fluid in A will be nearly in the same quantity, and disposed nearly in the same manner, into whatever part of A the canal is inserted by which it communicates with the ground.

122] Cor. IV. If B is undercharged the case will be reversed; that is, it will run out where it before run in, and will run in where it before run out.

As far as I can judge, these corollaries seem conformable to experiment: thus far is certain, that bodies at a distance from other electrified bodies receive electricity from the air, if negatively electrified, and part with some to it if positively electrified: and a body not electrified and not insulated receives electricity from the air if brought near an overcharged body, and loses some when brought near an undercharged body: and a body insulated and containing its natural quantity of fluid, in some cases, receives, and in others loses electricity, when brought near an over or undercharged body.

123] § 4. The well-known effects of points in causing a quick discharge of electricity seem to agree very well with this theory.

It appears from the 20th proposition, that if two similar bodies of different sizes are placed at a very great distance from each other, and connected by a slender canal, and overcharged, the force with which a particle of fluid placed close to corresponding parts of their surface is repelled from them, is inversely as the corresponding diameters of the bodies. If the distance of the two bodies is small, there is not so much difference in the force with which the particle is repelled by the two bodies; but still, if the diameters of the two bodies are very different, the particle will be repelled with much more force from the smaller body than from the larger. It is true indeed that a particle placed at a certain distance from the smaller body, will be repelled with less force than if it be placed at the same distance from the greater body; but this distance is, I believe, in most cases pretty considerable; if the bodies are spherical, and the repulsion inversely as the square of the distance, a particle placed at any distance from the surface of the smaller body less than a mean proportional between the radii of the two bodies, will be repelled from it with more force than if it be placed at the same distance from the larger body.

I think therefore that we may be well assured that if two similar bodies are connected together by a slender canal, and are overcharged, the fluid must escape faster from the smaller body than from an equal surface of the larger; but as the surface of the larger body is greatest, I do not know which body ought to lose most electricity in the same time; and indeed it seems impossible to determine positively from this theory which should, as it depends in great measure on the manner in which the air opposes the entrance of the electric fluid into it. Perhaps in some degrees of electrification the smaller body may lose most, and in others the larger.

124] Let now ACB (Fig. 18) be a conical point standing on any body DAB, C being the vertex of the cone; and let DAB be overcharged: I imagine that a particle of fluid placed close to the surface of the cone anywhere between b and C, must be repelled with at least as much, if not more, force than it would, if the part $AabB$ of the cone was taken away, and the part aCb connected to DAB by a slender canal; and consequently, from what has been said before, it seems reasonable to suppose that the waste of electricity from the end of the cone must be very great

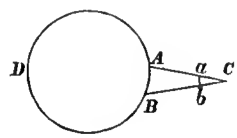


Fig. 18.

in proportion to its surface; though it does not appear from this reasoning whether the waste of electricity from the whole cone should be greater or less than from a cylinder of the same base and altitude*.

All which has been here said relating to the flowing out of electricity from overcharged bodies, holds equally true with regard to the flowing in of electricity into undercharged bodies.

125] But a circumstance which I believe contributes as much as any thing to the quick discharge of electricity from points, is the swift current of air caused by them, and taken notice of by Mr Wilson and Dr Priestly (*vide* Priestly, pp. 117 and 591); and which is produced in this manner.

If a globular body *ABD* is overcharged, the air close to it, all round its surface, is rendered overcharged by the electric fluid which flows into it from the body; it will therefore be repelled by the body; but as the air all round the body is repelled with the same force, it is in equilibrio, and has no tendency to fly off from it. If now the conical point *ACB* be made to stand out from the globe, as the fluid will escape much faster in proportion to the surface from the end of the point than from the rest of the body, the air close to it will be much more overcharged than that close to the rest of the body; it will therefore be repelled with much more force; and consequently a current of air will flow along the sides of the cone, from *B* towards *C*; by which means there is a continual supply of fresh air, not much overcharged, brought in contact with the point; whereas otherwise the air adjoining to it would be so much overcharged, that the electricity would have but little disposition to flow from the point into it.

The same current of air is produced in a less degree, without the help of the point, if the body, instead of being globular, is oblong or flat, or has knobs on it, or is otherwise formed in such manner as to make the electricity escape faster from some parts of it than the rest.

In like manner, if the body *ABD* be undercharged, the air adjoining to it will also be undercharged, and will therefore be repelled by it; but as the air close to the end of the point will be more undercharged than that close to the rest of the body, it will be repelled with much more force; which will cause exactly the same current of air, flowing the same way, as if the body was overcharged; and consequently the velocity with which the electric fluid flows into the body, will be very much increased. I believe indeed that it may be laid down as a constant rule, that the faster the electric fluid escapes from any body when overcharged, the faster will it run into that body when undercharged.

Points are not the only bodies which cause a quick discharge of electricity; in particular, it escapes very fast from the ends of long slender cylinders; and a swift current of air is caused to flow from the middle of the cylinder towards the end: this will easily appear by considering that

[* Note 9, p. 374.]

the redundant fluid is collected in much greater quantity near the ends of the cylinders than near the middle. The same thing may be said, but I believe in a less degree, of the edges of thin plates.

What has been just said concerning the current of air, serves to explain the reason of the revolving motion of Dr Hamilton's and Mr Kinnersley's bent pointed wires, vide *Philosophical Trans.* Vol. LI., p. 905, and Vol. LIII., p. 86; also Priestly, p. 429: for the same repulsion which impels the air from the thick part of the wire towards the point, tends to impel the wire in the contrary direction.

126] It is well known, that if a body B is positively electrified, and another body A, communicating with the ground, be then brought near it, the electric fluid will escape faster from B, at that part of it which is turned towards A, than before. This is plainly conformable to theory; for as A is thereby rendered undercharged, B will in its turn be made more overcharged, in that part of it which is turned towards A, than it was before. But it is also well known that the fluid will escape faster from B, if A be pointed, than if it be blunt; though B will be less overcharged in this case than in the other; for the broader the surface of A, which is turned towards B, the more effect will it have in increasing the overcharge of B. The cause of this phænomenon is as follows:

If A is pointed, and the pointed end turned towards B, the air close to the point will be very much undercharged, and therefore will be strongly repelled by A, and attracted by B, which will cause a swift current of air to flow from it towards B; by which means a constant supply of undercharged air will be brought in contact with B, which will accelerate the discharge of electricity from it in a very great degree: and moreover, the more pointed A is, the swifter will be this current. If, on the other hand, that end of A which is turned towards B is so blunt, that the electricity is not disposed to run into A faster than it is to run out of B, the air adjoining to B may be as much overcharged as that adjoining to A is undercharged; and therefore may by the joint repulsion of B and attraction of A, be impelled from B to A, with as much or more force than the air adjoining to A is impelled in the contrary direction; so that what little current of air there is may flow in the contrary direction.

It is easy applying what has been here said to the case in which B is negatively electrified.

127] § 5. In the paper of Mr Canton's, quoted in the second section, and in a paper of Dr Franklin's *Philosophical Transactions* 1755, p. 300, and Franklin's letters, p. 155, are some remarkable experiments, shewing that when an overcharged body is brought near another body, some fluid is driven to the further end of this body, and also some driven out of it, if it is not insulated. The experiments are all strictly conformable to the

11th, 12th, and 13th propositions: but it is needless to point out the agreement, as the explanation given by the authors does it sufficiently.

128] § 6. On the Leyden vial.

The shock produced by the Leyden vial seems owing only to the great quantity of redundant fluid collected on its positive side, and the great deficiency on its negative side; so that if a conductor was prepared of so great a size, as to be able to receive as much additional fluid by the same degree of electrification as the positive side of a Leyden vial, and was positively electrified in the same degree as the vial, I do not doubt but what as great a shock would be produced by making a communication between this conductor and the ground, as between the two surfaces of the Leyden vial, supposing both communications to be made by canals of the same length and same kind.

It appears plainly from the experiments which have been made on this subject, that the electric fluid is not able to pass through the glass; but yet it seems as if it was able to penetrate without much difficulty to a certain depth, perhaps I might say an imperceptible depth, within the glass; as Dr Franklin's analysis of the Leyden vial shews that its electricity is contained chiefly in the glass itself, and that the coating is not greatly over or undercharged.

It is well known that glass is not the only substance which can be charged in the manner of the Leyden vial; but that the same effect may be produced by any other body, which will not suffer the electricity to pass through it.

129] *Hence the phænomena of the vial seem easily explicable by means of the 22nd proposition. For let $ACGM$, Fig. 20, represent a flat plate of glass or any other substance which will not suffer the electric fluid to pass through it, seen edgeways; and let $BbdD$, and $EefF$, or Bd and Ef, as I shall call them for shortness, be two plates of conducting matter of the same size, placed in contact with the glass opposite to each other; and let Bd be positively electrified; and let Ef communicate with the ground; and let the fluid be supposed either able to enter a little way into the glass, but not to pass through it, or unable to enter it at all; and if it is able to enter a little way into it, let $b\beta\delta d$, or $b\delta$, as I shall call it, represent that part of the glass into which the fluid can enter from the plate Bd, and $e\phi$, that

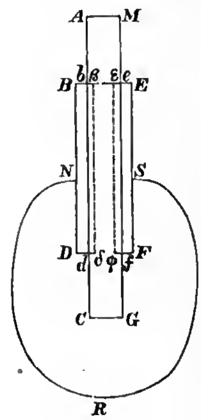


Fig. 20.

* The following explication is strictly applicable only to that sort of Leyden vial, which consists of a flat plate of glass or other matter. It is evident, however, that the result must be nearly of the same kind, though the glass is made into the shape of a bottle as usual, or into any other form; but I propose to consider those sort of Leyden vials more particularly in a future paper.

which the fluid from Ef can enter. By the above-mentioned proposition, if be, the thickness of the glass, is very small in respect of bd, the diameter of the plates, the quantity of redundant fluid forced into the space Bd, or $B\delta$, (that is, into the plate Bd, if the fluid is unable to penetrate at all into the glass, or into the plate Bd, and the space $b\delta$ together, if the fluid is able to penetrate into the glass,) will be many times greater than what would be forced into it by the same degree of electrification if it had been placed by itself; and the quantity of fluid driven out of $E\phi$ will be nearly equal to the redundant fluid in $B\delta$.

If a communication be now made between $B\delta$ and $E\phi$, by the canal *NRS*, the redundant fluid will run from $B\delta$ to $E\phi$; and if in its way it passes through the body of any animal, it will by the rapidity of its motion produce in it that sensation called a shock.

130] It appears from the 26th proposition, that if a body of any size was electrified in the same degree as the plate Bd, and a communication was made between that body and the ground, by a canal of the same length, breadth and thickness as *NRS*; that then the fluid in that canal would be impelled with the same force as that in *NRS*, supposing the fluid in both canals to be incompressible; and consequently, as the quantity of fluid to be moved, and the resistance to·its motion is the same in both canals, the fluid should move with the same rapidity in both: and I see no reason to think that the case will be different, if the communication is made by canals of real fluid.

Therefore what was said in the beginning of this section, namely, that as great a shock would be produced by making a communication between the conductor and the ground, as between the two sides of the Leyden vial, by canals of the same length and same kind, seems a necessary consequence of this theory; as the quantity of fluid which passes through the canal is, by the supposition, the same in both; and there is the greatest reason to think, that the rapidity with which it passes will be nearly if not quite the same in both. I hope soon to be able to say whether this agrees with experiment as well as theory.

131] It may be worth observing, that the longer the canal *NRS* is, by which the communication is made, the less will be the rapidity with which the fluid moves along it; for the longer the canal is, the greater is the resistance to the motion of the fluid in it; whereas the force with which the whole quantity of fluid in it is impelled, is the same whatever be the length of the canal. Accordingly, it is found in melting small wires, by directing a shock through them, that the longer the wire the greater charge it requires to melt it.

132] As the fluid in $B\delta$ is attracted with great force by the redundant matter in $E\phi$, it is plain that if the fluid is able to penetrate at all into the glass, great part of the redundant fluid will be lodged in $b\delta$, and in like manner there will be a great deficience of fluid in $e\phi$. But in order to form some estimate of the proportion of the redundant fluid which will be lodged in $b\delta$, let the communication between *Ef* and the ground be taken away, as well as that by which *Bd* is electrified; and let so much fluid be taken from $B\delta$, as to make the redundant fluid therein equal to the deficient fluid in $E\phi$. If we suppose that all the redundant fluid is collected in $b\delta$, and all the deficient in $e\phi$, so as to leave *Bd* and *Ef* saturated; then, if the electric repulsion is inversely as the square of the distance, a particle of fluid placed anywhere in the plane *bd*, except near the extremities *b* and *d*, will be attracted with very near as much force by the redundant matter in $e\phi$, as it is repelled by the redundant fluid in $b\delta$; but if the repulsion is inversely as some higher power than the square, it will be repelled with much more force by $b\delta$, than it is attracted by $e\phi$, provided the depth $b\delta$ is very small in respect of the thickness of the glass; and if the repulsion is inversely as some lower power than the square, it will be attracted with much more force by $e\phi$, than it is repelled by $b\delta$. Hence it follows, that if the depth to which the fluid can penetrate is very small in respect of the thickness of the glass, but yet is such that the quantity of fluid naturally contained in $b\delta$, or $e\phi$, is considerably more than the redundant fluid in $B\delta$; then, if the repulsion is inversely as the square of the distance, almost all the redundant fluid will be collected in $b\delta$, leaving the plate *Bd* not very much overcharged; and in like manner *Ef* will be not very much undercharged: if the repulsion is inversely as some higher power than the square, *Bd* will be very much overcharged, and *Ef* very much undercharged: and if the repulsion is inversely as some lower power than the square, *Bd* will be very much undercharged, and *Ef* very much overcharged.

133] Suppose, now, the plate *Bd* to be separated from the plate of glass, still keeping it parallel thereto, and opposite to the same part of it that it before was applied to; and let the repulsion of the particles be inversely as some higher power of the distance than the square. When the plate is in contact with the glass, the repulsion of the redundant fluid in that plate, on a particle in the plane *bd*, *id est*, the inner surface of the plate, must be equal to the excess of the repulsion of the redundant fluid in $b\delta$ on it, above the attraction of $E\phi$ on it; therefore, when the plate *Bd* is removed ever so small a distance from the glass, the repulsion of the redundant fluid in the plate, on a particle in the inner surface of that plate, will be greater than the excess of the repulsion of $b\delta$ on it, above the attraction of $E\phi$; for the repulsion of $b\delta$ will be much more diminished by the removal, than the attraction of $E\phi$: consequently, some fluid will fly from the plate to the glass, in the form of sparks: so that the plate will not be so much overcharged when removed from the glass, as it was when in contact with it. I should imagine, however, that it would still be considerably overcharged.

If one part of the plate is separated from the glass before the rest, as must necessarily be the case, if it consists of bending materials, I should guess it would be at least as much, if not more, overcharged, when separated, as if it is separated all at once.

In like manner, it should seem that the plate *Ef* will be considerably undercharged, when separated from the glass, but not so much so as when in contact with it.

From the same kind of reasoning I conclude, that if the repulsion is inversely as some lower power of the distance than the square, the plate *Bd* will be considerably undercharged, and *Ef* considerably overcharged, when separated from the glass, but not in so great a degree as when they are in contact with it.

134] § 7. There is an experiment of Mr Wilcke and Æpinus, related by Dr Priestly, p. 258, called by them, electrifying a plate of air: it consisted in placing two large boards of wood, covered with tin plates, parallel to each other, and at some inches asunder. If a communication was made between one of these and the ground, and the other was positively electrified, the former was undercharged; the boards strongly attracted each other; and, on making a communication between them, a shock was felt like that of the Leyden vial.

I am uncertain whether in this experiment the air contained between the two boards is very much overcharged on one side, and very much undercharged on the other, as is the case with the plate of glass in the Leyden vial; or whether the case is, that the redundant or deficient fluid is lodged only in the two boards, and that the air between them serves only to prevent the electricity from running from one board to the other: but whichever of these is the case, the experiment is equally conformable to the theory*.

It must be observed, that a particle of fluid placed between the two plates is drawn towards the undercharged plate, with a force exceeding that with which it would be repelled from the overcharged plate, if it was electrified with the same force, the other plate being taken away, nearly in the ratio of twice the quantity of redundant fluid actually contained in the plate, to that which it would contain, if electrified with the same force by itself; so that, unless the plate is very weakly electrified, or their distance is very considerable, the fluid will be apt to fly from one to the other, in the form of sparks.

135] § 8. Whenever any conducting body as A, communicating with the ground, is brought sufficiently near an overcharged body B, the electric

[* See Articles 344, 345, 511, 516.]

fluid is apt to fly through the air from B to A, in the form of a spark: the way by which this is brought about seems to be this. The fluid placed anywhere between the two bodies, is repelled from B towards A, and will consequently move slowly through the air from one to the other: now it seems as if this motion increased the elasticity of the air, and made it rarer: this will enable the fluid to flow in a swifter current, which will still further increase the elasticity of the air, till at last it is so much rarified, as to form very little opposition to the motion of the electric fluid, upon which it flies in an uninterrupted mass from one body to the other.

In the same manner may the electric fluid pass from one body to another, in the form of a spark, if the first body communicates with the ground, and the other body is negatively electrified, or in any other case in which one body is strongly disposed to part with its electricity to the air, and the other is strongly disposed to receive it.

136] In like manner, when the electric fluid is made to pass through water, in the form of a spark, as in Signor Beccaria's* and Mr Lane's† experiments, I imagine that the water, by the rapid motion of the electric fluid through it, is turned into an elastic fluid, and so much rarified as to make very little opposition to its motion: and when stones are burst or thrown out from buildings struck by lightning, in all probability that effect is caused by the moisture in the stone, or some of the stone itself, being turned into an elastic fluid.

137] It appears plainly, from the sudden rising of the water in Mr Kinnersley's electrical air thermometer‡, that when the electric fluid passes through the air, in the form of a spark, the air in its passage is either very much rarified, or intirely displaced: and the bursting of the glass vessels, in Beccaria's and Lane's experiments, shews that the same thing happens with regard to the water, when the electric fluid passes through it in the form of a spark. Now, I see no means by which the displacing of the air or water can be brought about, but by supposing its elasticity to be increased, by the motion of the electric fluid through it, unless you suppose it to be actually pushed aside, by the force with which the electric fluid endeavours to issue from the overcharged body: but I can by no means think, that the force with which the fluid endeavours to issue, in the ordinary cases in which electric sparks are produced, is sufficient to overcome the pressure of the atmosphere, much less that it is sufficient to burst the glass vessels in Beccaria's and Lane's experiments.

138] The truth of this is confirmed by Prop. XVI. For, let an undercharged body be brought near to, and opposite to the end of a long cylindrical body communicating with the ground, by that proposition the

* *Elettricismo artificiale e naturale*, p. 110. Priestly, p. 209.

† *Phil. Trans.* 1767, p. 451.

‡ *Phil. Trans.* 1763, p. 84. Priestly, p. 216.

pressure of the electric fluid against the base of the cylinder is scarcely greater than the force with which the two bodies attract each other, provided that no part of the cylinder is undercharged; which is very unlikely to be the case, if the electric repulsion is inversely as the square of the distance, as I have great reason to believe it is; and, consequently, if the spark was produced by the air being pushed aside by the force with which the fluid endeavours to issue from the cylinder, no sparks should be produced, unless the electricity was so strong, that the force with which the bodies attracted each other was as great as the pressure of the atmosphere against the base of the cylinder: whereas it is well known, that a spark may be produced, when the force, with which the bodies attract, is very trifling in respect of that*.

139] One may frequently observe, in discharging a Leyden vial, that if the two knobs are approached together very slowly, a hissing noise will be perceived before the spark; which shews, that the fluid begins to flow from one knob to the other, before it passes in the form of a spark; and therefore serves to confirm the truth of the opinion, that the spark is brought about in the gradual manner here described.

[* Note 10, p. 375.]

PRELIMINARY PROPOSITIONS

[From the MS. in the possession of the Duke of Devonshire, N°. 4; hitherto unpublished.]

{See Table of Contents at the beginning of this volume.}

In this and all the following propositions and lemmata the electric attraction and repulsion is supposed to be inversely as the square of the distance.

140] PROP. XXIX. Let a thin circular plate be connected to a globe [of the same diameter] placed at an infinite distance from it by a straight canal of incompressible fluid such as is described in Pr. XIX, perpendicular to the plane of the plate and meeting it in its center, and let them be overcharged.

If we suppose that part of the redundant fluid in the plate is spread uniformly, and that the remainder is disposed in its circumference, and that the part which is spread uniformly is to that which is disposed in the circumference as p to one, the quantity of redundant fluid in the plate will be to that in the globe as $p + 1$ to $2p + 1$.

For by Prop. XXII, Cor. V, the force with which that part of the redundant fluid in the plate which is disposed in the circumference repels the fluid in the canal is the same with which an equal quantity placed in the globe repels it in the contrary direction, and the repulsion of that part which is spread uniformly is the same as that of twice that quantity placed in the globe, and therefore the repulsion of a quantity of fluid equal to $p + 1$ disposed in the plate as expressed in the proposition is equal to that of the quantity $2p + 1$ placed in the globe.

141] PROP. XXX. Fig. 1. Let two equal thin circular plates AB and ab communicate with each other, and also with a third circular plate EF of the same size and shape and placed at an infinite distance from them, by the straight canal CD of incompressible fluid. Let the three plates be all parallel to each

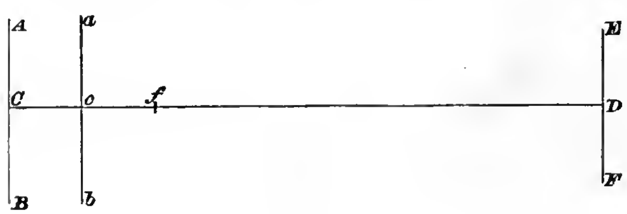


Fig. 1.

other and be placed so that CD shall pass through their centers and be perpendicular to their planes, and let the plates be overcharged. The quantity of redundant fluid in each of the plates AB and ab will be to that in EF as the repulsion of the plate ab on the canal cD to the sum of the repulsions on cD

and fD (cf being taken equal to cC), supposing that the redundant fluid in all three plates is disposed in the same manner.

For first, as the plates AB and ab are at an infinite distance from any other over or undercharged body, the repulsion of AB on the canal Cc in one direction must be equal to that of ab on it in the contrary, and therefore the redundant fluid in AB must be equal to that in ab.

Secondly, the sum of the repulsions of AB and ab on the canal cD must be equal to that of EF on it in the contrary direction, as otherwise some fluid must flow from ab to EF or from EF to ab. But as all three plates are of the same size, and the fluid in them is disposed in the same manner, the repulsions of EF and ab on cD will be to each other as the quantity of redundant fluid in them, and therefore the quantity of redundant fluid in ab will be to that in EF as the repulsion of ab on CD to the sum of the repulsions of AB and ab on it, that is, as the repulsion of ab on cD to the sum of its repulsions on fD and cD, for the repulsion of AB on cD is equal to the repulsion of ab on fD*.

142] Cor. I. If the fluid in these plates is disposed in the same manner as in Prop. XXIX the quantity of redundant fluid in each of the plates AB and ab will be to that in EF as

$$AC\,(p+\tfrac{1}{2}) \text{ to } AC\,(p+\tfrac{1}{2}) + p\,(Ac - Cc) + \frac{AC^2}{2Ac}.$$

For by Lemma X the repulsion of a given quantity of fluid spread uniformly over ab on the column cD; the repulsion of the same fluid on cf; the repulsion of the same quantity of fluid collected in the circumference of the plate ab on the column cD; and the repulsion of the same fluid on cf are to each other as

$$ac;\ ac + cf - af;\ \frac{ac}{2} \text{ and } \frac{ac}{2} - \frac{ac^2}{2af},$$

and therefore the whole repulsion of the plate ab on cD is to its repulsion on cf as

$$p \times ac + \frac{ac}{2} : p \times (ac + cf - af) + \frac{ac}{2} - \frac{ac^2}{2af},$$

and therefore the repulsion of ab on cD is to the sum of its repulsions on cD and fD as

$$ac \times (p+\tfrac{1}{2}) : ac\,(2p+1) - p\,(ac + cf - af) - \frac{ac}{2} + \frac{ac^2}{2af},$$

or as

$$ac\,(p+\tfrac{1}{2}) : ac\,(p+\tfrac{1}{2}) + p\,(af - cf) + \frac{ac^2}{2af}.$$

143] Cor. II. Therefore if all the redundant fluid in the plates is spread uniformly, the redundant fluid in each of the plates AB and ab will be to that in EF as $AC : AC + Ac - Cc$, and if it is all collected in the circumference, as

$$AC : AC + \frac{AC^2}{Ac}.$$

144] Cor. III. By Prop. XXIV it appears that the redundant fluid in the plate AB or ab will bear the same proportion to that in EF though they com-

[* Note 11, p. 377.]

municate with EF by separate canals, and whether the canals by which they communicate with it are straight or crooked, or in whatever direction EF is placed in respect of them, provided the situation of AB and ab in respect of each other remains the same. Only it must be observed that if the fluid in the plates is not disposed so as to be in equilibrio, as will most likely be the case if it is disposed as in the two preceding corollaries, it is necessary that the canals should meet them in their centers, for if the fluid in a plate is not in equilibrio, its repulsion on a canal of infinite length will not be the same in whatever part the canal meets it, as it will if the fluid in the plate is in equilibrio.

145] LEMMA XII. Fig. 2. Let BA be an infinitely slender cylindric column of uniform matter infinitely continued beyond A: the repulsion of a particle of

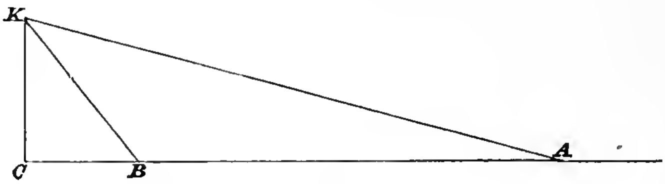


Fig. 2.

matter K on this column in the direction BA is proportional to or may be represented by $\frac{1}{KB}$, supposing the size of the particle and [the] base of the column to be given.

For draw KC perpendicular to AB continued, and let the point B flow towards C, the fluxion of the repulsion of K on the column equals

$$\frac{-\dot{CB}}{KB^2} \times \frac{CB}{KB} = \frac{-\dot{KB}}{KB^2},$$

the fluent of which, $\frac{1}{KB}$, is nothing when KB is infinite.

146] LEMMA XIII. Suppose now KC to represent an infinitely slender cylindric column of uniform matter: the repulsion of KC on the infinite column BA is to the repulsion of the same quantity of matter collected in the point C on the same column as the nat. log. of $\frac{KC + KB}{CB}$ to $\frac{KC}{CB}$.

For the repulsion of all the matter therein, when collected at C, on BA is proportional to $\frac{KC}{CB}$, and supposing the column CK to flow, the fluxion of its repulsion on BA is equal to $\frac{\dot{CK}}{KB}$, the fluent of which is the nat. log. of $\frac{KC + KB}{CB}$, and is nothing when CK is nothing.

147] LEMMA XIV. The repulsion of CK on a particle at B, in the direction CB, is proportional to $\frac{CK}{KB \times CB}$, supposing the base of CK and the size of the particle B to be given.

For supposing CK to flow, the fluxion of its repulsion on B in the direction CB is proportional to $\frac{\dot{CK}}{KB^2} \times \frac{CB}{KB}$, the fluent of which is $\frac{CK}{KB \times CB}$, and is nothing when CK is nothing.

148] LEMMA XV. Fig. 3. Let $GEFHMN$ be a cylinder whose bases are GEF and HMN and whose axis is CK. Let the convex surface of this cylinder be uniformly coated with matter, and let GC be small in respect of CK. Let

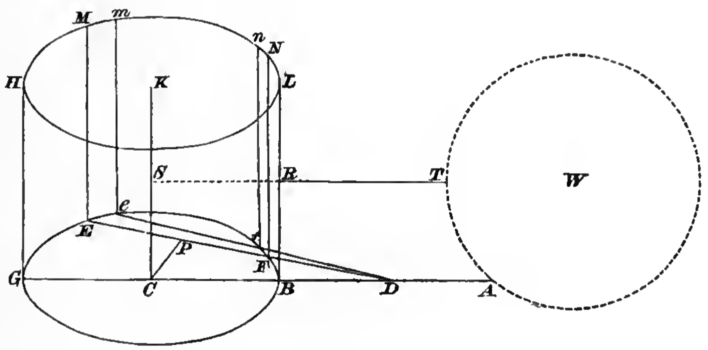


Fig. 3.

GA be a diameter of the base produced, and D any point therein. The repulsion of the convex surface of the cylinder on the point D in the direction CD is very nearly the same as if all the matter therein was collected in the axis CK and spread uniformly therein.

For let MED and meD be two planes infinitely near to each other, parallel to CK and passing through D, and cutting the convex surface in ME and NF and in me and nf, which will consequently be right lines equal to each other and perpendicular to ED; and draw CP perpendicular to ED.

The repulsion of $NnfF$ on D in the direction CD is proportional to $\frac{Ff \times FN}{FD \times ND} \times \frac{PD}{CD}$, and that of $MmeE$ is proportional to $\frac{Ee \times EM}{ED \times MD} \times \frac{PD}{CD}$.

But Ff is to Ee as FD to ED, therefore $\frac{Ff}{FD}$ and $\frac{Ee}{ED}$ are each equal to $\frac{Ff + Ee}{FD + ED} = \frac{Ff + Ee}{2PD}$, therefore the sum of the repulsions of $MmeE$ and $NnfF$ is proportional to

$$\frac{(Ff + Ee)\, CK \times PD}{2PD \times CD} \times \left(\frac{1}{ND} + \frac{1}{MD}\right) = \frac{(Ff + Ee)\, CK}{2CD} \times \left(\frac{1}{ND} + \frac{1}{MD}\right).$$

But the repulsion of the same quantity of matter collected in CK is proportional to $\frac{(Ff + Ee) \times CK}{2CD} \times \frac{2}{KD}$, and, as CG is small in respect of CK, $\frac{1}{ND} + \frac{1}{MD}$

differs very little from $\frac{2}{KD}$*, therefore the sum of the repulsions of $MmeE$ and $NnfF$ is very nearly the same as if all the matter in them was collected in CK, and consequently the repulsion of the whole convex surface of the cylinder will be very nearly the same as if all the matter in it was collected in CK.

149] COR. Therefore if BA represents an infinitely thin cylindric column of uniform matter infinitely extended beyond A, the repulsion of the convex surface of the cylinder thereon in the direction BA is very nearly the same as if all the matter therein was collected in CK, and therefore is to the repulsion of the same quantity of matter collected in the point C thereon very nearly as nat. log. $\frac{CK+KB}{CB}$ to $\frac{CK}{CB}$, that is very nearly as nat. log. $\frac{2CK}{CB}$ to $\frac{CK}{CB}$. In like manner the repulsion on the infinite column DA is to the repulsion of the same quantity of matter collected in C very nearly as nat. log. $\frac{CK+KD}{CD}$ to $\frac{CK}{CD}$.

150] PROP. XXXI. Fig. 3. Let the cylinder $GEFHMN$ be connected to the globe W, whose diameter is equal to GB and whose distance from it is infinite, by a canal TR of incompressible fluid of any shape, and meeting the cylinder in any part, and let them be overcharged: the quantity of redundant fluid in the cylinder will be to that in the globe in a less ratio than that of CK to nat. log. $\frac{2CK}{CB}$, and in a greater ratio than that of $\frac{CK}{2CB}$ to nat. log. $\frac{CK}{CB}$, provided CB is small in respect of CK.

By Prop. XXIV the quantity of redundant fluid in the cylinder will bear

* As neither MD nor ND differ from KD by so much as CB, it is plain that $\frac{1}{MD}+\frac{1}{ND}$ cannot differ from $\frac{2}{KD}$ in so great a proportion as that of BC to KD, but in reality it does not differ from it in so great a ratio as that of CB^2 to KD^2, but as it is not material being so exact, I shall omit the demonstration. See A. I.

[From MS. "A. I"] Demonstration of note at bottom of page 8,

$$CB = r,\ CP = b,\ PF = d,\ PD = a,\ CR^2 + CD^2 = e^2,$$

$$b^2 - d^2 = f^2,\qquad e^2 - f^2 = g^2,$$

$$\frac{2}{g} = \frac{2}{e} + \frac{f^2}{e^3},$$

$$ND^2 = CR^2 + a^2 - 2ad + d^2 = e^2 - b^2 - 2ad + d^2 = e^2 - f^2 - 2ad$$
$$= g^2 - 2ad,$$

$$MD^2 = g^2 + 2ad,$$

$$\frac{1}{ND} + \frac{1}{MD} = \left\{\begin{matrix} \frac{1}{g} + \frac{ad}{g^3} + 3\frac{a^2d^2}{2g^5} \\ \frac{1}{g} - \frac{ad}{g^3} + 3\frac{a^2d^2}{2g^5} \end{matrix}\right\} = \frac{2}{g} + \frac{3a^2d^2}{g^5}$$

$$= \frac{2}{e} + \frac{f^2}{e^3} + 3\frac{a^2d^2}{g^5} = \frac{2}{e} + \frac{b^2}{e^3} + 3\frac{a^2d^2}{g^5} - \frac{d^2}{c^3},$$

which is less than

$$\frac{2}{e} + \frac{b^2}{e^3} + 2\frac{d^2}{e^3} = \frac{2}{e} + \frac{r^2+d^2}{e^3}.$$

the same proportion to that in the globe in whatever part the canal meets the cylinder, therefore first I say the redundant fluid in the cylinder will bear a greater proportion to that in the globe than that of $\frac{CK}{2CB}$ to nat. log. $\frac{CK}{CB}$.

For let the canal TR be straight and perpendicular to BL, and let it meet the cylinder in R, the middle point of the line BL, and let it, if produced, meet the axis in S, which will consequently be the middle point of CK; then, if the redundant fluid in the cylinder was spread uniformly on its convex surface, the quantity of redundant fluid therein would be to that in the globe very nearly as $\frac{CK}{2CB}$ to nat. log. $\frac{CK}{CB}$.

For in that case the repulsion of the cylinder on the canal RT would be to the repulsion of the same quantity of redundant fluid collected in C very nearly as nat. log. $\frac{2SK}{SR}$ to $\frac{SK}{SR}$ or as nat. log. $\frac{CK}{CB}$ to $\frac{CK}{2CB}$, and the force with which the globe repels the canal in the direction TR is the same with which a quantity of redundant fluid equal to that in the globe placed at S would repel it in the contrary direction.

But there can be no doubt but that almost all the redundant fluid in the cylinder will be collected on its surface, and also will be collected in greater quantity near the ends than near the middle, consequently the repulsion of the cylinder on RT will be less than if the redundant fluid was spread uniformly on its convex surface, and therefore the quantity of redundant fluid in it will bear a greater proportion to that in the globe than it would on that supposition.

Secondly, the quantity of fluid in the cylinder will bear a less proportion to that in the globe than that of $\frac{CK}{CB}$ to nat. log. $\frac{2CK}{CB}$.

For suppose the canal to meet the cylinder in B and to coincide with BA. Then, if the redundant fluid was spread uniformly on the convex surface, the quantity therein would be to that in the globe very nearly as $\frac{CK}{CB}$ to nat. log. $\frac{2CK}{CB}$, and the real quantity of redundant fluid in it will bear a less proportion to that in the globe than if it was spread uniformly on the convex surface.

151] Cor. Therefore the quantity of redundant fluid in the cylinder is to that in a globe whose diameter equals CK in a ratio between that of 2 to nat. log. $\frac{2CK}{CB}$ and that of 1 to nat. log. $\frac{CK}{CB}$*.

152] Prop. XXXII. Fig. 4. Let $ADFB$ and $adfb$ be two equal cylinders whose axes are EC and ec, let them be parallel to each other and placed so that Cc, the line joining the ends of the axes, shall be perpendicular to the axes, and let the lines EC and fb be bisected in G and g, and let them be connected by canals of incompressible fluid of any shape to a third cylinder of the same size and shape placed at an infinite distance from them, and let them be over-

[* Note 12, p. 382.]

charged: the quantity of redundant fluid in each of them will be to that in the third cylinder in a ratio between that of $\log\frac{EC}{CB}$ to $\log\frac{EC}{CB}+\log\frac{EG+eg}{Cb}$ and that of $\log\frac{2EC}{CB}$ to $\log\frac{2EC}{CB}+\log\frac{EC+Eb}{Cb}$, provided the redundant fluid in the third cylinder is disposed in the same manner as in the other two.

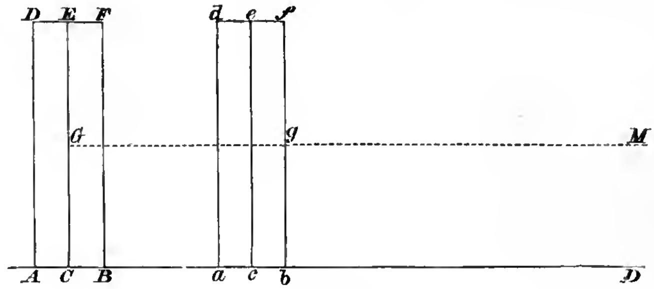


Fig. 4.

For let us suppose that *ADFB* and *adfb* are connected to the third cylinder by the canal *GM*, then, if the redundant fluid in each cylinder is disposed uniformly on its convex surface, the sum of the repulsions of *ADFB* and *adfb* on the canal *gM* will be to the repulsion of the third cylinder thereon (supposing the quantity of redundant fluid in it to be equal to that in each of the two others) as $\log\frac{2EG}{CB}+\log\frac{EG+Eg}{Gg}$ to $\log\frac{2EG}{CB}$.

Let us now suppose the fluid in the first two cylinders to be disposed so as to be in equilibrio, and consequently to be disposed in greater quantity near their extremities than near their middles, and let the fluid in the third cylinder be disposed in the same manner, and be the same in quantity as before. The repulsion of *ADFB* on *Gg* will be diminished in a greater ratio, and consequently its repulsion on *gM* will be diminished in a less ratio than that of *adfb* on *gM*, consequently the sum of the repulsions of *ADFB* and *adfb* on *gM* will be diminished in a less ratio than that of the third cylinder thereon, and therefore the sum of the repulsions of *ADFB* and *adfb* on *gM* will be to that of the third cylinder thereon in a greater ratio than that of

$$\log\frac{2EG}{CB}+\log\frac{EG+Eg}{Gg} \text{ to } \log\frac{2EG}{CB}.$$

Therefore the real quantity of redundant fluid in each of the first two cylinders will be to that in the third cylinder in a less ratio than that of $\log\frac{2EG}{CB}$ to $\log\frac{2EG}{CB}+\log\frac{EG+Eg}{Gg}$.

In like manner, by supposing them to be connected to the third cylinder by the canal *bD*, it may be shewn that the quantity of redundant fluid in either of the first two cylinders is to that in the third in a greater ratio than that of $\log\frac{2EC}{CB}$ to $\log\frac{2EC}{CB}+\log\frac{EC+Eb}{Cb}$*.

[* Note 13, p. 388.]

153] PROP. XXXIII. If two bodies B and b are successively connected by canals of incompressible fluid to a third body C placed at an infinite distance from them, and are overcharged, that is, if one of them, as B, is first connected to C and afterwards B is removed and b put in its room, the quantity of redundant fluid in C being the same in both cases, it is plain that the quantity of redundant fluid in B will bear the same proportion to that in b that it would if B and b were placed at an infinite distance from each other, and connected by canals of incompressible fluid.

154] LEMMA XV. Fig. 5. Let AB be a thin flat plate of any shape whatsoever, of uniform thickness and composed of uniform matter. Let CG be an infinitely slender cylindric column of uniform matter perpendicular to the plane of AB and meeting it in C and extended infinitely beyond G. Let ab be a thin circular plate perpendicular to cG whose center is c. Let the area of ab be equal to that of AB, and let the quantity of matter in it be the same, and let it be disposed uniformly.

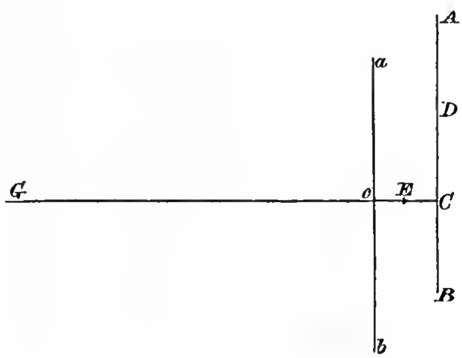


Fig. 5.

Let B be that point of the circumference of AB which is nearest to C. If EC is small in respect of CB, the repulsion of the plate AB on the short column EC is to the repulsion of ab on the infinite column cG nearly as EC to cb.

For let BD be a circle drawn through B with center C, as EC is very small in respect of CB, the repulsion of the circle BD on EC is to its repulsion on CG very nearly as EC to CB, and therefore is to the repulsion of ab on cG very nearly as EC to cb. But the repulsion of AB on EC is very little greater than that of DB, for the repulsion of DB is very near as great as it would be if its size was infinite.

155] LEMMA XVI. {Fig. 6.} Let ACB and DEF be two thin plates, not flat but concave on one side, let their distance be everywhere the same, and let it be very small in respect of the radius of curvature of all parts of their surface. Let C be any point of the surface of AB, and let CE be perpendicular to the surface in that point. Let Tt be a flat plate perpendicular to CE.

Let R be any point in AB and S the corresponding point in DF, and let T

be the corresponding point in Tt*: the sum of the repulsions of R on the column CE in the direction CE and of S on the same column in the opposite direction EC is very nearly equal to the force with which they would repel the same column in the direction CE if they were both transferred to T, provided CR^2

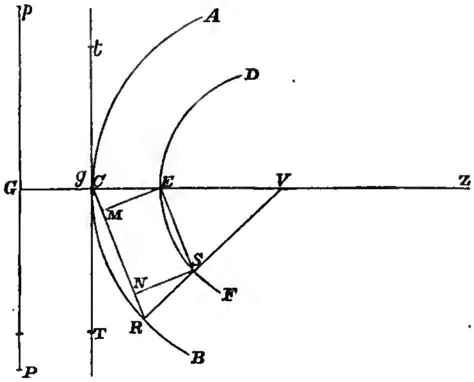


Fig. 6.

is very small in respect of the square of the least radius of curvature of the surface of AB.

Let RS be continued till it meets CE continued in V, draw EM and SN perpendicular to CR.

Let $CM = C$, $RE - RM = E$, $SC - NC = S$, and $SE - NM = D$.

As CE is very small in respect of the least radius of curvature of AB, and CV is not less than the least radius of curvature, CM and NR are each very small in respect of CR, and therefore CN, MR, and ES differ from CR in a very small ratio. Moreover as CR^2 is very small in respect of CV^2, CM^2 and RN^2 are very small in respect of CE^2, and therefore ME and NS differ in a very small ratio from CE; and, moreover, $2 \times (TE - TC)$ is greater than $\dfrac{CE^2}{TE}$.

Now the repulsion of the point R on the column CE in the direction CE is $\dfrac{1}{RC} - \dfrac{1}{RE}$† $= \dfrac{RE - RC}{RC \times RE}$, and the repulsion of the point S on the same column in the opposite direction is $\dfrac{SC - SE}{SC \times SE}$, and the sum of the repulsions of R and S is

$$\frac{RE - RC}{RC \times RE} + \frac{SC - SE}{SC \times SE} = \frac{E - C}{RC \times RE} + \frac{S + C - D}{SC \times SE}$$

$$= \frac{E}{RC \times RE} + \frac{S}{SC \times SE} + \frac{D}{SC \times SE} - \frac{C}{RC \times RE} + \frac{C}{SC \times SE},$$

* If RS is drawn perpendicular to the surface of AB at the point R cutting DF in S, I call S the corresponding point of the plate DF, and if CT is taken in the intersection of the plane RCE with that of the plate Tt equal to the right line CR, I call T the corresponding point of Tt.

† Lemma XII [Art. 145].

and the repulsion of the two particles when transferred to T on the column CE, or the repulsion of T, as I shall call it for shortness, is $2\,\dfrac{TE - TC}{TE \times TC}$.

But as ME differs in a very small ratio from CE, and RM differs in a very small ratio from RC, $RE - RM$ or E differs in a very small ratio from $TE - TC$. In like manner $SC - NC$ or S differs in a very small ratio from $TE - TC$, and ER and CS both differ in a very small ratio from TE, and SE differs in a small ratio from TC.

Therefore $\dfrac{E}{RC \times RE} + \dfrac{S}{SC + SE}$ differs very little from $2 \times \dfrac{TE - TC}{TE \times TC}$, that is, from the repulsion of T.

Moreover, as EM and SN differ very little from each other, D is very small in respect of $TE - TC$, and $\dfrac{D}{SC \times SE}$ is very small in respect of the repulsion of T.

Moreover, $\dfrac{RC - SE}{RC}$ is less than $\dfrac{CM + RN}{RC}$ or than $\dfrac{CE}{CV}$, and $\dfrac{RE - SC}{RE}$ is hardly greater than $\dfrac{RM - CN}{RE}$, and is therefore still less than $\dfrac{RC - SE}{RC}$; therefore $\dfrac{RC}{SE}$ and $\dfrac{RE}{SC}$ each differ from one in a less ratio than that of CE to CV, and therefore $\dfrac{RC \times RE}{SE \times SC}$ differs from one in a less ratio than that of $2CE$ to CV.

Consequently, $-\dfrac{C}{RC \times RE} + \dfrac{C}{SE \times SC}$ or $\dfrac{-C}{RC \times RE} \times \left(1 - \dfrac{RC \times RE}{SE \times SC}\right)$ is less than $\dfrac{-C}{RC \times RE} \times \dfrac{2CE}{CV}$, which is less than

$$\frac{CE \times RC}{CV \times RC \times RE} \times \frac{2CE}{CV} = \frac{2CE^2}{CV^2 \times RE},$$

which is very small in respect of $\dfrac{2CE^2}{TE \times TC \times RE}$, that is, of the repulsion of T.

Therefore the sum of the repulsions of R and S differs very little from the repulsion of T.

N.B. Though the distance CR is ever so great, it may be shewn that the sum of the repulsions of R and S cannot be more than double that of T*.

156] Cor. I. Let the edges of the plates ACB and DEF correspond, that is, let them be such that if a line is erected on any part of the circumference of one plate perpendicular to the [tangent] plane of the plate in that part, that line shall meet the other plate in its circumference. Let the two plates be of an uniform thickness, and let the thickness of DF bear such a proportion to that of AB that the quantity of matter shall be the same in both. Consequently the quantity of matter in each part of DF will be very nearly equal to that in the corresponding part of AB. Also let the size of the plates be such that CE

[* Note 14, p. 389.]

shall be very small in respect of the distance of C from the nearest part of the circumference of AB, and let the least radius of curvature of the surface of AB be so great in respect of CE that a point R may be taken such that CR shall be small in respect of that radius of curvature, and yet very great in respect of CE.

Let Pp be a flat circular plate whose center is G and whose plane is perpendicular to GZ, and let its area be equal to that of AB, and let the quantity of matter in it be also equal to that in AB, and let it be disposed uniformly: the sum of the repulsions of AB and DF on CE in the opposite directions CE and EC will be to the repulsion of Pp on the infinite column GZ very nearly as $2CE$ to GP.

For suppose each particle of matter in all that part of AB whose distance from C is not greater than CR and in the corresponding part of DF to be transferred to its corresponding point in Tt, so as to form a circular plate whose radius is CR.

If we suppose that the thickness of the plates Tt and Pp are both equal to that of AB, the matter in all parts of Tt will be very nearly twice as dense as that in AB or as that in Pp. Therefore the repulsion of Tt on CE will be very nearly twice the repulsion of Pp on Gg, supposing Gg to be equal to CE.

But from the foregoing lemma it appears that the sum of the repulsions which the above-mentioned part of AB and DF exerted on CE before the matter was transferred is very nearly equal to that which Tt exerts thereon after the matter is transferred, and the sum of the repulsions of the remaining part of AB and DF, or that whose distance from C is greater than CR, is very small in respect of that part whose distance is less, therefore the sum of the repulsions of the whole plates AB and DF on CE is to the repulsion of Pp on GZ very nearly as $2CE$ to GP.

It may perhaps be supposed from this demonstration that it would be necessary that CE should be excessively small in respect of CV, in order that the sum of the repulsions of the plates on CE should be very nearly equal to the repulsion of Pp on Gg, but in reality this seems not to be the case, for if the plates are segments of concentric spheres whose center is V, the sum of their repulsions will exceed twice the repulsion of Pp on Gg in a not much greater ratio than that of $1 + \frac{CE}{CV}$ to 1, and if the radius of curvature of their surfaces is in some places greater than CV, and nowhere less, I should think that the sum of their repulsion could hardly exceed twice the repulsion of Pp in so great a ratio as that.

157] Cor. II. If we now suppose that the matter of the plate AB is denser near the circumference than near the point C, and that the density at and near C is to the mean density (or the density which it would everywhere be of if the matter was spread uniformly) as δ to one, and that the quantity of matter in each part of DF is equal to that in the corresponding part of AB as before, the sum of the repulsions of the plates on CE will be less than if the matter was spread uniformly in a ratio approaching much nearer to that of δ to one than to that of equality.

For if any particle of matter is removed from that part of AB which is near C to that point which is at a distance from it, and an equal alteration is made in the plate DF, the sum of the repulsions of these particles will be much less after their removal than before.

158] LEMMA XVII. Fig. 7. Let ACB be a thin plate, not flat but concave on one side, let the radius of curvature of its surface be nowhere less than CV, and let MV be perpendicular to its surface at C; let MC be very small in respect of CV, and let Tt be a plane perpendicular to MC: the difference of the repulsion

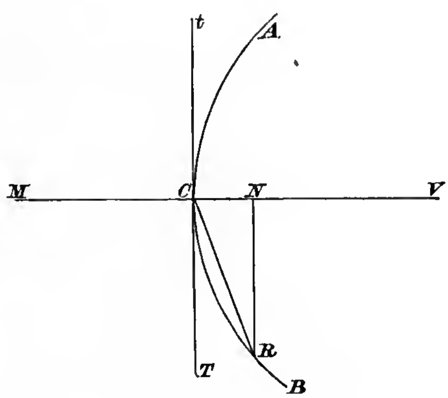


Fig. 7.

of any particle of matter as R in the plate ACB on the point M in the direction CM, and of its repulsion on the point C in the same direction, is very nearly the same as if the particle was transferred to T (CT being equal to the right line CR), provided CR is small in respect of CV.

Draw RN perpendicular to MC, the difference of the repulsions of R on the points M and $C = \frac{MN}{MR^3} - \frac{CN}{CR^3} = \frac{MC}{MR^3} + \frac{CN}{MR^3} - \frac{CN}{CR^3}$, and the difference of the repulsions of the same particle placed at T on the same points $= \frac{MC}{MT^3}$, but

$$\begin{aligned} MR^2 &= (MC + CN)^2 + RN^2 \\ &= MC^2 + CR^2 + 2MC \times CN \\ &= MT^2 + 2MC \times CN, \end{aligned}$$

and CN is not greater than $\frac{CR^2}{2CV}$, and therefore $2MC \times CN$ is not greater than $\frac{MC \times CR^2}{CV}$, and therefore is very small in respect of CR^2 or MT^2.

Therefore MR^2 differs very little from MT^2, and $\frac{1}{MR^3}$ from $\frac{1}{MT^3}$.

This being premised there are two cases to be considered.

First, if CR is considerably greater than MC, as

$$CR^2 = MR^2 - MC^2 - 2MC \times CN = MR^2 \times \left\{1 - \frac{MC\,(MC + 2CN)}{MR^2}\right\},$$

$$\frac{1}{CR^3} \text{ differs not much from } \frac{1}{MR^3} \times \left\{1 - \frac{3}{2}\frac{MC\,(MC + 2CN)}{MR^2}\right\},$$

$$\text{and } \frac{CN}{MR^3} - \frac{CN}{CR^3} \text{ differs not much from } \frac{CN}{MR^3} \times \frac{-3MC\,(MC + 2CN)}{2MR^2},$$

or from $\dfrac{MC}{MR^3} \times \dfrac{-3CN\,(MC + 2CN)}{2MR^2}$, which is very small in respect of $\dfrac{MC}{MT^3}$, provided CR is small in respect of CV.

For as CN is less than $\dfrac{CR^2}{2CV}$,

$$\frac{3CN\,(MC + 2CN)}{2MR^2} \text{ is less than } \frac{3CR^2 \times MC}{4MR^2 \times CV} + \frac{3CR^4}{4MR^2 \times CV^2},$$

or than

$$\frac{3MC}{CV} + \frac{3CR^2}{4CV^2}.$$

Therefore as $\dfrac{CN}{MR^3} - \dfrac{CN}{CR^3}$ is very small in respect of $\dfrac{MC}{MT^3}$, and as $\dfrac{MC}{MR^3}$ differs very little from $\dfrac{MC}{MT^3}$, $\dfrac{MC}{MR^3} + \dfrac{CN}{MR^3} - \dfrac{CN}{CR^3}$, or the difference of the repulsions of R on the points M and C differs very little from $\dfrac{MC}{MT^3}$, the difference of the repulsions of T on the same points.

Secondly, if CR is not considerably greater than MC, CN must be very small in respect of CR, and consequently must be very small in respect of MC. Therefore $\dfrac{CN}{CR^3} - \dfrac{CN}{MR^3}$ is very small in respect of $\dfrac{CN}{MR^3}$, and therefore the difference of the repulsions of R on C and M differs very little from $\dfrac{MC}{MT^3}$.

159] Cor. Therefore by the same method of reasoning as was used in Cor. to Lemma XVI, the difference of the repulsions of the whole plate ACB on the points M and C is very nearly the same as if each particle of matter in it was transferred to the plane Tt and placed at the same distance from C as before, and therefore its repulsion on M is very nearly equal to its repulsion on C, provided MC is very small in respect of the least distance of the circumference of the plate from C, and that the thickness of the plate is everywhere very nearly the same, except at such a distance from C as is very great in respect of MC.

160] Prop. XXXIV*. Fig. 8. Let $NnvV$ be a plate of glass or any other substance which does not conduct electricity, of uniform thickness, either flat, or concave on one side and convex on the other, and let the electric fluid be unable to penetrate at all into the glass or to move within it.

Let ACB and DEF be thin coatings of metal, or any substance which conducts electricity, applied to the glass.

* This proposition is nearly the same as Prop. XXII, only made more general.

Let these coatings be of any shape whatsoever, and let their edges correspond as in Lemma XVI, Cor. I.

Let AB communicate with the body H, and DF with the body L, by the straight canals CG and EM of incompressible fluid.

Let the points C and E be so placed that the two canals shall form one right line perpendicular to AB at the point C, and let the lengths of these canals be so

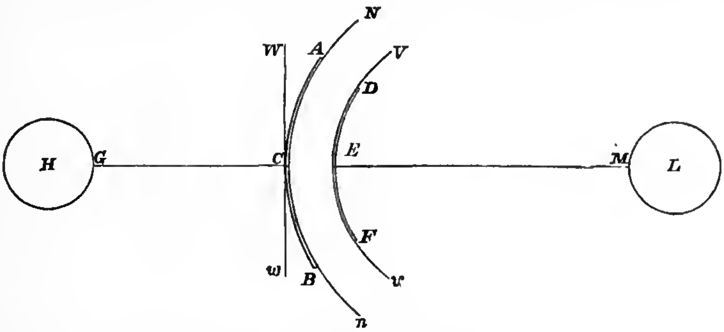


Fig. 8.

great that the repulsion of the coatings on the fluid in them shall be not sensibly less than if they were infinite, and let H be overcharged and let L be saturated.

It is plain from Prop. XII that DF will be undercharged, and that AB will be more overcharged than it would otherwise be.

Let Ww be a thin flat circular plate whose center is C, perpendicular to CE, and whose area is equal to that of AB, let the force with which the redundant fluid in AB would repel the short column CE (if ME was continued to C) be called m, and let the force with which it would repel CM, or with which it repels CG (for they are both alike), be called M. Let the force with which the same quantity of redundant fluid disposed in DF, in the same manner in which the deficient fluid therein is actually disposed, would repel $\begin{Bmatrix} CE \\ EG \end{Bmatrix}$ be called $\dfrac{g}{G}$, let the force with which the same quantity of redundant fluid uniformly disposed on Ww would repel CG be called W, and let the force with which H repels CG be the same with which a quantity of fluid, which we will call B, uniformly distributed on Ww would repel it in the contrary direction: then will the quantity of redundant fluid in AB be $B \times \dfrac{GW}{Mg + Gm - mg}$, which, if M and G are very nearly alike, and m and g are very small in respect of G, differs very little from $\dfrac{BW}{g + m}$, and the deficient fluid in DF will be to the redundant fluid in AB as $M - m$ to G, and therefore on the same supposition will be very nearly equal to it.

For the force with which AB repels the fluid in EM must be equal to that with which DF attracts it, for otherwise some fluid would run out of DF into L, or out of L into DF. For the same reason the excess of the repulsion of AB on CG above the attraction of DF thereon must be equal to the force with which a quantity of redundant fluid equal to B spread uniformly on Ww would repel it.

By the supposition the force with which AB repels the canal EM is $M - m$, and the force with which the same quantity of redundant fluid, spread on DF in the same manner in which the deficient fluid therein is actually disposed, repels it is G, therefore if the redundant fluid in AB is called A, the deficient fluid in DF will be $A \times \frac{M-m}{G}$; therefore the force with which DF attracts CG is $(G-g)\frac{M-m}{G}$, and the excess of the force with which AB repels CG above that with which DF attracts it is

$$M - \frac{(G-g)(M-m)}{G} = \frac{Mg + Gm - mg}{G},$$

which must be equal to the force with which a quantity of fluid equal to B spread uniformly over Ww would repel it, that is, it must be equal to $W\frac{B}{A}$; therefore A equals $\frac{BGW}{Mg + Gm - mg}$.

161] COR. I. If the plate of glass is flat, and its thickness is very small in respect of the least distance of the point C from the circumference of AB, and the fluid in AB and DF is spread uniformly, the quantity of redundant fluid in DF will differ very little from $\frac{B \times CW}{2CE}$, and the deficient fluid in DF will be very nearly equal to the redundant fluid in AB.

For as the plate of glass is flat, the two coatings will be equal to each other, and therefore M and G are equal to each other, and so are m and g, and $\frac{g}{W}$ differs very little from $\frac{CE}{CW}$*, and moreover g is very small in respect of G.

162] COR. II. If the plate is flat and the two coatings are circular, their centers being in C and E, the quantity of redundant fluid in AB will be more accurately equal to $\frac{B \times CW}{2CE} \times \frac{CW}{CW - CE}$, CW being in this case equal to the semi-diameter of the coatings, and the deficient fluid in DF will be to the redundant in AB nearly as $CW - CE$ to CW.

For in this case $\frac{m}{W}$ is accurately equal to

$$\frac{CE + CW - \sqrt{CE^2 + CW^2}}{CW},$$

and therefore

$$\frac{2m}{W} - \frac{m^2}{W^2} = \frac{2CE\sqrt{CW^2 + CE^2} - 2CE^2}{CW^2},$$

which, if CE is small in respect of CW, differs very little from

$$\frac{2CE\,(CW - CE)}{CW^2}.$$

* Lemma XV [Art. 148.]

163] Cor. III. If the plate of glass is not flat, and its thickness is very small in respect of the radius of curvature of its surface at and near C, everything else being as in Cor. I, the quantity of redundant fluid in AB will still be very nearly equal to $\frac{B \times CW}{2CE}$.

For as CE is very small in respect of the radius of curvature, the two coatings will be very nearly of the same size, and therefore G differs very little from M, and $m + g$ is to W very nearly as CE to CW*, and moreover m and g are both very small in respect of M and G†.

164] Cor. IV. If we now suppose that the density of the redundant fluid in AB is greater at its circumference than it is near the point C, and that its density at and near C is less than the mean density, or the density which it would everywhere be of if it was spread uniformly, in the ratio of δ to one, and that the deficient fluid in DF is spread nearly in the same manner as the redundant in AB, the quantity of redundant fluid in AB will be greater than before in a ratio approaching much nearer that of one to δ than to that of equality, and that whether the glass is flat or otherwise.

For by Lemma [XVI, Cor. II], m and g will each be less than before in the above-mentioned ratio.

165] Cor. V. Whether the plate of glass is flat or concave, or whatever shape the coatings are of, or whatever shape the canals CG and EM are of, or in whatever part they meet the coatings, provided the thickness of the plate is very small in respect of the smallest diameter of the coatings, and is also sufficiently small in respect of the radius of curvature of its surface in case it is concave, the quantity of redundant fluid in AB will differ very little from $\frac{B \times CW}{2CE}$.

For suppose that the canal GC meets the coating AB in the middle of its shortest diameter, and that the point in which ME meets DF is opposite to L, as in Prop. [XXII, Art. 74], the thickness of the glass will then be very small in respect of the distance of the point C from the nearest part of the circumference of AB, and moreover, by just the same reasoning as was used in the Remarks to Prop. XXII, it may be shewn that δ will in all probability differ very little from one, and consequently by Cors. I and III the redundant fluid in AB will be as above assigned. But by Prop. XXIV the quantity of fluid in the coatings will be just the same in whatever part the canals meet them, or whatever shape the canals are of.

166] Cor. VI. On the same supposition, if the body H is a globe whose diameter equals Ww, *id est* the diameter of a circle whose area equals that of

* Lemma XVI, Cor.

† As the demonstration of the sixteenth Lemma and its corollary is rather intricate, I chose to consider the case of the flat plate of glass separately in Cor. I, and to demonstrate it by means of Lemma XV.

the coating AB, the redundant fluid in AB will be to that in H very nearly as CW to $4CE$.

For the quantity of redundant fluid in H will be $2B$.

167] Cor. VII. On the same supposition the redundant fluid in AB will be very nearly the same whether the glass is flat or otherwise, or whatever shape the coatings are of.

168] Cor. VIII. On the same supposition, if the size and shape of the coatings and also the thickness of the glass is varied, the size and quantity of redundant fluid in H remaining the same, the quantity of redundant fluid in AB will be very nearly directly as its surface, and inversely as the thickness of the glass.

169] Prop. XXXV (Fig. 9). Let Pp, Rr, Ss, Tt represent any number of surfaces whose distance from Nn, and consequently from each other, is the same in all parts, and let everything be as in the preceding proposition, except that the fluid in the spaces $PprR$, $SstT$, &c., that is, in the spaces comprehended between the surfaces Pp and Rr, and between Ss and Tt, &c. is moveable*, in such manner, however, that though the fluid in any of these spaces as $PprR$ is able to move freely from Pp to Rr or from Rr to Pp, in a direction perpendicular to the surface Pp or Rr, yet it is not able to move sideways, or in a

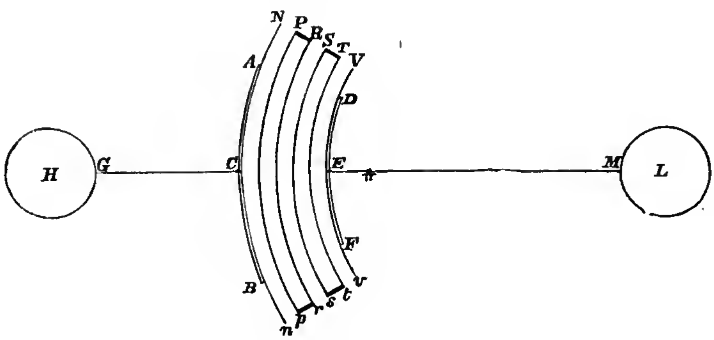


Fig. 9.

direction parallel to those surfaces†, and let the fluid in the remaining spaces $NnpP$, $RrsS$, $TtvV$, &c. be immoveable: the quantity of redundant fluid in AB and the deficient fluid in DF will be very nearly the same that they would be if the whole fluid within the glass was immoveable, and its thickness was only equal to $NP + RS + TV$, &c., that is, to the sum of the thicknesses of those spaces in which the fluid is immoveable, provided that NV, the thickness of the glass, is very small in respect of the smallest diameter of AB, and also in respect of the radius of curvature of the surface of the glass.

* To avoid confusion I have drawn in the figure only two spaces in which the fluid is supposed to be moveable, but the case would be just the same if there were ever so many.

† [Note 15.]

Let the canals GC and EM be perpendicular to the plate of glass and opposite to each other, so as to form one right line, and let them meet AB and DF in the middle of their shortest diameters. The coating AB will be very much overcharged, and DF almost as much undercharged, in consequence of which some fluid will be driven from the surface Pp to Rr and from Ss to Tt. Moreover the quantity of fluid driven from any portion of the surface Pp near the line CE will be very nearly equal to the quantity of redundant fluid lodged in the corresponding part of AB, or more properly will be very nearly equal to a mean between that and the quantity of deficient fluid in the corresponding part of DF.

For a particle of fluid placed anywhere in the space $PprR$ near the line CE is impelled from Pp to Rr by the repulsion of AB and the attraction of DF, and it is not sensibly impelled either way by the spaces $SstT$, &c., as the attraction of the redundant matter in Ss is very nearly equal to the repulsion of the redundant fluid in Tt; and moreover the repulsion of AB on the particle and the attraction of DT are very nearly as great as if their distance from it was no greater than that of Pp and Rr, and therefore the particle could not be in equilibrio unless the quantity of fluid driven from Pp to Rr was such as we have assigned.

As to the quantity of fluid driven from Pp to Rr at a great distance from CE, it is hardly worth considering. It is plain, too, that the quantity of fluid driven from Ss to Tt will be very nearly the same as that driven from Pp to Rr.

Let now G, g, M, m and W signify the same things as in the preceding proposition, and let the quantity of redundant fluid in AB be called A as before, and let $NP + RS + TV +$ &c., *id est*, the sum of the thicknesses of those spaces in which the fluid is immoveable, be to NV, or the whole thickness of the glass, as S to 1, and let $PR + ST +$ &c., or the sum of the thicknesses of those spaces in which the fluid is moveable, be to NV as D to one.

Take $E\Pi$ equal to PR, the repulsion of the space $PprR$ on the infinite column EM is equal to the repulsion of the redundant fluid in Rr on $E\Pi$, and therefore is to the repulsion of AB on CE very nearly as $E\Pi$ or PR to CE. Therefore the repulsion of all the spaces $PprR$, $SstT$, &c. on EM is to the repulsion of AB on CE very nearly as D to one, or is equal to mD, and therefore the sum of the repulsions of AB and those spaces together on EM is very nearly equal to $M - m + mD$ or to $M - mS$.

But the attraction of DF on EM must be equal to the above-mentioned sum of the repulsions, and therefore the deficient fluid in DF must be very nearly equal to $\dfrac{A\,(M - mS)}{G}$.

By the same way of reasoning it appears that the force with which CG is repelled by AB, DF, and the spaces $PprR$ and $SstT$, &c. together is very nearly equal to

$$M - \frac{(M - mS)\,(G - g)}{G} - gD, \text{ or to } \frac{Mg}{G} + mS - \frac{mgS}{G} - gD,$$

which, as M differs very little from G, and $\dfrac{mgS}{G}$ is very small in respect of

mS or gS, is very nearly equal to $g + mS - gD$ or to $(g + m) S$, therefore the quantity of redundant fluid in AB will be very nearly equal to $\frac{BW}{(g+m)S}$, and will therefore be greater than if the fluid within the glass was immoveable very nearly in the ratio of one to S, or will be very nearly the same as if the thickness of the glass was equal to $CE \times S$, and the fluid within it was immoveable.

170] PROP. XXXVI. Fig. 10. Let every thing be as in the preceding proposition, except that the electric fluid is able to penetrate into the glass on the side Nn as far as to the surface Kk, and on the side Vv as far as to Yy; in such manner, however, that though the fluid can move freely from AB to $\alpha\beta$ or from

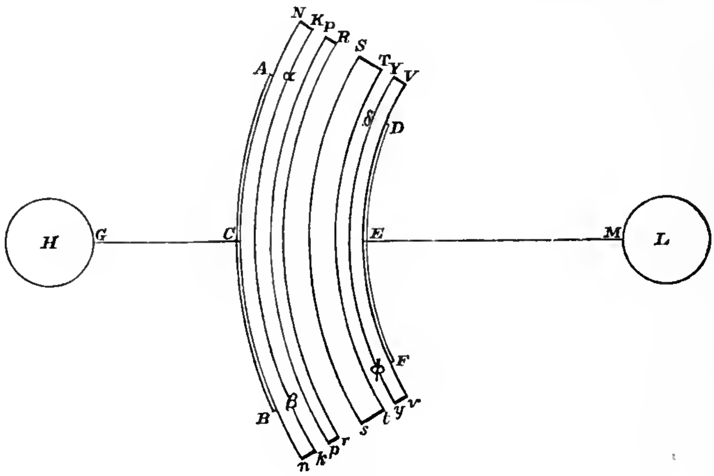


Fig. 10.

$\alpha\beta$ to AB, and also from DF to $\delta\phi$ or from $\delta\phi$ to DF, in a direction perpendicular to those surfaces, yet it is unable to move sideways, or in a direction parallel to those surfaces: the quantity of redundant fluid on one side of the glass, and of deficient fluid on the other, will be very nearly the same as if the spaces $NnkK$ and $VvyY$ were taken away and the coatings AB and DF were applied to the surfaces Kk and Yy.

For by [Art. 132] of former Part, almost all the redundant and deficient fluid will be lodged on the surfaces $\alpha\beta$ and $\delta\phi$, and the coatings AB and DF will be not much over or undercharged. Now if the whole of the redundant and deficient fluid was lodged in $\alpha\beta$ and $\delta\phi$, it is evident that the quantity of redundant and deficient fluid would be exactly the same as if the spaces $NnkK$ and $VvyY$ were taken away, and therefore it will in reality be very nearly the same.

171] COR. I. Therefore the quantity of redundant fluid on the positive side of the glass, that is, in the coating AB, and the space $A\alpha\beta B$ together, as well as the quantity of deficient fluid on the negative side of the glass, will be very nearly the same that they would be if the fluid was unable to penetrate into the glass or move within it, and that the thickness of the glass was equal only to the sum of the thicknesses of those spaces in which the fluid is immoveable.

172] Cor. II. Whether the electric fluid penetrates into the glass or not, it is evident that the quantity of redundant fluid on one side the glass, and of deficient fluid on the other, will be very nearly the same, whether the coatings are thick or thin.

173] Prop. XXXVII. It was shewn in the remarks on Prop. XXII in the first Part, that when the plate of glass is flat, and the fluid within it is immoveable, the attraction of the deficient fluid in DF makes the redundant fluid in AB to be disposed more uniformly than it would otherwise be. Now if we suppose the fluid within the glass to be moveable as in the preceding proposition, and that the deficient fluid in the planes Pp, Ss, &c. and the redundant fluid in the planes Rr, Tt, &c. is equal to, and disposed similarly to that in DF, the redundant fluid in AB will be disposed more uniformly than it would be if the fluid within the glass was immoveable, and its thickness no greater than the sum of the thicknesses of those spaces in which the fluid is immoveable.

For let the intermediate spaces be moved so that Tt shall coincide with Vv and Rr with Ss, &c., but let the distance between Tt and Ss and between Rr and Pp, &c. remain the same as before, that is, let the thickness of the spaces in which the fluid is moveable remain unaltered. The distance of Pp from Nn will now be equal to the sum of the thicknesses of the spaces $TtVv$, $RrSs$, $NnPp$, &c. in which the fluid is immoveable.

Now, after this removal, the effect of the planes Tt and DF and of Rr and Ss, &c. will destroy each other, so that the intermediate spaces and DF together will have just the same effect in rendering the redundant fluid in AB more uniform than the plane Pp alone will have, that is, the fluid in AB will be disposed in just the same manner as if the thickness of the glass was no greater than the sum of the thicknesses of the spaces in which the fluid is immoveable, and the whole fluid within the glass was immoveable.

But the effect of the intermediate spaces in making the fluid in AB more uniform was greater before their removal than after, for the effect of the two planes Pp and Rr together, and also that of Ss and Tt together, &c. is the greater the nearer they are to AB.

174] Cor. The redundant and deficient fluid in the intermediate spaces will in reality be not exactly equal and similarly disposed to that in DF, and in all probability the quantity of deficient fluid disposed near the extremity of DF will be greater than that in the corresponding parts of Pp, Ss, &c., or than the redundant fluid in the corresponding parts of Rr, Tt, &c., so that the redundant fluid in AB will perhaps be disposed rather less uniformly than it would be if the deficient and redundant fluid in those spaces was equal to and similarly disposed to that in DF; but on the whole there seems no reason to think that it will be much less, if at all less, uniformly disposed than it would be if the thickness of the glass was equal to the sum of the thicknesses of the spaces in which the fluid is immoveable, and the whole fluid within the glass was immoveable.

APPENDIX: FROM MS. NO. 5.

175] As the following propositions are not so necessary towards understanding the experiment as the former, I chose to place them here by way of appendix.

PROP. I. Let everything be as in Prop. XXXIV, except that the bodies H and L are not required to be at an infinite distance from the plates of glass; let now an overcharged body N be placed near the glass in such manner that the force with which it repels the column CG towards G shall be to that with which it repels the column EM towards M as the force with which the deficient fluid in DF attracts the column CG is to that with which it attracts EM: it will make no alteration in the quantity of redundant fluid in AB, provided the repulsion of N makes no alteration in the manner in which the fluid is disposed in each plate.

For increase the deficience of fluid in DF so much as that that coating and N together shall exert the same attraction on EM as DF alone did before, they will also exert the same attraction on CG as DF alone did before, and consequently the fluid in the two canals will be in equilibrio.

176] COR. In like manner, if the forces with which the body N repels the columns CG and EM bear the same proportion to each other as those with which the plate AB repels those columns, and therefore bear very nearly the same proportion to each other as those with which EM repels those columns, the quantity of deficient fluid in DF will be just the same as before N was brought near, and the redundant fluid in AB will be diminished by a quantity whose repulsion on CG is the same as that of N thereon.

Therefore, if the repulsion of N on CG is not greater than that of H thereon, the diminution of the quantity of redundant fluid in AB will bear but a very small proportion to the whole. For the quantity of redundant fluid in AB is many times greater than that which would be contained in it if DF was away, *id est*, than that whose repulsion on CG is equal to the repulsion of H thereon in the contrary direction.

177] PROP. II. From the preceding proposition and corollary we may conclude that if the force with which N repels the columns CG and EM bears very nearly the same proportion to each other as the force with which DF attracts those columns, the quantity of redundant fluid in AB will be altered by a quantity which will bear but a very small proportion to the whole, unless the repulsion of N on CG is much greater than that of H thereon.

If the reader wishes to see a stricter demonstration of this proposition, as well as to see it applied to the case in which the fluid is supposed moveable in the intermediate spaces, as in Prop. XXXV, he may read the following:

178] PART I. Take $Ee = \frac{1}{2}$ thickness of those spaces in which the fluid is moveable, draw def equal and similar to DEF, and let the deficient fluid therein be equal to that in DF: the repulsion of the intermediate spaces on EM is to the difference of the attractions of DF on ϵM and $\epsilon\mu$ (supposing $E\epsilon$ and $M\mu$ to be equal to CE) very nearly as twice Ee to CE, and is therefore very nearly equal to twice the difference of the attraction of df and DF on EM.

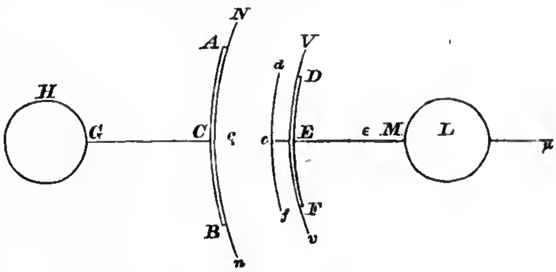


Fig. 10 a.

In like manner the attraction of the intermediate spaces on CG is very nearly equal to twice the difference of the attraction of DF and df thereon.

Suppose now the quantity of deficient fluid in DF to be increased in the ratio of $1 + f$ to 1, the redundant fluid in AB remaining the same as before, a new attraction is produced on EM, very nearly equal to

$$f \times \text{(attraction of } DF \text{ on } EM) - \frac{f}{2} \times 2 \text{ (diff. attr. of } df \text{ and } DF \text{ on } EM),$$

that is, very nearly equal to $f \times$ (attraction of df on EM).

In like manner a new attraction is produced on CG, very nearly equal to $f \times$ (attraction of df on CG), therefore, the new attraction produced on EM is to that produced on CG very nearly as the attraction of df on EM is to its attraction on CG, and therefore in order that the quantity of redundant fluid in AB shall not be altered by the approach of N, the repulsion of N on EM must be to its repulsion on CG very nearly as the attraction of df on EM to its attraction on CG.

179] PART 2. Let the fluid within the glass be either moveable, as in Prop. [XXXV, Art. 169], or let it be immoveable, and let the distance of H and L from the glass be either great or not.

Let the repulsion of H on $\begin{cases} GC \\ EM \end{cases}$ in direction GC be $\begin{cases} H \\ Hp \end{cases}$, and let the sum of these repulsions $= S$.

Let the repulsion of N on $\begin{cases} GC \text{ in direction } CG = A \\ EM \text{ in direction } EM = B \end{cases}$, and let the repulsion which N should exert on CG in order that the redundant fluid in AB should remain unaltered be to that which it should exert on EM :: 1 : P.

The quantity of redundant fluid in AB will be increased in the ratio of $1 + \frac{B - PA}{S} \frac{1 + p}{P + p}$ to 1, which, if P differs very little from 1, differs very little from that of

$$1 + \frac{B - PA}{S} \text{ to } 1.$$

For the force $\begin{cases} A \\ B \end{cases}$ may be divided into two parts, namely

$$\begin{cases} \dfrac{PA - B}{P + p} + \dfrac{pA + B}{P + p} \\[2ex] \dfrac{- PpA + pB}{P + p} + \dfrac{PpA + PB}{P + p} \end{cases},$$

but the latter part of these two repulsions, or the force $\begin{Bmatrix} \dfrac{pA + B}{P + p} \\[2ex] \dfrac{PpA + PB}{P + p} \end{Bmatrix}$, has no tendency to alter the redundant fluid in AB, but the first part, or the force

$$\begin{cases} \dfrac{PA - B}{P + p} \\[2ex] \dfrac{- PpA + pB}{P + p} \end{cases} \text{acting on } \begin{cases} CG \text{ in direction } CG \\ EM \text{ in direction } EM \end{cases}$$

or $$\begin{cases} \dfrac{- PA + B}{P + p} \\[2ex] \dfrac{- PpA + pB}{P + p} \end{cases} \text{acting on } \begin{cases} CG \text{ in direction } GC \\ EM \text{ in direction } EM \end{cases}$$

as they are to the repulsion of H on $\begin{cases} GC \\ EM \end{cases}$ as $\dfrac{- PA + B}{P + p}$ to H,

$$\text{or as } (- PA + B)\, \frac{1 + p}{P + p} \text{ to } S,$$

increases the redundant fluid in the ratio of

$$1 + \frac{B - PA}{S} \, \frac{1 + p}{P + p} \text{ to } 1.$$

180] Cor. I. If the lengths of the columns CG and EM are such that the repulsion and attraction of AB and DF on them are not sensibly less than if they were of an infinite length, the attraction of DF on CG will be very nearly equal to its attraction on EM, and therefore, if the forces with which N repels the columns CG and EM are very nearly equal to each other, the quantity of redundant fluid in AB will be very little altered thereby.

N.B. If the size of H is much greater than that of AB, it is possible that its distance from the glass may be such as to exert a very considerable repulsion on EM, and yet that the action of AB and DF on CG shall be not sensibly less than if it was of an [infinite length].

181] Cor. II. Let the bodies H and L be of the same size and shape and at an infinite distance from the glass, and let the fluid be in equilibrio. Let now an equal quantity of fluid be taken from H and L, the quantity of redundant fluid in AB will be very little altered thereby.

For the repulsion of the whole quantity of fluid in L on the canal EM will be as much diminished as that of H on CG, so that it comes to the same thing as placing an overcharged body N in such manner that its repulsion on CG

shall be equal to that on *EM*; which by the preceding proposition will make very little alteration in the quantity of redundant fluid in *AB*.

182] Cor. III. Let the bodies *H* and *L* be at an infinite distance, and either of the same or different size, and let the fluid be in equilibrio. Let now the body *H* be brought so near to *AB* that its repulsion on *GC* shall be sensibly less than before. The quantity of redundant fluid in *AB* will be very little altered thereby, provided the repulsion of the two plates on the column *CG* is not sensibly diminished.

For whereas when *H* was at an infinite distance from *AB* it exerted no repulsion on *EM*, now it is brought nearer it does exert some, and its repulsion on *EM* is very nearly equal to the diminution of its repulsion on *CG*, so that it comes to the same thing as placing a body *N* in such manner as to repel *EM* with very nearly the same force that it does *CG* in the contrary direction.

183] Cor. IV. Let the body *H* be brought near *AB* as in the preceding corollary, and let the fluid be in equilibrio; let now an overcharged body *R* be placed near *H*, the quantity of redundant fluid in *H* must be so much diminished, in order that the fluid may remain in equilibrio, supposing the fluid in *AB* to remain unaltered, as that the diminution of its repulsion on the two columns *GC* and *EM* shall be equal to the repulsion of *R* on the same columns. Consequently, if the repulsion of *R* on them is to the repulsion which *H* exerted on them before the approach of *R* as n to 1, the quantity of redundant fluid in *H* will be diminished in the ratio of $1 - n$ to 1.

For supposing the quantity of fluid in *H* to be thus diminished, I say, the quantity of fluid in *A* will remain very nearly the same as before. For the repulsion of *H* and *R* on the two columns will be the same as that of *H* was before, but it is possible that their repulsion on *GC* may be a little less, and their repulsion on *EM* as much greater than that of *H* was before, but this, by the preceding corollaries, will make very little alteration in the quantity of fluid in *AB*.

184] Cor. V. It appears from Prop. XXIII that the repulsion of the body *R* on the two columns *GC* and *EM* will be the same in whatever direction it is placed in respect of *H* and the canal, provided its distance from the point *G* is given, and consequently the diminution of the quantity of fluid in the body *H* will be very nearly the same in whatever direction *R* is situated, provided its distance from *G* is given.

185] Cor. VI. Fig. 11. Suppose now that instead of the body *H* there is placed a plate of glass *KkiI*, coated as in Props. XXXIV and XXXV, with the plates *Tt* and *Ss*, whereof *Tt* communicates with *AB* by the canal *GC*, and the other *Ss* communicates by the canal *gP* with the body *P*, placed at an infinite distance and saturated with electricity, and let *AB* and consequently *Tt* be overcharged, and let the fluid be in equilibrio.

Suppose now that an overcharged body *R* is brought near the glass *KkiI*, I say that the proportion which the redundant fluid in *Tt* bears to that in *AB* will be very little altered thereby, supposing the length of the canal *CG* to be such that the repulsion of the coatings AB and DF thereon shall be not sensibly less than if it was infinite, and that the thickness of the glass Gg is very small in respect of the distance of R from it, and that the repulsion of R does not sensibly alter the disposition of the fluid in Tt and Ss, and also that the re-

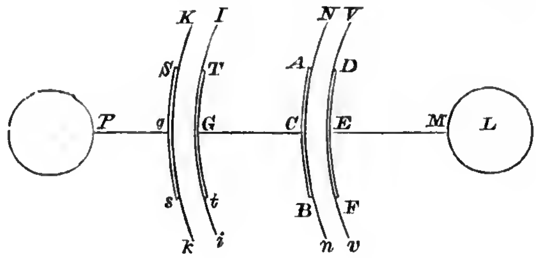


Fig. 11.

pulsion of R on GC and EM together is not much less than if GM was infinite, and also not much greater than the repulsion of the glass $NnvV$ on CG.

For let the quantity of fluid in Tt and Ss be so much altered that the united repulsion of R and those two coatings on the two canals GC and EM together, and also their repulsion on gP, shall be the same as that of the two coatings alone before the approach of R.

By Prop. II, Cor. I the quantity of fluid in Tt will be very little altered thereby, for the repulsion of R on the canal gP is very nearly the same as its repulsion on gC and EM together.

As the repulsion of Tt, Ss and R together on the two canals GC and EM together is the same as before the approach of R, it follows that if their repulsion on gC is less than before, their repulsion on EM will be as much increased.

Let now the quantity of fluid in AB and DF be so much altered that their repulsion on gC shall be as much diminished as that of $KkiI$ and R on the same column is diminished, and that their repulsion on EM shall be as much diminished as that of $KkiI$ and R on the same is increased, it is plain that the fluid in all three canals will be exactly in equilibrio, and by the preceding corollary the quantity of fluid in AB will be very little altered, and therefore the proportion of the redundant fluid in AB and Tt to each other will be very little altered*.

186] Cor. VII. By Prop. [XXIV, Art. 86] all which is said in this proposition and corollaries holds good equally whether the canals GC, EM and GP are straight or crooked.

Lemma.

* [Note 16, p. 392.]

187] Let DE be an uniform canal of incompressible fluid infinitely continued towards E, and let A and B be given points in a right line with D, and let AB be bisected in C; the force with which any particle of fluid repels this canal (supposing the repulsion to be inversely as the square of the distance) is inversely as its distance from the point D, and therefore the sum of the forces with which two equal particles of fluid placed in A and B repel this canal is to the sum of the forces with which they would repel it if both collected in the point C,

$$\text{as } \frac{1}{AD} + \frac{1}{BD} : \frac{2}{CD},$$

$$\text{or as } CD^2 : CD^2 - CB^2,$$

$$\text{or as } 1 : 1 - \frac{CB^2}{CD^2}.$$

188] Let us now examine how far the proportion of the quantity of fluid in the large circle and the two small ones in Experiment V [Art. 273] Fig. 18, bear to each other will be affected by the circumstances mentioned in [Art. 276], supposing the plates to be connected by canals of incompressible fluid.

First it appears from Cor. [VII, Art. 186], that the quantity of redundant fluid in the large circle, and also in the two small ones, will bear very nearly the same proportion to that in the jar A as it would if it had been placed at an infinite distance from A, for the distance of the plate from the jar was in neither

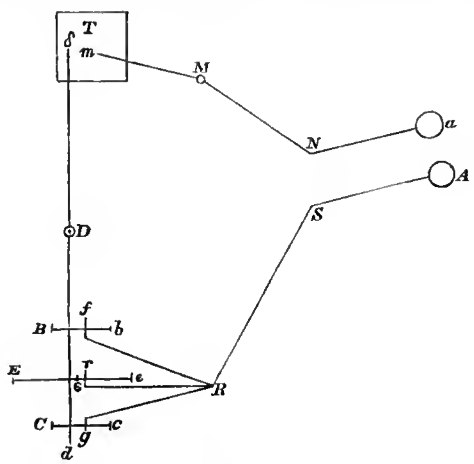


Fig. 18.

experiment less than 63 inches, and neither the length nor the diameter of the coated part of the jar exceeded four inches, so that the repulsion of the jar on the canal connecting it to the plate could not differ by more than $\frac{1}{31}$ part from what it would be if the canal was infinitely continued, and would most probably differ from it by not more than $\frac{1}{2}$ or $\frac{1}{3}$ part of that quantity*; for the same

* The repulsion of a globe 4 inches diameter on a straight uniform canal of incompressible fluid extending 63 inches from it differs by only $\frac{1}{31}$ part from what it would be if the canal was infinitely continued, but the repulsion of a Leyden vial of that size on the same column differs probably not more than $\frac{1}{2}$ or $\frac{1}{3}$ of that quantity from what it would be if infinitely continued.

reason the deficience of fluid in the trial plate will bear very nearly the same proportion to that in the jar, &c. as it would if it had been placed at an infinite distance from it.

• It is plain that if the plates had been placed at such a distance from the jar that the quantity of fluid in them had been considerably less than if they had been placed at an infinite distance, still the quantity in the large circle would bear very nearly the same proportion to that in the two small ones as it would if they had been placed at an infinite distance.

189] Secondly, it is plain that in trying the large circle, the repulsion of that circle increases the deficience of fluid in the trial plate, and the attraction of the trial plate increases the redundance in the circle. Now the repulsion of the plate Ee on the canal $mMNa$, and the attraction of the trial plate T on $rRSA$ (supposing $mMNa$ and $rRSA$ to be infinitely continued beyond a and A) are by [Cor. IV, Art. 183] very nearly the same as if the redundant fluid in Ee and the deficient fluid in T were both collected in the centers of their respective plates, and the quantity of redundant fluid in Ee may be considered as equal to the deficient in T, and consequently the repulsion of Ee on $mMNa$ is very nearly equal to the attraction of T on $rRSA$. Moreover, the repulsion of Ee on its own canal $rRSA$ must be equal to the attraction of T on $mMNa$, as the jars with which they communicate are both equally electrified, and therefore, by Cor. [IV], the quantity of redundant fluid in Ee will be increased in very nearly the same ratio as the deficient in T.

190] In like manner, in trying the two small circles, the quantity of redundant fluid in them is increased in very nearly the same ratio as the deficient in T, for as half the distance of the two circles never bore a greater proportion to ϵm than that of 18 to 72, the repulsion of the two circles on the canal $mMNa$ will be very nearly the same, and the deficience of fluid in T will be increased in very nearly the same ratio as if all the redundant fluid in them were collected in ϵ, the middle point between them.

The quantity of redundant fluid in Bb indeed will be increased in a rather greater ratio, and that in Cc in a rather less ratio than if it was placed at ϵ, but the ratio in which the quantity of fluid in Bb is increased must very nearly as much exceed that in which it would be increased if it was placed at ϵ as that in which Cc is increased falls short of it, as the attraction of T on the canal $fRSA$ exceeds that on $rRSA$ by nearly the same quantity as its attraction $gRSA$ falls short of it, and therefore the quantity of redundant fluid in both circles together is increased in very nearly the same proportion as that in a circle placed in ϵ would be, and consequently the redundance in the two circles is increased in very nearly the same ratio as the deficience in the trial plate.

191]* The attraction of the trial plate on the canals $fRSA$ and $gRSA$ and the repulsion of the circles Bb and Cc on the canal $mMNa$ is very nearly the same as if the deficient or redundant fluid in the plates was collected in the center of their respective plates, and therefore the repulsion of the circles Bb and Cc on the canal

* Memorandum relating to the second article.

mMNn is inversely as the distances of their centers from *m*, and the increase of the quantity of redundant fluid in the circles *Bb* and *Cc* by the attraction of *T* is in the same proportion.

Therefore take the point *a* so that the repulsion of a particle at *a* on that canal shall be a mean between the repulsions of the same particle thereon when placed at *B* and *C*, the charge of *T* will be increased in the same proportion as it would be by the repulsion of a plate containing as much redundant fluid as the two plates together whose center was *a*, and the charge of the two circles together will also be increased in the same proportion as that of the circle whose center is *a* would be thereby.

192] Consequently, in trying either the large circle or the two small ones, the trial plate must be opened to very nearly the same surface to contain the same charge as them as it must be if they were placed at an infinite distance from the trial plate, and consequently no sensible alteration can be produced in the phænomena of the experiment by the repulsion and attraction of the circles and trial plate on each other.

193] Thirdly, for the same reason it appears that as the circles and the trial plate are both at much the same distance from the ground and walls of the room, no sensible alteration can be produced in the experiment by the ground near the circles being rendered undercharged and that near the trial plate overcharged.

It must be observed, indeed, that the distance of the circles and trial plate from the ground is much less than their distance from each other, and consequently the alteration of the charge of the two circles and trial plate produced by this cause will not be so nearly alike as that caused by their attraction and repulsion on each other; but as, on the other hand, the whole alteration of their charge produced by this cause is, I imagine, much less than that produced by the other, I imagine that this cause can hardly have a more sensible effect in the experiment than the preceding.

194] Fourthly, we have not as yet taken notice that the canals by which the jars *A*, *a* communicate with the ground are but short, and meet the ground at no great distance from the jars.

But it may be shewn by the same kind of reasoning used in Prop. [II, Art. 178], with the help of the second corollary to the preceding proposition, that the quantity of redundant fluid in the circles will bear very nearly the same proportion to that in the positive side of the jar *A*, whether the canal by which *A* communicates with the ground is long or short.

Besides that, if it was possible for this circumstance to make much alteration in the proportion which the redundant fluid in the circles bears to that in *A*, it would in all probability have very nearly the same effect in trying the two small circles as in trying the large one, so that no sensible alteration can be produced in the experiment from this circumstance.

It appears, therefore, that none of the above-mentioned circumstances can cause any sensible alteration in this experiment*.

* [Note 17, p. 394.]

THOUGHTS CONCERNING ELECTRICITY

[From MS. Nº 10: probably an early draft of the theory.]
{See Table of Contents at the beginning of this volume.}

195] Electricity seems to be owing to a certain elastic fluid interspersed between the particles of bodies, and perhaps also surrounding the bodies themselves in the form of an atmosphere.

196] This fluid, if it surrounds bodies in the form of an atmosphere, seems to extend only to an imperceptible distance from them*, but the attractive and repulsive power of this fluid extends to very considerable distances.

197] That the attraction and repulsion of electricity extend to considerable distances is evident, as corks are made to repel by an excited tube held out at a great distance from them. That the electric atmospheres themselves cannot extend to any perceptible distance, I think, appears from hence, that if two electric conductors be placed ever so near together so as not to touch, the electric fluid will not pass rapidly from one to the other except by jumping in the form of sparks, whereas if their electric atmospheres extended to such a distance as to be mixed with one another, it should seem as if the electricity might flow quietly from one to the other in like manner as it does through the pores of any conducting matter.

But the following seems a stronger reason for supposing that these atmospheres cannot extend to any perceptible distance from the body they surround, for if they did it should seem that two flat bodies whenever they were laid upon one another should always become electric thereby, for in that case there is no room for the electric atmosphere to extend to any sensible distance from those surfaces of the bodies which touch one another, so that the electric fluid which before surrounded those surfaces would be forced round to the opposite sides, which would thereby become overcharged with electricity, and consequently appear electrical, which is contrary to experience.

198] Many Electricians seem to have thought that electrified bodies were surrounded with atmospheres of electric matter extending to great distances from them. The reasons which may have induced them to think so may be first, that an electrified body affects other bodies at a considerable distance. But this may, with much more probability, be supposed

* There are several circumstances which shew that two bodies, however smooth and strongly pressed together, do not actually touch each other. I imagine that the distance to which the electric atmospheres, if there are any, extend must be less than the smallest distance within which two bodies can be made to approach.

owing to the attraction and repulsion of the electric matter within the body or close to its surface. And, secondly, because a body placed near a positively electrified body receives electricity itself, whence it is supposed to receive that electricity from the electrified body itself, and therefore to be within its atmosphere. But, in all probability, the body in this case receives its electricity from the contiguous air, and not immediately from the electrified body, as will be further explained in its place.

199] Let any number of bodies which conduct electricity with perfect freedom be connected together by substances which also conduct electricity. It is plain that the electric fluid must be equally compressed* in all these bodies, for if it was not, the electric fluid would move from those bodies in which it was more compressed to those in which it was less compressed till the compression became equal in all. But yet it is possible that some of these bodies may be made to contain more than their natural quantity of electricity, and others less. For instance, let some power be applied to some of these bodies which shall cause the electric fluid within their pores to expand and grow rarer †, those bodies will thereby be made to contain less electric matter than they would otherwise do, but yet the electric matter within them will be just as much compressed as it would be if this power were not applied.

On the other hand, if some power were applied which shall diminish the elasticity of the electric fluid within them and thereby make it grow more dense, those bodies will be made to contain more electricity, but yet the compression will remain still the same.

200] To make what is here said more intelligible, let us suppose a long tube to be filled with air, and let part of this tube, and consequently the air within, be heated, the air will thereby expand, and consequently that part of the tube will contain less air than it did before, but yet the air in that part will be just as much compressed as in the rest of the tube.

In like manner, if you suppose the electric fluid to be not only confined within the pores of bodies, but also to surround them in the form of an atmosphere, let some power be applied to some of those bodies which shall prevent this atmosphere from extending to so great a distance from

* Note by Editor. [That is, must sustain an equal pressure. In modern scientific language the words compression, extension, distortion, are used to express *strain*, or change of form, while pressure, tension, torsion, are reserved to indicate the *stress* or internal force which accompanies this change of form. Cavendish uses the word compression to indicate stress. The idea is precisely that of *potential*.]

† [No such power has been discovered. There is nothing among electrical phenomena analogous to the expansion of air by heat.—Ed.] {Nowadays the free electrons in a metal, or on a large scale in an incandescent star, are often treated theoretically as constituting the molecules of a gaseous medium, responding to change of temperature. It is interesting to compare the text with electron theories of conductance and equilibrium.}

them, those bodies will thereby be made to contain less electricity than they would otherwise do, but yet the electric fluid that surrounds them will be just as much compressed as it would [be] if that power was not applied.

It will surely be needless to warn the reader here not to confound compression and condensation.

201] I now proceed to my hypothesis.

DEF. 1. When the electric fluid within any body is more compressed than in its natural state, I call that body positively electrified: when it is less compressed, I call the body negatively electrified.

It is plain from what has been here said that if any number of conducting bodies be joined by conductors, and one of the bodies be positively electrified, that all the others must be so too.

DEF. 2. When any body contains more of the electric fluid than it does in its natural state, I call it overcharged. When it contains less, I call it undercharged.

202] HYP. 1st. Every body overcharged with electricity repels an overcharged body, and attracts an undercharged one.

HYP. 2nd. Every undercharged body attracts an overcharged body, and repels an undercharged one.

HYP. 3rd. Whenever any body overcharged with electricity is brought near any other body, it makes it less able to contain electricity than before.

HYP. 4th. Whenever an undercharged body is brought near another it makes it more able to contain electricity.

203] COR. I. Whenever any body at a distance from any other electrified body is positively electrified it will be overcharged, and if negatively electrified it will be undercharged.

COR. II. If two bodies, both perfectly insulated, so that no electricity can escape from them, be positively electrified and then brought near to each other, as they are both overcharged they will each, by the action of the other upon it, be rendered less capable of containing electricity, therefore, as no electricity can escape from them, the fluid within them will be rendered more compressed, just as air included within a bottle will become more compressed either by heating the air or by squeezing the bottle into less compass; but it is evident that the bodies will remain just as much overcharged as before.

204] COR. III. If two bodies be placed near together, and then equally positively electrified, they will each be overcharged, but less so than they would [be] if they had not been placed near together.

It may perhaps be said that this is owing to the electric atmosphere not having so much room to spread itself when the two bodies are brought near together as when they are at a distance; but I think it has already been sufficiently proved that these atmospheres cannot extend to any sensible distance from their respective bodies.

Cor. IV. If two bodies are placed near together and then equally negatively electrified, they will each be undercharged, but less so (*id est*, they will contain more electricity) than if placed at a distance.

This phenomenon cannot be accounted for on the foregoing supposition.

205] Cor. V. If a body overcharged with electricity be brought near a body not electrified and not insulated, part of the electric fluid will be driven out of this body, and it will become undercharged.

But if the body be insulated, as in that case the electric fluid cannot escape from it, it will not become undercharged, but the electric fluid within it will be more compressed than in its natural state, *id est*, the body will become positively electrified, and will remain so as long as the overcharged body remains near it, but will be restored to its natural state as soon as the overcharged body is taken away, provided no electricity has escaped during the meantime.

This is in effect the same case as that described in the 5th experiment of Mr Canton's paper in the 48th vol. of [the *Philosophical*] *Transactions*, p. 353, and is explained by him much in the same manner as is done here.

206] Cor. VI. If a body positively electrified in such a manner that if it is by any means made more or less capable of containing electricity, the electric fluid shall run into it from without or shall run out of it, so as to keep it always equally electrified, be brought near another body not electrified and not insulated, the second body will thereby be rendered undercharged, whereby the first body will become more capable of containing electricity, and consequently will become more overcharged than it would otherwise be with the same degree of electrification. This again will make the second body more undercharged, which again will make the first body more overcharged, and so on.

It must be observed here, that if the two bodies are brought so near together that their action on one another shall be considerable, the electricity will jump from one to the other; otherwise if the two bodies were brought so near together that their distance should not be greater than the thickness of the glass in the Leyden bottle, it seems likely that the first body might receive many times as much additional electricity as it would otherwise receive by the same degree of electrification; and that the second body would lose many times as much electricity as it would by the same degree of negative electrification.

If the second body be negatively electrified, the same effect will be produced in a greater degree.

It may also happen that the second body shall be made undercharged though it is positively electrified, provided it be much less electrified than the first body, and that the two bodies be placed near enough to each other.

207] The shock produced by making a communication between the two surfaces of the Leyden vial seems owing only to the glass prepared in that manner containing vastly more electricity on its positive side than an equal surface of metal equally electrified, and vastly less on its negative side than the same surface of metal negatively electrified to the same degree, so that if two magazines of electricity were prepared, each able to receive as much additional electricity by the same degree of electrification as one of the surfaces of a Leyden vial, and one of the magazines was to be positively electrified and the other negatively, there is no doubt but what as great a shock would be produced by making a communication between the two magazines as between the two surfaces of the Leyden vial.

I think, therefore, that the phenomena of the Leyden vial may very well be accounted for on the principle of the 6th Corollary, for in the Leyden vial the two surfaces of the glass are so near together, that the electric matter on one surface may act with great force on that on the other, and yet the electricity cannot jump from one surface to the other, by which means perhaps the positive side may be made many times more overcharged, and the negative side many times more undercharged, than it would otherwise be.

208] Hyp. 5th. It seems reasonable to suppose that when the electric fluid within any body is more compressed than it is in the air surrounding it, it will run out of that body, and when it is less compressed it will run into the body.

Cor. I. Let the body *A*, not electrified, be perfectly insulated, and let an overcharged body be brought near it. The body *A* will thereby be rendered less capable of containing electricity, and therefore the electric fluid within it, as it cannot escape, will be rendered more compressed. But the electricity in the adjoining air will, for the same reason, be also compressed, and in all probability equally so, therefore the electricity will have no disposition either to run in or out of the body.

Cor. II. It is evidently the same thing whether *A* be insulated, or whether it be not insulated, but electrified in such manner that the fluid within it be as much compressed as it was before by virtue of the insulation. Therefore if the body *A* be now not insulated, but positively electrified, and an overcharged body be brought to such a distance from it that the electric fluid in the adjacent air be equally compressed with that in *A*, such a quantity of electricity will thereby be driven out of *A* that it will retain only its natural quantity. So that *A* will be neither overcharged nor undercharged, nor will the electricity have any disposition to run either in or out of it.

209] If the overcharged body be now brought nearer, A will become undercharged, and the electricity will run into it from the surrounding air. If the overcharged body be not brought so near A will be overcharged, and the electricity will run out of it. If an undercharged body be brought near A it will become more overcharged than before, and the electricity will run out stronger than before.

COR. III. If the body A be negatively electrified, and an undercharged body be brought near it till the electric fluid in the adjoining air is as much compressed as that in the body A, the electricity will have no disposition to run either in or out of A, nor will it be either overcharged or undercharged, as will appear from the same way of reasoning as was used with regard to the 2nd Corollary.

If the undercharged body be now brought nearer, A will become overcharged, and the electricity will also run out of it. If the undercharged body be removed farther off, A will become undercharged, and the electricity will also run into it. If an overcharged body be brought near to A, it will become more undercharged than before, and the electricity will also run in faster than before.

On the whole, therefore, it appears that whenever a body is undercharged the electricity will run into it, and whenever it is overcharged it will run out.

210] It has usually been supposed that two bodies, whenever the electricity either runs into or out of both of them, repel each other; but that when it runs into one and out of the other, they attract. In the beginning of this paper I laid down a different rule for the electric attraction and repulsion, namely, that when the two bodies are both overcharged or both undercharged they repel, but attract when one is overcharged and the other undercharged.

But by what has been just said it appears that these two rules agree together, or at least if they do differ, they differ so little that there is no reason to think my rule will agree less with experiment than the other.

The reasoning here used would have been more satisfactory if the bodies were capable of containing electricity only on one side, namely, on that which is turned towards the other body. But I do not imagine, however, that this will make much difference in the effect.

211] What has been here said holds good only in cases where the size of the body A is small in respect of the distance of the electrified body from it, so that the influence of the electrified body may be nearly the same on all parts of the body A as is the case in bits of cork held near an excited tube; but when the size of the body A is such that the influence of the electrified body may be much greater on that part of *A* which is directly under it than on that which is farther removed from it, as is the case in electrifying a prime conductor by an excited tube, then the case is very different, for then on approaching the electrified tube, part of the electric fluid will be driven away from that part of the prime conductor which is nearest the excited tube to the remoter parts where its influence is weaker, whereby that part of the conductor nearest the tube will be undercharged, and consequently the compression of the electric fluid in that part will be less than in the contiguous air, consequently some electric matter will flow into it from the adjoining air, whereby the conductor will be overcharged, and therefore on taking away the tube will be positively electrified.

Thus if the excited tube or other electrified body is not brought within a certain distance, the conductor receives its electricity only from the contiguous air, as was before said, and not immediately from the electrified body; but if the body be brought near enough, the electric matter jumps from the electrified body to the conductor in form of a spark.

212] The means by which this is brought about seems thus—When the part of the conductor nearest the excited tube has received any electricity from the contiguous air, that air will be undercharged, and will receive electricity from the adjacent air between it and the tube, by which means the electric matter will flow in gentle current between the particles of air from the excited tube to the conductor. It seems now as if the particles of air were by this means made to repel each other with more force, and thereby to become rarer; this will suffer the electric fluid to flow in a swifter current, which again will increase the repulsion of the particles of air, till at last a vacuum is made, upon which the electric fluid jumps in a continued body to the conductor.

213] That a vacuum is formed by the electric fluid when it passes in the form of a spark through air or water appears, I think, from the violent rising of the water in Mr Kinnersley's electrical air-thermometer (Priestley, p. 216), and still more strongly from the bursting the vial of water, in Mr Lane's experiment, by making the electrical fluid pass through the water in the form of a spark.

If I am not much mistaken I have frequently observed, in discharging a Leyden vial, that if the two knobs are approached together very gently, a hissing noise may be perceived before the spark, which shews that the electricity does begin to flow from one knob to the other before it moves in the form of the spark, and may therefore induce one to think that the spark is brought about in the gradual manner here described.

214] The attraction and repulsion of electrified bodies, according to the law I have laid down, may perhaps be accounted for in the following manner. Let a fluid consisting of particles mutually repelling each other, and whose repulsion extends to considerable distances, be spread uniformly all over the globe, except in the space A, which we will suppose to contain more than its proper quantity of the fluid. The fluid placed in any space B within reach of the repulsion of A will be repelled from A with more force than it will [be] in any other direction. But as it cannot recede from A without an equal quantity of the fluid coming into its room which will be equally repelled from A, it is plain that it will have no tendency to recede from A, any more than a body of the same specific gravity as water has any tendency to sink in water. Let now the space B be made to contain more than its natural quantity of this fluid, it will then really have a tendency to recede from A, or will appear to be repelled by it, just as a body heavier than water tends to descend in it, and, on the contrary, if B is made to contain less than its natural quantity of the fluid, it will have a tendency towards A, or will appear to be attracted by it.

215] Let now the space A be made to contain less than its natural quantity of the fluid (as the fluid in B is now repelled from A with less force than it is in any other direction, *id est*, apparently attracted towards it), if B also contain less than its natural quantity of the fluid it will tend to recede from A, *id est*, appear to be repelled by it; but if B contain more than its natural quantity, it will then tend to approach towards A, *id est*, appear to be attracted by it.

216] If the electric fluid is diffused uniformly through all bodies not appearing electrical and the repulsion of its particles extends to considerable distances, it is plain that the consequences are such as are here described; but how far that supposition will agree with experiment I am in doubt*.

* [Note 18, p. 397.]

EXPERIMENTS ON ELECTRICITY

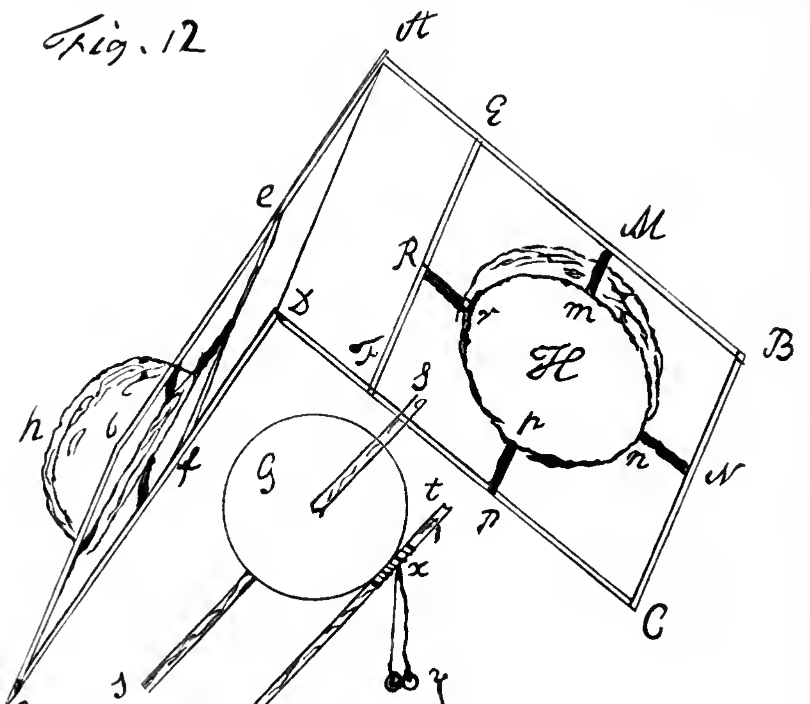
{See the reasoned summary of methods and results in the latter part of Prof. Clerk Maxwell's 'Introduction.'}

{I.} EXPERIMENTAL DETERMINATION OF THE LAW OF ELECTRIC FORCE.

[From MS. No. 7: apparently prepared for publication. See Table of Contents at the beginning of this volume.]

217] I now proceed to give an account of the experiments, in all of which I shall suppose, according to the received opinion, that the electricity of glass is positive, but it is not at all material to the purpose of this paper whether it is so or not, for if it was negative, all the experiments would agree equally well with the theory.

218] Experiment I. The intention of the following experiment was to find out whether, when a hollow globe is electrified, a smaller globe



inclosed within it and communicating with the outer one by some conducting substance is rendered at all over or undercharged; and thereby to discover the law of the electric attraction and repulsion.

219] I took a globe 12·1 inches in diameter, and suspended it by a solid stick of glass run through the middle of it as an axis, and covered with sealing-wax to make it a more perfect non-conductor of electricity. I then inclosed this globe between two hollow pasteboard hemispheres, 13·3 inches in diameter, and about $\frac{1}{20}$ of an inch thick, in such manner that there could hardly be less than $\frac{4}{10}$ of an inch distance between the globe and the inner surface of the hemispheres in any part, the two hemispheres being applied to each other so as to form a complete sphere, and the edges made to fit as close as possible, notches being cut in each of them so as to form holes for the stick of glass to pass through.

By this means I had an inner globe included within an hollow globe in such manner that there was no communication by which the electricity could pass from one to the other.

I then made a communication between them by a piece of wire run through one of the hemispheres and touching the inner globe, a piece of silk string being fastened to the end of the wire, by which I could draw it out at pleasure.

220] Having done this I electrified the hemispheres by means of a wire communicating with the positive side of a Leyden vial, and then, having withdrawn this wire, immediately drew out the wire which made a communication between the inner globe and the outer one, which, as it was drawn away by a silk string, could not discharge the electricity either of the globe or hemispheres. I then instantly separated the two hemispheres, taking care in doing it that they should not touch the inner globe, and applied a pair of small pith balls, suspended by fine linen threads, to the inner globe, to see whether it was at all over or undercharged.

221] For the more convenient performing this operation, I made use of the following apparatus. It is more complicated, indeed, than was necessary, but as the experiment was of great importance to my purpose, I was willing to try it in the most accurate manner.

ABCDEF and *AbcDef* (Fig. 12) are two frames of wood of the same size and shape, supported by hinges at *A* and *D* in such manner that each frame is moveable on the horizontal line *AD* as an axis. *H* is one of the hemispheres, fastened to the frame *ABCD* by the four sticks of glass *Mm*, *Nn*, *Pp*, and *Rr*, covered with sealing-wax. *h* is the other hemisphere fastened in the same manner to the frame *AbcD*. *G* is the inner globe, suspended by the horizontal stick of glass *Ss*, the frame of wood by which *Ss* and the hinges at *A* and *D* are supported being not represented in the figure to avoid confusion.

Tt is a stick of glass with a slip of tinfoil bound round it at *x*, the place where it is intended to touch the globe, and the pith balls are suspended from the tinfoil.

The hemispheres were fixed within their frames in such manner that when the frames were brought near together the edges of the hemispheres touched each other all round as near as might be, so as to form a complete

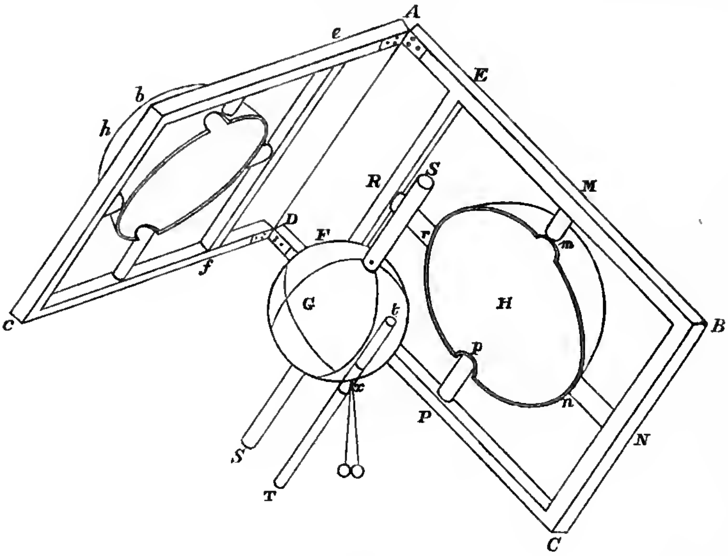


Fig. 12.

sphere, and so that the inner globe was inclosed within them without anywhere touching them, but on the contrary being at nearly the same distance from them in all parts.

222] It was also so contrived, by means of different strings, that the same motion of the hand which drew away the wire by which the hemispheres were electrified, immediately after that was done, drew out the wire which made the communication between the hemispheres and the inner globe, and immediately after that was drawn out, separated the hemispheres from each other and approached the stick of glass Tt to the inner globe. It was also contrived so that the electricity of the hemispheres and of the wire by which they were electrified was discharged as soon as they were separated from each other, as otherwise their repulsion might have made the pith balls to separate, though the inner globe was not at all overcharged.

The inner globe and hemispheres were also both coated with tinfoil to make them the more perfect conductors of electricity.

223] In trying the experiments a coated glass jar was connected to the wire by which the hemispheres were electrified, and this wire was withdrawn so as not to touch the hemispheres till the jar was sufficiently charged. It was then suffered to rest on them for two or three seconds and then withdrawn, and the hemispheres separated as above described.

224] An electrometer also was fastened to the prime conductor by which the coated jar was electrified, by which means the jar and consequently the hemispheres were always electrified in the same degree. This electrometer as well as the pith balls will be described in [Arts. 244 and 248]; the strength of the electricity was the same as was commonly used in the following experiments, and is described in [Arts. 263, 329, 359, 520].

225] My reason for using the glass jar was that without it it would have been difficult either to have known to what degree the hemispheres were electrified or to have kept the electricity of the same strength for a second or two together, and if the wire had been suffered to have rested on the hemispheres while the jar was charging, I was afraid that the electricity might have spread itself gradually on the sticks of glass which supported the globe and hemispheres, which might have made some error in the experiment.

226] From this manner of trying the experiment it appears:

First, that at the time the hemispheres are electrified, there is a perfect communication by metal between them and the inner globe, so that the electricity has free liberty to enter the inner globe if it has any disposition to do so, and moreover that this communication is not taken away till after the wire by which the hemispheres are electrified is removed.

Secondly, before the hemispheres begin to be separated from each other, the wire which makes the communication between them and the globe is taken away, so that there is no longer any communication between them by any conducting substance.

Thirdly, from the manner in which the operation is performed, it is impossible for the hemispheres to touch the inner globe while they are removing, or even to come within $\frac{4}{10}$ths of an inch of it.

And Fourthly, the whole time of performing the operation is so short, that no sensible quantity of electricity can escape from the inner globe, between the time of taking away the communication between that and the hemispheres, and the approaching the pith balls to it, so that the quantity of electricity in the globe when the pith balls are approached to it cannot be sensibly different from what it is when it is inclosed within the hemispheres and communicating with them.

227] The result was, that though the experiment was repeated several times*, I could never perceive the pith balls to separate or shew any signs of electricity.

228] That I might perceive a more minute degree of electricity in the inner globe, I tried the experiment in a different manner, namely, before the hemispheres were electrified, I electrified the pith balls positively, making them separate about one inch. When the hemispheres were then separated, and the tinfoil, x, brought in contact with the globe, and

[* Dec. 18–24, 1772, Arts. 512, 513, and April 4, 1773, Art. 562.]

consequently the electricity of the pith balls communicated to the globe, they still continued to separate, though but just sensibly. I then repeated the experiment in the same manner, except that the pith balls were negatively electrified in the same degree that they before were positively. They still separated negatively after being brought in contact with the globe, and in the same degree that they before did positively.

229] It must be observed that if the globe was at all overcharged the pith balls should separate further when they were previously positively electrified than when negatively, as in the first case the pith balls must evidently separate further than they would do if the globe was not overcharged, and in the latter case less.

Moreover, a much smaller degree of electricity may be perceived in the globe by this manner of trying the experiment than the former, for when the pith balls have already got a sufficient quantity of electricity in them to make them separate, a sensible difference will be produced in their degree of divergence by the addition of a quantity of fluid several times less than what was necessary to make them separate at first. It is plain that this method of trying the experiment is not just, unless the hemispheres are electrified in nearly the same degree when the pith balls are previously electrified positively as when negatively, which was provided for by the electrometer.

230] In order to find how small a quantity of electricity in the inner globe might have been discovered by this experiment, I took away the hemispheres with their frames, leaving the globe and the pith balls as before. I then took a piece of glass, coated as a Leyden vial, which I knew by experiment contained not more than $\frac{1}{59}$th of the quantity of redundant fluid on its positive side that the jar by which the hemispheres were electrified did, when both were charged from the same conductor.

I then electrified this coated plate to the same degree, as shewn by the electrometer, that the jar was in the former experiment, and then separated it from the prime conductor, and communicated its electricity to the jar, which was not at all electrified. Consequently the jar contained only $\frac{1}{60}$th part of the redundant fluid in this experiment that it did in the former, for the coated plate and jar together contained only $\frac{1}{59}$th, and therefore the jar alone contained only $\frac{1}{60}$th.

By means of this jar, thus electrified, I electrified the globe in the same manner that the hemispheres were in the former experiment, and immediately after the electrifying wire was withdrawn, approached the pith balls. The result was that by previously electrifying the balls, as in the second way of trying the experiment, the electricity of the globe was very manifest, as the balls separated very sensibly more when they were previously electrified positively than when negatively, but the electricity of the globe was not sufficient to make the balls separate, unless they were previously electrified.

It is plain that the quantity of redundant fluid communicated to the globe in this experiment was less than $\frac{1}{60}$th part of that communicated to the hemispheres in the former experiment, for if the hemispheres themselves had been electrified they would have received only $\frac{1}{60}$th of the redundant fluid they did before, and the globe, as being less, received still less electricity.

231] It appears, therefore, that if a globe 12·1 inches in diameter is inclosed within a hollow globe 13·3 inches in diameter, and communicates with it by some conducting substance, and the whole is positively electrified, the quantity of redundant fluid lodged in the inner globe is certainly less than $\frac{1}{60}$th of that lodged in the outer globe, and that there is no reason to think from any circumstance of the experiment that the inner globe is at all overcharged.

232] Hence it follows that the electric attraction and repulsion must be inversely as the square of the distance, and that when a globe is positively electrified, the redundant fluid in it is lodged intirely on its surface.

For by Prop. V [Art. 20], if it is according to this law, the whole redundant fluid ought to be lodged on the outer surface of the hemispheres, and the inner globe ought not to be at all over or undercharged, whereas, if it is inversely as some higher power of the distance than the square, the inner globe ought to be in some degree overcharged.

233] For let *ADB* (Fig. 13) be the hemispheres and *adb* the inner globe, and *Aa* the wire by which a communication is made between them. By Lemma IV [Art. 18], if the electric attraction and repulsion is inversely as some higher power of the distance than the square, the redundant fluid in *ABD* repels a particle of fluid placed anywhere in the wire *Aa* towards the center, and consequently, unless the inner globe was sufficiently overcharged to prevent it, some fluid would flow from the hemispheres to the globe.

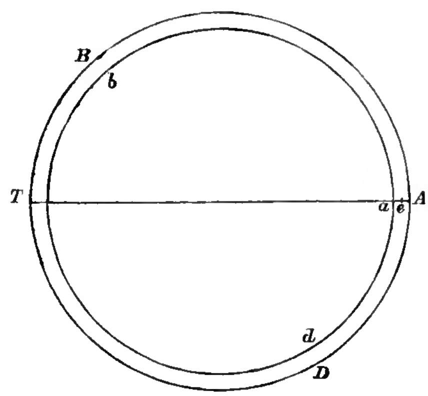


Fig. 13.

But if the electric attraction and repulsion is inversely as some lower power of the distance than the square, the redundant fluid in *ABD* impels the particle in the contrary direction, that is, from the center, and therefore the inner globe must be undercharged.

234] In order to form some estimate how much the law of the electric attraction and repulsion may differ from that of the inverse duplicate ratio of the distances without its having been perceived in this experiment, let AT be a diameter of the two concentric spheres ABD and abd, and let Aa be bisected in e. Ae in this experiment was about ·35 of an inch and Te 13·1 inches, therefore if the electric attraction and repulsion is inversely as the $2 + \frac{1}{50}$th power of the distance, it may be shewn that the force with which the redundant fluid in ABD repels a particle at e towards the center is to that with which the same quantity of fluid collected in the center would repel it in the contrary direction as 1 to 57.

But as the law of repulsion differs so little from the inverse duplicate ratio, the redundant fluid in the inner globe will repel the point e with very nearly the same force as if it was all collected in the center, and therefore if the redundant fluid in the inner globe is $\frac{1}{57}$th part of that in ABD the particle at e will be in equilibrio, and as e is placed in the middle between A and a, there is the utmost reason to think that the fluid in the whole wire Aa will be so too. We may therefore conclude that the electric attraction and repulsion must be inversely as some power of the distance between that of the $2 + \frac{1}{50}$th and that of the $2 - \frac{1}{50}$th, and there is no reason to think that it differs at all from the inverse duplicate ratio*.

235] EXPERIMENT II. A similar experiment was tried with a piece of wood 12 inches square and 2 inches thick, inclosed between two wooden drawers each 14 inches square and 2 inches deep on the outside, so as to form together a hollow box 14 inches square and 4 thick, the wood of which it was composed being ·5 to ·3 of an inch thick.

The experiment was tried in just the same manner as the former. I could not perceive the inner box to be at all over or undercharged, which is a confirmation of what was supposed at the end of Prop. IX [Art. 41]—that when a body of any shape is overcharged, the redundant fluid is lodged entirely on the surface, supposing the electric attraction and repulsion to be inversely as the square of the distance†.

DEMONSTRATION OF COMPUTATIONS IN [ART. 234].

Let aef be a sphere, c its center, b any point within it, af a diameter, Ee any plane perpendicular to af.

Let $cb = a$, $ba = d$, $bf = s$ and $ad = x$, and let the repulsion be inversely as the n power of the distance. The convex surface of the segment Eae is to that of the whole globe as $ad : af$, and therefore if the point d is supposed to flow towards f, the fluxion of the surface Eae is proportional to x, and the fluxion of its repulsion on b in the direction dc is proportional to

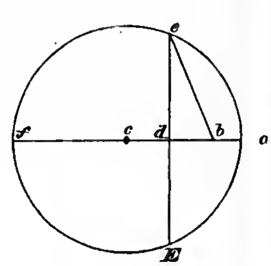


Fig. 13 *a*.

$$\frac{\dot{x}\,(d - x)}{be^{n+1}},$$

* [Note 19, p. 404.]

† [Art. 561.]

or may be represented thereby, but

$$be^2 = (d - x)^2 + x\,(2a + 2d - x) = d^2 + 2ax,$$

therefore the fluxion of the repulsion is

$$\frac{\dot{x}\,(d - x)}{(d^2 + 2ax)^{\frac{n+1}{2}}},$$

the variable part of the fluent of which is

$$\frac{-\,2ad - d^2}{4a^2\,\frac{n - 1}{2}\,(d^2 + 2ax)^{\frac{n-1}{2}}} - \frac{(d^2 + 2ax)^{\frac{3-n}{2}}}{4a^2\,\frac{3 - n}{2}};$$

but when x is nothing, $d^2 + 2ax$, or $be^2 = d^2$, and when $x = af$, or $s + d$, it $= s^2$, therefore the whole fluent generated while b moves from a to f is

$$\frac{2ad + d^2}{2a^2\,(n - 1)}\left(\frac{1}{d^{n-1}} - \frac{1}{s^{n-1}}\right) + \frac{d^{3-n} - s^{3-n}}{2a^2\,(3 - n)};$$

but the repulsion of all the fluid collected in the center on b

$$= \frac{s + d}{a^n},$$

and

$$a = \frac{s - d}{2},$$

and

$$2ad + d^2 = ds,$$

therefore the repulsion of the surface of the globe is to that of the same quantity of fluid collected in the center as

$$\frac{ds}{n - 1} \times \frac{s^{n-1} - d^{n-1}}{(ds)^{n-1}} + \frac{d^{3-n} - s^{3-n}}{3 - n} : \frac{2\,(s + d)}{a^{n-2}},$$

or as

$$\frac{s^{n-1} - d^{n-1}}{(n - 1)\,(ds)^{n-2}} + \frac{d^{3-n} - s^{3-n}}{3 - n} : \frac{(s + d)\,2^{n-1}}{(s - d)^{n-2}},$$

or dividing by s^{3-n}, as

$$\frac{s^{n-2}}{d^{n-2}\,(n - 1)} - \frac{d}{s\,(n - 1)} + \frac{d^{3-n}}{s^{3-n}\,(3 - n)} - \frac{1}{3 - n} : \frac{s + d}{s}\left(\frac{s - d}{s}\right)^{2-n} 2^{n-1},$$

or as

$$\frac{p^{2-n} - p}{n - 1} + \frac{p^{3-n} - 1}{3 - n} : (1 + p)\,2^{n-1}\,(1 - p)^{2-n},$$

supposing $\frac{d}{s}$ to be called p.

[{II.} EXPERIMENTS ON THE CHARGES OF BODIES.]

[From MS. Nos. 9 and 10: apparently prepared for publication. See Table of Contents at the beginning of this volume.]

236] The intention of the remaining experiments was to find out the proportion which the quantity of redundant fluid in bodies of several different shapes and sizes would bear to each other if placed at a considerable distance from each other and connected together by a slender wire, or, which comes to the same thing, to find the proportion which the quantity of redundant fluid in them would bear to each other if they were successively connected by a slender wire to a third body placed at a great distance from them, supposing the quantity of redundant fluid in the third body to be the same each time; and to examine how far that proportion agrees with what it should be by theory if the bodies were connected by canals of incompressible fluid.

237] To avoid circumlocution I shall frequently in the following pages make use of a term the meaning of which is given in the following definition.

DEF. When in relating any experiment in which two bodies B and b were successively connected to a third body and overcharged, I say that the charge of B was found to be to that of b as P to 1, I mean that the quantity of redundant fluid in B would have been to that in b in the above proportion, provided the quantity of redundant fluid in the third body was exactly the same each time, everything else being exactly the same as in the experiment, that is, the bodies being situated exactly as in the experiment. But when I say simply that the charge of one body is to that of another in any particular proportion, for instance, when I say that the charge of a thin circular plate is to that of a globe of the same diameter as 1 to 1·57, I would be understood to mean that if the circular plate and globe are successively connected to a third body by a thin wire the redundant fluid in the plate would be to that in the globe in that proportion, provided they were placed at a very great distance both from the third body and from any other over- or undercharged matter, and that the quantity of redundant fluid in the third body was exactly the same each time.

238] The method I took in making these experiments was by comparing each of the two bodies I wanted to examine, or B and b as I shall call them, one after another with a third body, which I shall call the trial plate, in this manner. I took two Leyden vials and charged both of them from the same conductor; I then electrified B positively by the inside of one of the vials, and at the same time electrified the trial plate negatively by the coating of the other vial. Having done this I tried whether the redundant fluid in B was more or less than sufficient to saturate the redundant matter in the trial plate, by making a communication between them by a piece of wire; for if the redundant fluid in B was more than sufficient to saturate the redundant matter in the trial plate, they would both be overcharged after the communication was made between them; if, on the other hand, the redundant fluid in B was not sufficient to saturate the redundant matter in the trial plate, they would be undercharged. Having by these means found what size the trial plate must be made so that the redundant matter in it should be just sufficient to saturate the redundant fluid in B, I tried the body b in the same manner, and if I found that it required the trial plate to be of the same size in order that the redundant matter in it should be just sufficient to saturate the redundant fluid in b, I was well assured that if B and b were successively made to communicate with a third body and positively electrified they would each of them contain the same quantity of fluid, supposing the quantity of redundant fluid in the third body to be the same each time; that is, that the charge of B was equal to that of b.

Having thus given a general idea of the method I used, I proceed to describe it more particularly.

239] The trial plates I made use of consisted of two flat tin plates $ABCD$ and *abcd* (Fig. 15), made to slide one upon the other, so that by

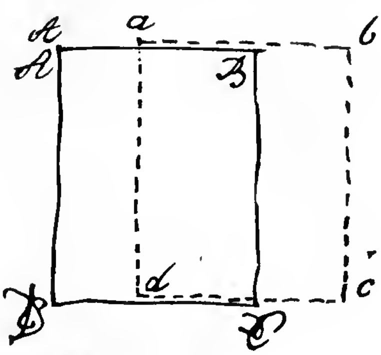


Fig. 15

making the side bc of one plate extend more or less beyond the side BC of the other it formed a plate of a greater or less size, and which consequently contained more or less electricity*.

240] The apparatus used in making these experiments is represented in Fig. 14, where the parallelogram T represents the trial plate and B one of the bodies to be compared together, each supported on non-conductors. $dD\delta$ is the wire for making a communication between them, having a

* [See table for trial plate at Art. 468.]

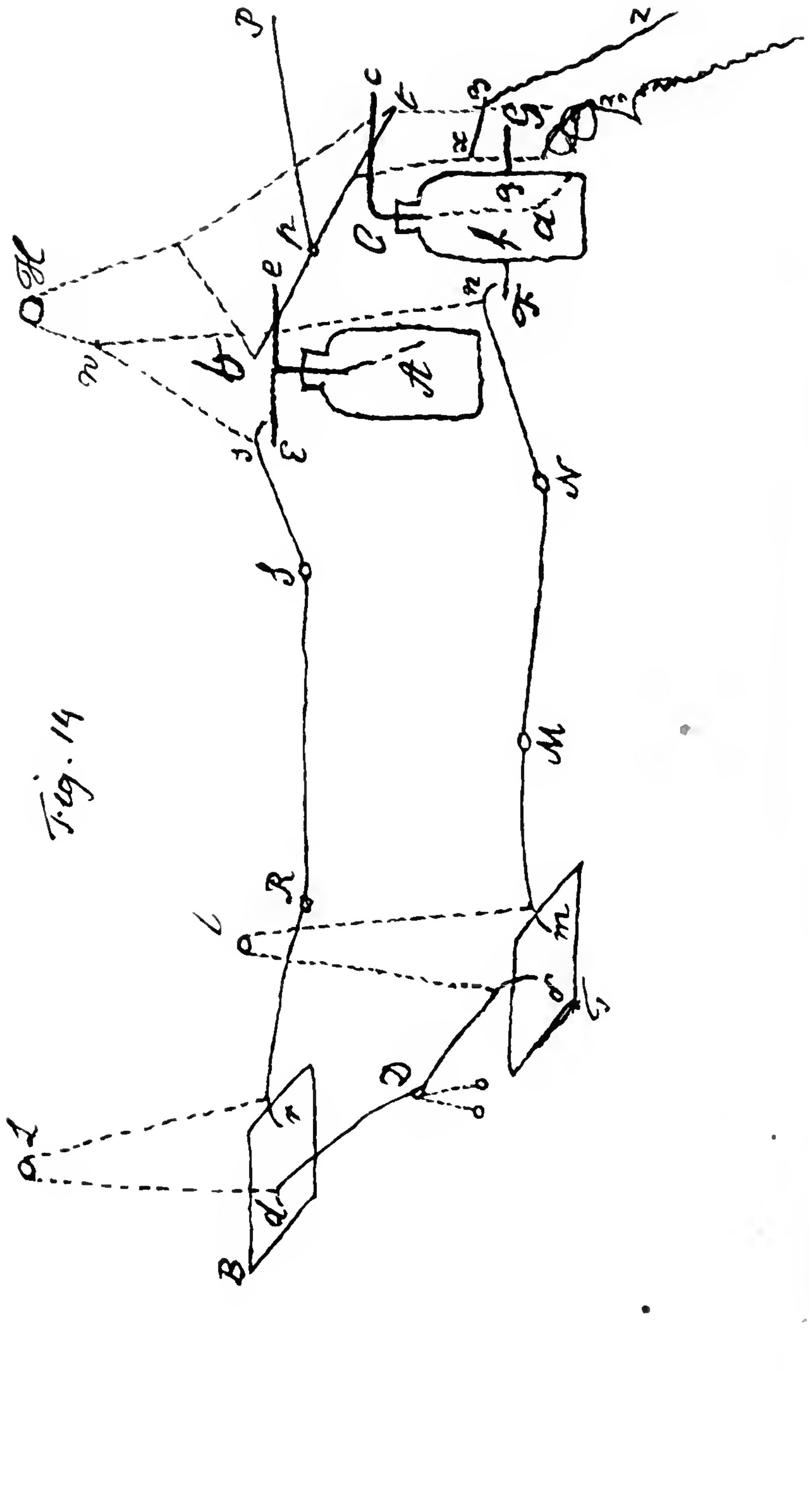


Fig. 14

joint in it at D, where it is supported by a non-conductor, and where are also hung two small pith balls to show whether B and T are over- or undercharged after the communication is made between them. A and a are the two vials; Ee is a wire communicating with the inside coating of A, aCc a wire communicating with the same coating of a; and Ff and Gg are wires fastened to the outside coating of a; $rRSs$ is a wire for making a communication between B and the vial A, having joints in it at R and S, where it is supported by non-conductors, and $mMNn$ is another wire of the same kind for making a communication between T and the vial a*.

241] In order to try the experiment I proceed in this manner: the wires Dd and $D\delta$ are lifted off from the plates B and T so as not to touch them, and consequently so that there is no communication between B and T: the wires Rr and Mm are suffered to rest on B and T, and the wires Ss and Nn are lifted up so as not to touch Ee and Ff. The vials are then charged by means of the wire bt which rests on Ee and Ce, and communicates by the wire Pp with the prime conductor, a communication being made between the outside of the vial A and the ground, and the vial a being made to communicate with the ground by the wire xyz, which rests on Gg, and is suspended from the wire bt by silk strings represented in the figure by dotted lines. When the vials are sufficiently charged, the wire bt is lifted up till xy bears against the bottom of Cc, xy being still suffered to communicate with the ground as before, and the communication between the outside of the vial A and the ground being still preserved. At the same time the wires Ss and Nn are let fall upon Ee and Ff. For the sake of doing this more commodiously I make use of the silk strings represented in the figure by dotted lines and passing over the pulley H. A weight is fastened to the string at w, which is supported while the vials are charging in such manner that the wires Ss and Nn are lifted up so as not to touch Ee and Ff, and the wire bt is suffered to rest on Ee and Cc, and the wire xy on Gg; and when the vials are sufficiently charged the weight is let down, by which means Ss and Nn are suffered to fall down upon Ee and Ff, and the wire bt is lifted up till xy bears against the bottom of Cc.

242] From what has been said it appears that whilst the vials were charging, the outsides of each of them communicated with the ground, and consequently the inside of each vial is overcharged and the outside undercharged. As soon as the vials are charged the communication of each of them with the prime conductor is taken away, and at the same time the communication between the outside of the vial a and the ground is taken away, so that it is entirely insulated, and, immediately after, a communication is made between its inside and the ground, and at the same time the body B is made to communicate with the inside of the

* [See plan, Fig. 17, p. 137.]

vial A, and the trial plate with the outside of the vial a; consequently the body B will be overcharged as it communicates with the overcharged part of the vial A, while the undercharged side communicates with the ground; and the trial plate will be undercharged, as it communicates with the undercharged side of the vial a, while the overcharged side communicates with the ground.

Immediately after this operation is performed the wires Rr and Mm are lifted up, so as to cut off the communications of the bodies B and T with the vials, and, instantly after, the wires Dd and $D\delta$ are let down, so as to make a communication between the body B and the trial plate. For the sake of expedition this operation was performed nearly in the same manner as the former, by means of the silk strings passing over the pullies L and l, and represented in the figure by dotted lines. I also employed an assistant to turn the electrical machine and to manage the silk strings passing over the pulley H, while I stood ready near D to perform the last mentioned operation as soon as the wires Ss and Nn were let down, and also to see whether the pith balls separated or not.

243] From the manner of performing the last mentioned operation it appears that the communication is not made between B and T till after their communication with the vials and all other bodies is cut off; consequently, if the quantity of redundant fluid communicated to B is more than sufficient to saturate the redundant matter in T, they will be overcharged after the communication is made between them, and the pith balls at D will separate positively, but if the redundant fluid in B is not sufficient to saturate the redundant matter in T they will be undercharged, and the pith balls will separate negatively.

244] The balls were made of pith of elder, turned round in a lathe, about one-fifth of an inch in diameter, and were suspended by the finest linen threads that could be procured, about 9 inches long.

245] In making these experiments I did not open the trial plate to such a surface that the pith balls should not separate at all on making the communication between B and T, and assume that for the size which must be given to the trial plate in order that the deficience of fluid in it should be equal to the redundance in B (or for the required surface of the trial plate, as I shall call it for shortness); but I first made the surface of the trial plate such that the deficient fluid therein should exceed the redundant in B, and that the pith balls should separate negatively, just enough for me to be sure they separated: I then diminished the surface of the trial plate till I found, on repeating the experiment, that the pith balls separated positively as much as they before separated negatively: and the mean between these I concluded to be the required surface of the trial plate.

246] This way of making the experiment I found much more accurate than the other, for supposing the required surface of the trial plate to be expressed by the number 16, I found that its surface must be increased to about 20 before I could be certain that the pith balls would separate negatively, and that it must be diminished to about 12 before they would separate positively; whereas I found that increasing its surface from 20 to 21 would make the balls separate sensibly further, and that diminishing its surface from 12 to 11 would have the same effect; so that I could determine the required surface of the trial plate at least four times more exactly by the latter method than by the former.

247] It will be shewn hereafter* that the quantity of deficient fluid in the trial plate is in proportion to the square root of its surface; consequently the redundant fluid in B must exceed, or fall short of, the deficient fluid in the trial plate by about $\frac{1}{8}$th part, in order that the balls should separate, and moreover the increasing or diminishing the deficience of fluid in the trial plate by about $\frac{1}{32}$ part will make a sensible difference in the separation of the balls.

248] It is plain that this way of finding the required surface of the trial plate is not just, unless the vials are charged equally in both trials, namely, that in which the balls separate positively and that in which they separate negatively; I therefore fastened an electrometer to the wire Pp, at a sufficient distance from the vials, consisting of two paper cylinders about three-quarters of an inch in diameter and one inch in height, suspended by linen threads about eight inches long, and in changing the vials took care always to turn the globe† till these cylinders just began to separate.

249] In all the later experiments, however, I made use of a more exact kind of electrometer, consisting of two wheaten straws, Aa and Bb (Fig. 30), eleven inches long, with cork balls A and B at the bottom, each one-third of an inch in diameter, and supported at a and b by fine steel pins bearing on notches in the brass plate C, and turning on these pins as centers. This electrometer was suspended by the piece of brass C from the prime conductor, and a piece of pasteboard, with two black lines drawn upon it, was placed six inches behind the electrometer on a level with the balls, in order to judge of the distance to which the balls separated, the eye being placed before the electrometer at thirty inches distance from them (a guide for the eye being placed for that purpose‡), and the

* [Arts. 284, 479, 682.] † [Of Nairne's electrical machine, see Art. 563.]

‡ It is necessary that the eye should always be placed nearly at the same distance from the electrometer, as it is evident that the nearer the eye is placed the further the balls will appear to separate. But as the distance of the balls from the eye is so much greater than their distance from the pasteboard, a small alteration in the distance of the balls either from the eye or the pasteboard will make no sensible alteration in the distance to which the balls appear to separate.

electrical machine was turned till the balls appeared even with those lines. By these means I could judge of the strength of the electricity to a con-

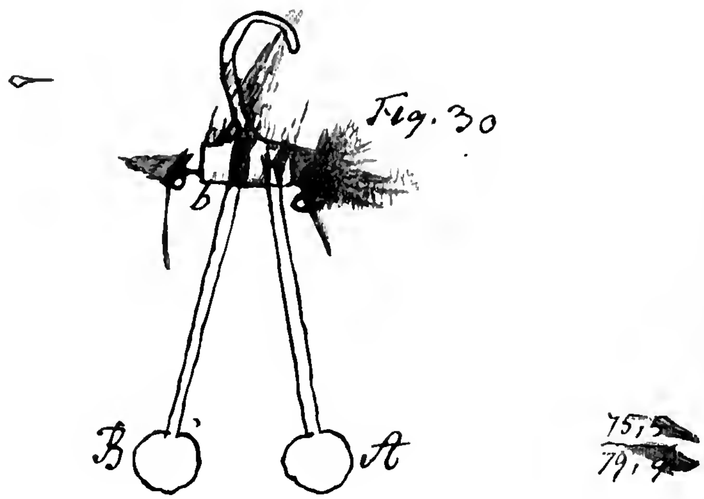


Fig. 30

siderable degree of exactness. In order to make the straws conduct the better they were gilt over.

250] In order to estimate what error may arise from the vials being not equally charged in both trials, let the required surface of the trial plate be called 16; then must the surface which must be given to it in order that the balls may separate negatively be 20, or $16 + 4$, supposing the vials to be charged with the usual degree of strength. Suppose now that in the next trial, in which the balls are to separate positively, the vials are charged stronger than before, in the ratio of x to 1, so that the quantity of redundant fluid in B shall be greater than before, in the ratio of x to 1, and that the deficience in the trial plate should be greater than before in the same ratio, provided its surface remained unaltered; then must the surface which must be given to the trial plate, in order that the balls shall separate positively as much as they did negatively, be $16 - \frac{4}{x}$; for, if this surface is given to it, it is plain that the redundant fluid in B will as much exceed the deficient in the trial plate as it before fell short of it. The mean between these two surfaces is $16 + \frac{4(x-1)}{2x}$, whereas it ought to have been 16, so that the error which will proceed from thence in finding the required surface of the trial plate is $\frac{2(x-1)}{x}$, and, consequently, is less than half of the error which we are liable to in finding it the other way (or that in which we endeavour to find that surface of the trial plate with which the balls do not separate at all), though x is ever so great; for in that way it was before said that we were liable to an error of four. But if x is equal to $\frac{5}{4}$, which is as great an error of strength as I think can well arise in charging the vials, even when the first mentioned electrometer is used, the error in finding the required surface is only $\frac{1}{32}$ of the whole surface, or only $\frac{1}{8}$ part of what might arise the other way.

251] Having thus found what surface must be given to the trial plate, in order that the deficience of fluid in it shall be equal to the redundance in B, I take away the body B and put the other body b, which I want to compare with it, in its room, and if I find on repeating the experiment that the trial plate must be drawn out to the same surface as before, in order that the deficience of fluid in it shall be equal to the redundance in b, or, in other words, if the required surface of the trial plate is the same in trying b as in trying B, I am well assured that if B and b were successively made to communicate with one of the vials, or with any other third body, and were positively electrified, they would each of them contain the same quantity of redundant fluid, supposing the quantity of redundant fluid in the third body to remain the same each time. On the other hand, if I find that the required surface of the trial plate is greater in trying b than in trying B in the ratio of t^2 to T^2, I am well assured that the quantity of redundant fluid in b would exceed that in B in the ratio of t to T, supposing, as was said before, that the deficience of fluid in the trial plate is in proportion to the square root of its surface.

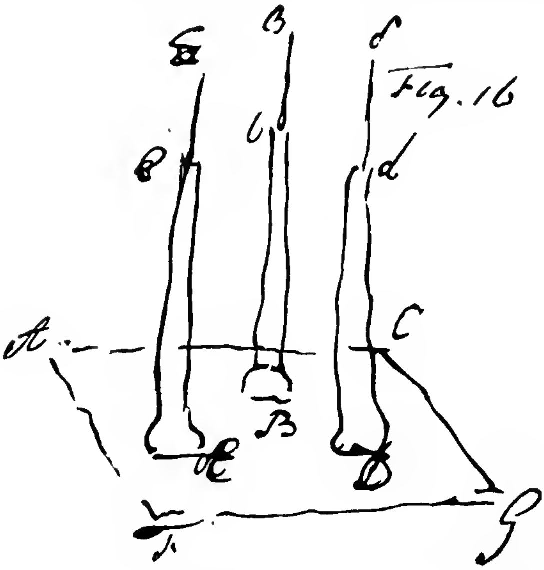
252] If the reader should think that this conclusion requires any proof it may be thus demonstrated:

Suppose that in trying B it was found that the required surface of the trial plate was T^2 and that in trying b it was t^2, and let us first suppose that the vials are charged in exactly the same degree in trying b as in trying B, then is the conclusion evident, for then are B and b successively made to communicate with the vial A, the charge of this vial being exactly the same each time, and the quantity of redundant fluid communicated to b is, actually, to that communicated to B as t to T. But it is plain that the conclusion is equally just, though the vials are charged higher in trying one than in trying the other. For though, in this case, the redundant fluid actually communicated to b will not be to that communicated to B in the ratio of t to T, yet we are sure that it would have been so if the vials had been charged in the same degree each time, for the required surfaces which must be given to the trial plate in trying b must evidently be the same whether the vials are charged to the same degree as they were in trying B, or to a different degree.

253] Though it is of no signification whether the vials are charged to the same degree in trying b as in trying B, yet it is necessary, as I said before, that in trying either B or b the vials should be charged nearly with the same strength when the balls are to separate positively as when they are to separate negatively, as otherwise a small error will arise in finding the required surface of the trial plate.

254] In all the following experiments I took care to proportion the size of the bodies *B* and *b* in such manner that the quantity of redundant fluid in one should not be very different from that in the other, so that, though the deficience of fluid in the trial plate should not be very nearly as the square root of its surface, it would make very little error in the conclusion.

255] The usual distance of the centers of *B* and *J* in these experiments was 83 inches, the distance of *B* from the vial *A* 106 inches, and that of *T* from *a* 86 inches, and the distance of the two vials about 10 inches*. The usual height of the body *B* and the trial plate above the ground was 50 inches; they were commonly supported upon pillars such as are represented in Fig. 16, where *Ee*, *Bb* and *Dd* are three upright pillars of baked



wood about 40 inches long, and $e\epsilon$, $b\beta$, and $d\delta$ are sticks of glass 10 inches long and $\frac{1}{3}$ inch thick let into the wood, and covered with sealing-wax. *ACGF* is a piece of board which the pillars are fastened into. The points *M*, *N*, *R*, and *S* were each supported by a pillar of the same kind, and the point *D* was supported nearly in the same manner. In some experiments, however, the body *B* was suspended by silk strings. The wires $dD\delta$, *rRSs*, and *mMNn* were about $\frac{1}{14}$ inch thick.

256] It is well known that the air of a room is easily rendered over- or undercharged, in particular if a wire such as *rRSs* [Fig. 14] is positively

* [See plan at Art. 265, details at Art. 466, and theory in Note 17.]

electrified, though even in no greater degree than in these experiments, and kept so for a second or two, and its electricity then destroyed, the air near it will be sensibly overcharged, as may be thus shewn. Take a pair of pith balls, like those hung at *D*, and suspend them within a few feet of the wire from some body communicating with the ground. The balls will instantly separate on electrifying the wire on account of the repulsion of the redundant fluid in it, but they will also continue to separate, though in a less degree, after the electricity of the wire is destroyed, which can be owing only to the air being rendered overcharged by it.

257] It may be suspected that this electrification of the air by the wires may affect the separation of the pith balls at *D* and thereby cause an irregularity in the experiments, but it must be considered that the wire *mMNn* is made as much undercharged as *rRSs* is overcharged, and the pith balls are placed about equally distant from both, so that the undercharged air near one wire will nearly balance the effect of the overcharged air near the other. Besides that, if it had any effect upon the separation of the balls, it would have much the same effect in trying *B* as in trying *b*, and therefore could hardly cause any error in the result of the experiment. However, still further to obviate any error from that cause, I had a contrivance by which the electricity of the wires *rRSs* and *mMNn*, as well as that of the vials, was destroyed as soon as the wires *rR* and *mM* were lifted up from *B* and *T*.

258] It is necessary that the outside of the bottle *A* and the wire *yx* should have as perfect a communication with the ground as possible, as otherwise it might happen that the body *B* and the trial plate might not receive their full degree of electrification before the wires *rR* and *mM* were lifted up. I therefore made them to communicate by a piece of wire with the outside wall of the house. This I found to be sufficient, for if I charged a vial, making the outside to communicate with the outside wall, and then made a communication by another wire between the inside of the vial and another portion of the outside wall of the house at several feet distance from the other, I found the vial to be discharged instantly; but if I made the wires to communicate only with the floor of the room instead of the wall of the house, I found it took up some time before the vial was discharged.

It must be observed that in this case, where you want to carry off the electricity very fast by an imperfect conductor, such as the wall, the best way is to apply a pretty broad piece of metal to the wall, so as to touch it in a considerable surface, and to fasten the wire to that, which was the way I last made use of, for if you only apply the wire against the wall, as it will touch the wall only in a few points, the electricity will not escape near so fast.

259] In dry weather the linen threads by which the pith balls are suspended are very imperfect conductors, so that the balls are apt not to separate or close immediately on giving or taking away the electricity. To remedy this inconvenience I moistened the threads with a solution of sea-salt, which I found answered the end perfectly well, for the threads after having been once moistened conveyed the electricity ever after very well, though the air was ever so dry.

260] As the charge of the vials A and a is continually diminishing from the time that the communication between them and the electrical machine is taken away, both by the electricity running along the surface of the vial from the inside to the outside, and by the waste of electricity from the wires $rRSs$ and $mMNn$ and their supports, it is necessary that the operation of electrifying B and T and lifting up the wires rR and mM should be performed as soon as possible, and, above all, it is necessary that the communication should be made between B and T as soon as possible after lifting up the wires rR and mM. This end was obtained very well by the manner, already described, of performing the operation.

261] Before I begin to relate the experiments, it will be proper to say something more about the accuracy that is to be expected in them. I before said that increasing or diminishing the surface of the trial plate by $\frac{1}{16}$ of what I called the required surface, *i.e.*, that surface in which the deficience was equal to the redundance in B, made a sensible alteration in the distance to which the pith balls separated. In reality I found that increasing or diminishing it by only $\frac{1}{24}$ part of the required surface would in general make a sensible alteration, but I could not be certain to nearly so small a quantity, for it would frequently happen that after having determined the surface of the trial plate at which the balls separated to a given degree, that on repeating the experiment a little after, the balls would separate differently from what they did before, and that I was obliged to alter the surface of the trial plate by $\frac{1}{12}$ and sometimes even $\frac{1}{8}$ of the required surface in order to make the balls separate in the same degree as before. Therefore, as increasing the surface of the trial plate by $\frac{1}{12}$ part increases the deficience of fluid therein by $\frac{1}{24}$ part, it appears that if the bodies B and b really contain the same quantity of redundant fluid, it might seem from the experiments as if B contained $\frac{1}{24}$ or even $\frac{1}{16}$ part more or less redundant fluid than b, so that I am liable to make an error of $\frac{1}{24}$ or $\frac{1}{16}$ part in judging of the proportion of the quantity of redundant fluid in two bodies. I imagine, however, that it will not often happen that the error will amount to as much as $\frac{1}{32}$.

262] I do not very well know what this irregularity proceeded from. Part of it might arise from the difference in the strength with which the vials were charged, but I believe that part of it must arise from some other cause which I am not acquainted with. For greater security I always compared each body with the trial plate 6 or 7 times running.

263] It appears from the description of the electrometer fastened to the wire *Pp* that the vials were charged extremely weakly in these experiments (they were indeed charged so weakly that if tried by Lane's electrometer they would not discharge themselves, if the distance of the knobs was more than $\frac{1}{25}$ of an inch*), and it perhaps may be asked why I chose to charge them so weakly, as it is plain that the stronger the vials are charged the less alteration in the size of the trial plate would it have required to produce the same alteration in the separation of the pith balls.

264] My reason was this,—that the electricity seems to escape remarkably faster from any body, both by running into the air and by running along the surface of the non-conductor on which it is supported, when the body is electrified strongly than when it is weak, which made me afraid that if I had charged the vials much stronger the experiment might have been too much disturbed by the diminution of the quantity of redundant fluid in *B* and the deficience in the trial plate between the lifting up of the wires *Rr* and *Mm* and letting fall the wires *Dd* and *Dδ*, and also by the diminution of the charge of the vials between lifting up the wire *bt* and lifting up the wires *Rr* and *Mm*; and indeed it seemed, from some trials I made with a heavier electrometer fastened to *Pp*, as if the experiments were not more exact, if so much so, when the vials were charged stronger, as when they were charged in the usual degree.

I now proceed to relate the experiments I have made.

265] Exp. III. This experiment was made with a view to discover whether the quantity of redundant fluid communicated to the body *B*

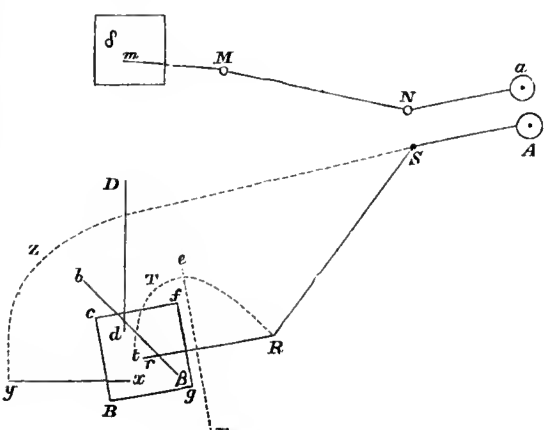


Fig. 17. [Scale $\frac{1}{18}$.]

* [Difference of potentials about 11·8. See Art. 329 and Note 10.]

was different according to the different situations in which it was placed in respect of the vial A, or according to the different shape of the wire $sSRr$ by which it was touched, or according to the different parts in which it was touched by that wire. The body which I used for this purpose was a square tin plate, 12 inches each way, and the different ways in which it was tried are drawn in Fig. 17, which represents a plan of the disposition of the whole apparatus, in which the letters B, d, D, δ, t, m, M, N, a, A, S, R and r represent the same things as in Fig. 14.

266] 1st Way. The tin plate was placed in a vertical plane so as to be represented in the plan by the line $b\beta$, the wires Rr and Dd when let down resting on the edge of the plate as in the figure.

2nd. The tin plate was placed horizontal, as represented by the square $Bcfg$, the plate being placed so that the wire Rr touched it near the middle. N.B. The wire Rr was bent at right angles about $\frac{3}{4}$ of an inch from the end r, so that $\frac{3}{4}$ of an inch was in a vertical situation, and the rest horizontal. Consequently the wire touched the plate only by its extremity.

3rd. The same as the last, except that the wire Rr touched the plate not far from the side fg, and pretty near the middle of that side.

4th. The same as the last, except that a cross wire eE was fastened horizontally across the wire Rr, so as to be parallel to the side fg, and about one inch distance from it.

5th. The plate in the same situation as before, but the wire Rr was bent into an arch, like tTR, only the plane of that arch was vertical. The wire touched the plate near the middle.

6th. The plate in the same situation as before, but the wire Rr was removed into the situation yx, the communication between y and S being by the wire yzS bent into an arch, as in the figure, the plane of which was vertical. The wire yx touched the plate near the middle.

N.B. In all these ways the tin plate was supported on silk lines.

267] The charges of the plate in the different situations were found to be to each other in the following proportions*:

1st Way	11·7,
2nd ,, ...	11·7,
3rd ,, ...	12·0,
4th ,, ...	10·8,
5th ,, ...	11·5,
6th ,, ...	10·8.

* [Art. 470, Dec. 17, 1771. The numbers there found are here multiplied by a constant, so as to make the result by the 3rd way equal to 12.]

The plate was tried in some of these situations another night, when the charges came out in the following proportions*:

2nd Way. . . .	11·9,
3rd ,, ...	12·0,
5th ,, ...	11·8,
6th ,, ...	11·0.

268] It should seem from these experiments that the charge of the tin plate is not exactly the same in all the ways of trying it, as the extremes seem to differ from each other by above $\frac{1}{12}$ part, which is more than could arise from the error of the experiments; but, excepting the 4th and 6th ways, the others seem to differ by less than $\frac{1}{24}$. This I think we may be well assured of, that no sensible error can arise in the following experiments from any small difference in the manner in which the bodies are touched by the wire.

269] Exp. IV. These experiments were made with intent to see whether the charge of a body of a given shape and size was the same whatever materials it consisted of, as it ought to be according to Prop. XVIII†, and also to see how far the charge of a flat plate depended on its thickness‡. The substances used for this purpose were all flat plates about one foot square. The results of the experiments are given in the following Table§:

Names of substances used	Mean side of square	Thickness	Charge	Side increased by $1\frac{1}{3}$ of thickness	Reduced charge
A tin plate	12·00	·02	11·92	12·03	11·89
A hollow plate composed of tin plates soldered together ...	11·03	1·01	12·30	12·38	11·92
Another of the same kind, but thinner	11·62	·37	12·08	12·11	11·97
A piece of pasteboard such as used for the covering of books ...	12·02	·087	11·95	12·14	11·81
A piece of Portland stone ...	12·00	·40	12·44	12·53	11·91
A sandstone known in London by the name of Bremen stone ...	12·04	·42	12·44	12·60	11·84
A slate such as used for the covering of houses	12·00	·16	12·20	12·21	11·99

N.B. The three pieces of stone were all ground flat, and of an uniform thickness.

270] As it would have been difficult to try the following substances by themselves, I coated panes of crown-glass with them on one side and tried them in that manner, which, as glass does not conduct electricity,

* [Art. 468.]

† [Art. 68.]

‡ [Prop. XXI, Art. 73.]

§ [Arts. 293, 471, 480, 481.]

seems as unexceptionable as it would have been to have tried them by themselves, supposing it had been possible to have done so.

Names of substances with which the glass was coated	Mean side of square	Thickness of glass	Thickness of coating	Charge	Reduced charge
Gold leaf	11·98	·056		11·87	11·89
Thin tinfoil	11·96	·058	·00113	11·55	11·59
Several folds of thick tinfoil stuck together with gum-water ...	11·98	·056	·017	11·93	11·95
Gum Arabic laid on in the form of gum-water and suffered to dry ...	12·05	·064		12·17	12·12
The same mixed up with a good deal of salt	11·96	·061		11·95	11·99
Charcoal powder mixed with a little gum-water	12·04			11·95	11·91
Water thickened with a little gum	11·96	·061		11·80	11·84

The last mentioned substance was quite fluid, but had sufficient tenacity to prevent its flowing immediately to the lowest part of the plate. In those substances in which the thickness of the coating is not set down it was not measured, but the thickness was small.

271] All these things were supported on the pillars of baked wood and waxed glass described at [Art. 255]. The panes of glass were laid on these pillars with their coated sides uppermost, so that the wires *Rr* and *Dd* fell on their coated sides. As many of the substances used were but imperfect conductors of electricity, I fastened bits of tinfoil about an inch square on the places on which the wires *Rr* and *Dd* touched the plate in order to make the electric fluid spread more readily over it, and I satisfied myself beforehand that with this precaution they conducted readily enough for my purpose, as I found by discharging a Leyden vial, and making these substances part of the circuit.

272] It appears from these experiments that the charge of a thick plate is greater than that of a thin one of the same base, as might be guessed from the theory*, and it seems to be equal to that of a very thin one whose side exceeds that of the thick one by about $1\frac{1}{3}$ of its thickness. Let us therefore increase the mean side of each of these plates by $1\frac{1}{3}$ of its thickness, where that quantity is worth regarding, and alter the charge found by experiment in the ratio of 12 inches to the side thus increased, which will give us the charge of a plate of the same materials and shape whose increased side is 12 inches, when the charge of each substance will stand as in the last column of the preceding Table. These numbers do not differ from each other by more than what may fairly be supposed owing to the error of the experiment, and therefore I think we may conclude—firstly, that the charge of a body of a given shape and size is the same whatever materials it consists of, and, though the experiment was tried only with square plates, yet I think there can be no doubt but

* [Note 20, p. 409.]

the case will be the same with bodies of any other shape; secondly, that the charge of any thin plate is very nearly the same whatever its thickness may be, provided its thickness is very small in respect of its breadth or smallest diameter; and there can be no doubt also but what this will hold good in thin plates of any shape, though it was tried only with square ones; and thirdly, if the plate is square and its thickness is several times less than its side, though not small enough to be disregarded, its charge is equal to that of a very thin square plate whose side exceeds that of the former by about $1\frac{1}{3}$ of its thickness.

This last circumstance seems far from being repugnant to the theory; but as I do not know how to calculate the charge of such a plate within tolerably near limits, I shall not trouble the reader any further about it.

273] Exp. V. This experiment was made with a view to find what proportion the charges of similar bodies of different sizes bear to each other, and whether it is the same that it ought to be by the theory on a supposition that the electric attraction and repulsion is inversely as the square of the distance, and that the bodies are connected to the jar by which they are electrified by canals of incompressible fluid. It was tried by taking two circular tin plates of 9 inches diameter, and comparing the charge of these two circles together with that of one of twice the diameter. The circles were placed in a vertical situation, and were disposed as in Fig. 18, where the letters *D*, δ, *m*, *T*, *M*, *N*, *a*, *A*, *S* and *R* stand for the

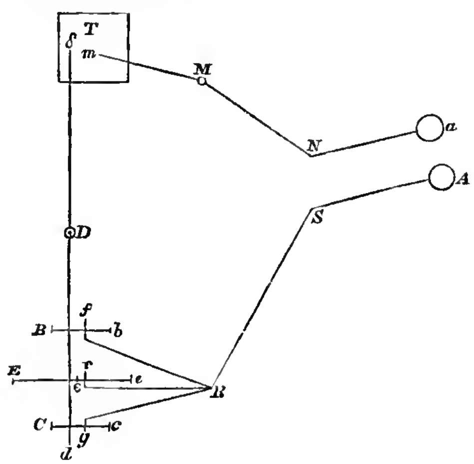


Fig. 18.

same things as in Fig. 17. *Bb* and *Cc* are the two small circles placed parallel to each other, which, as they are in a vertical situation, appear in the plan as straight lines. *Rf* and *Rg* are the wires by which they are electrified, which are bent near *f* and *g* so as to enable them to rest on the edges of the circles. *Dd* is the wire for making a communication between them and the trial plate; *Ee* is the large circle placed half way between the two small ones, and *Rr* the wire by which it is electrified. But it must be observed, that in trying the large circle the two small circles and the wires *Rf* and *Rg* are taken away and the wire *Rr* put in their room; and in like manner, when the small circles are tried, the circle *Ee* and the wire *Rr* are removed.

274] It must be observed that the charge of the two small circles together will not be as much as double the charge of one circle, unless the distance of the two circles from each other is extremely great. In order, therefore, to know better what allowance to make on this account, I tried the experiment with the two small circles placed at three different distances, namely, at 18, 24, and 36 inches from each other, the circles being always placed so that the middle point between them was at the same distance from *D*. Their charges came out in the following proportion*:

The large circle	1·000,
The two small ones at 36 inches distance	·899,
,, ,, ,, 24 ,, ,,	·859,
,, ,, ,, 18 ,, ,,	·811.

275] I repeated the experiment in the same manner, except 1st, that the distances of the vials from the circles and trial plate were different from what they were before, namely, in the foregoing experiment the distance *Ta* from the middle of the trial plate to the vial *a* was 87 inches and ϵA, or the distance from the center of *Ee* to the vial *A*, was 106 inches, whereas in this experiment *Ta* was 98 inches and ϵA 63 inches; the distance $T\epsilon$ was 83 inches in both experiments; and 2ndly, that I placed a frame of wood about 5 feet square under the circles 14 inches from the ground. The reason of these alterations will be shewn by and by†. Their charges came out as follows:

The large circle	1·000,
The two small ones at 36 inches distance	·894,
,, ,, ,, 24 ,, ,,	·840,
,, ,, ,, 18 ,, ,,	·798.

276] Let us now endeavour to find out what proportion the charges ought to bear to each other by the theory on the above-mentioned supposition of their being connected by canals of incompressible fluid, and of the electrical attraction and repulsion being inversely as the square of the distance. This cannot be done exactly without knowing the manner in which the redundant fluid is disposed in the circles, which I am not acquainted with, but if we suppose the fluid to be spread uniformly over

* [Arts. 452, 454, 472–475.].

† [Arts. 277, 339, 474 and Note 17.]

the plates, it will appear, by calculating according to Prop. XXX [Art. 141], that their charges should be in the following proportion:

The large circle						1·000,
The two small ones at			36	inches	distance	·933,
,,	,,	,,	24	,,	,,	·911,
,,	,,	,,	18	,,	,,	·890.

If we suppose that the whole redundant fluid is collected in the circumference, they should be as follows:

The large circle						1·000,
The two small ones at			36	inches	distance	·890,
,,	,,	,,	24	,,	,,	·844,
,,	,,	,,	18	,,	,,	·805;

and if we suppose that $\frac{11}{24}$ of the whole redundant fluid is collected in the circumference, and the remainder, or $\frac{13}{24}$, spread uniformly, they should be as follows:

The large circle						1·000,
The two small ones at			36	inches	distance	·920,
,,	,,	,,	24	,,	,,	·890,
,,	,,	,,	18	,,	,,	·863.

277] I think this latter proportion of the charges much the most likely to agree with the truth*, as it appears from an experiment which will be mentioned hereafter, that the charge of a circular plate bears the same proportion to that of a globe that it would do if the fluid was disposed in that manner. But it must be observed that in these calculations the circles are supposed to be placed at an infinite distance from the vial by which they are electrified, and also from any other over- or undercharged body, whereas in these experiments the circles were at such a distance from the vial that their repulsion on the canal by which they communicated with it was sensibly less than if it was infinite, and moreover the attraction of the undercharged trial plate on the wire *mMNn* has some tendency to increase the quantity of fluid in the circles, and the repulsion of the circles tends to diminish the quantity of fluid in the trial plate, and moreover the floor and walls of the room will be made undercharged near the circles and overcharged near the trial plate, which will also have some tendency to alter the quantity of fluid in the circles and trial plate.

It was with a view to find out what error could proceed from these causes that I tried the experiment in the two different ways above mentioned. It will be shewn, however, in the appendix†, that the first two of these causes cannot produce any sensible alteration in the experiment,

* I would not be understood by this to suppose that the fluid is actually disposed in this manner in a circular plate, but only that the charges will bear the same proportion to each other that they ought to do on this supposition.

† [Art. 188, and Notes 17 and 21.] {Prof. Maxwell has supplied a series of very elegant calculations of capacity, for the cases measured by Cavendish, in Notes 20 to 25.}

and that it is not likely that the last should. This is also confirmed by the near agreement of the results in both ways of trying the experiment, as the difference in the proportion of the charges in these two ways of trying the experiment was not greater than what might well be owing to the error of the experiment.

278] It seems reasonable to conclude, therefore, that the proportion which the charges ought to bear to each other in the theory on the supposition of their being connected by canals of incompressible fluid, and of the electrical attraction and repulsion being inversely as the squares of the distances, must be nearly as in the last Table, and therefore it should seem that the observed charges of the two small plates were rather less in proportion to that of the large one than they ought to have been by theory on the above-mentioned supposition; but the difference is not great, and perhaps not more than what may be owing to our not being able to compute the true proportion with sufficient accuracy, and to the error of the experiment, though I am more inclined to think that the difference is real. This, however, can by no means be looked upon as a sign of any error in the theory, but, on the contrary, I think that the difference being so small is a strong sign that the theory is true. For it cannot be expected that the charges of bodies connected together by wires should bear exactly the same proportion to each other that they should do if they were connected by canals of incompressible fluid; and, indeed, the third experiment shews that they do not, as the charge of the tin plate was found to be a little different according to the situation in which it was placed and the disposition of the wire by which it was touched, which should not be the case if it was connected to the vial by a canal of incompressible fluid.

279] Exp. VI. This experiment was made with the same view as the last, and consisted in comparing the charge of two brass wires together, with that of a single one of twice the length and thickness. The small wires were 3 feet long and $\frac{1}{10}$th of an inch thick; they were placed horizontal and parallel to each other, as represented by the lines *Bb* and *Cc* in Fig. 18, and were tried at three different distances from each other, viz.:—18, 24, and 36 inches. The long wire was 6 feet long and $\frac{1}{5}$th of an inch in thickness, and was placed in the same direction as the small ones, as represented by *Ee*. They were electrified by the same wires and in the same manner as the circles, only they were placed so as to be touched by the wires *fR*, *rR*, and *gR*, very near their extremities *b*, *c*, and *e*. Their charges were as follows:—

The long wire					1·000,
The two short ones at			36 inches distance		·903,
,,	,,	,,	24	,, ,,	·860,
,,	,,	,,	18	,, ,,	·850.

280] The charges of the two small wires at the several distances of 36, 24, and 18 inches ought by theory to have been to that of the long wire in a proportion between that of ·923, ·905, and ·883 to 1 and that of ·893, ·860, and ·835 to 1, supposing them to be connected to the vial by canals of incompressible fluid, but, as it should seem from the next experiment, ought in all probability to approach much nearer to the former proportion than the latter. The observed charges were actually between these two proportions, but approached much nearer to the latter, so that they agreed as nearly with the computation as could be expected *.

281] Exp. VII. Being a comparison of the proportional charges of several bodies of different shapes: the result is as follows:—

A globe 12·1 inch in diameter	1·000
A tin circle 18·5 „ „	·992
A tin plate 15·5 inches square	·957
An oblong tin plate 17·9 inches by 13·4 inches	·965
A brass wire 72 inches long and ·185 thick	·937
A tin cylinder 54·2 inches long and ·73 in diameter	·951
A tin cylinder 35·9 inches long and 2·53 in diameter	·999

The globe was the same that was used in the first experiment. The wire and cylinders were placed in the same manner as the large wire in the preceding experiment, and were touched in the same manner †.

282] Remarks on this experiment.

First, the proportion which the charge of the circular plate bears to that of the globe agrees very well with the theory, for by Prop. XXIX [Art. 140] the proportion should be between that of ·76 to 1 and that of 1·53 to 1, and the observed proportion is that of ·992 to 1. We may conclude also from this experiment that the charge of a circular plate is to that of a globe of the same diameter as 12 to $18\frac{1}{2}$, which by the above-mentioned proposition is the proportion which ought to obtain if $\frac{13}{24}$ of the whole quantity of redundant fluid in the plate was spread uniformly [over the surface], and the remainder, or $\frac{11}{24}$, was spread uniformly [round the circumference], that is, if the value of p in that proposition equals $\frac{13}{11}$ ‡.

283] 2ndly. The charge of a square plate is to that of a circle whose diameter equals the side of the square, as 1·153 to 1, or its charge is to that of a circle whose area equals that of the square as 1·02 to 1 §.

284] 3rdly. The charge of the oblong plate is very nearly equal to that of a square of the same area, and consequently as the length of the trial plates used in these experiments never differed from their breadth (whether the trial plate was more or less drawn out) in a greater proportion than those of this oblong plate do, and as the charges of similar bodies

* [Arts. 453, 476, 477, 683, and Note 13.]

† [Arts. 478, 682.]

‡ [Arts. 654, 681, and Note 2.]

§ [Arts. 479, 682, and Note 22.]

of different sizes are as their corresponding diameters, or sides, I think we may safely conclude that the charges of these trial plates were as the sides of a square of the same area, agreeable to what was said in [Art. 247].

285] 4thly. By Prop. XXXI [Art. 150] the charge of a cylinder whose length $= L$ and diameter $= D$ is to that of a globe whose diameter $= L$ in a ratio between that of 1 to $\log_e \frac{2L}{D}$ and that of 2 to $\log_e \frac{4L}{D}$, and therefore the charges of the brass wire, long cylinder and short cylinder, should be to that of the globe, supposing them to be connected with the vial by which they were electrified by canals of incompressible fluid, in a ratio between that of ·894, ·896 and ·887 to 1 and that of 1·619, 1·573 and 1·469 to 1. The observed charges are as ·966, ·980 and 1·028 to 1, which are between the two above-mentioned proportions, but approach much nearer to the former than the latter, as might have been expected; so that the observed charges agree very well with the theory*.

286] 5thly. If we suppose that the redundant fluid is disposed in the same manner in a cylinder, whether the length is very great in respect of the diameter or not, it is reasonable to suppose that the charges of the brass wire, long cylinder and short cylinder, should be to each other in a proportion not much different from that of ·894, ·896 and ·887, or that of ·966, ·968 and ·959. The observed charges do not differ a great deal from that ratio, only the charges of the two cylinders, especially the shorter, are rather greater in proportion to that of the brass wire than they ought [to be], so that according to this supposition the observed charges do not agree exactly with computation. But if we suppose that the redundant fluid is spread less uniformly in a cylinder whose length is not very great in proportion to its diameter than in another, that is, that there is a greater proportion of the redundant fluid lodged near the extremities, which seems by no means an improbable supposition, the observed charges may perhaps agree very well with what they should be by theory, if they were connected by canals of incompressible fluid.

287] With regard to the small disturbing causes mentioned in [Art. 277], as the length of the brass wire bears so great a proportion to its distance from the trial plate and to its distance from the ground, it is possible that its effect in increasing the deficiency of fluid in the trial plate may be sensibly less, and also that the increase of charge, which it receives itself from the ground near it being under-charged, may be sensibly different from what it would be if it had been of a more compact shape, so that perhaps some alterations may have been made in the experiments by these two causes. I should imagine, however, that they could be but small. It must be observed that the first of these two causes tends to make the charge of the wire appear greater than it really was, and consequently

* [Note 12, p. 382.]

to make the observed charges appear to agree nearer with the theory than they really did. Which way the second cause should operate I cannot say.

On the whole it should seem as if the true charge of a cylinder whose length is L and diameter D is to that of a globe whose diameter is L nearly as $\frac{9}{8}$ to natural logarithm $\frac{2L}{D}$, or as ·489 to Tabular log. $\frac{2L}{D}$.

288] Exp. VIII. Let AB, ab and eg (Fig. 19) be three equal thin parallel plates equidistant and very near to each other, and let Cf, the line joining their centers, be perpendicular to their planes, and let all three plates communicate with each other and be positively electrified: it may easily be shewn that according to the theory the quantity of redundant fluid in the middle plate will be many times less than that in either of the outer plates, or than that which it would receive by the same degree of electrification if placed by itself. I therefore took three tin plates, each 12 inches square, and placed them as above described, and electrified them by means of a wire fixed to a Leyden jar, the end of the wire being formed in such manner as to touch all three plates at once. As soon as the electrifying wire was taken away I drew away the outer plates, and at the same time approached a pair of cork balls to the middle plate in the same manner as I did to the globe in the first experiment and observed how much they separated, care being taken to take away the electricity of the outer plates as soon as drawn away. I then removed the outer plates and, by the same means that I used in the first experiment, made the quantity of redundant fluid in the jar less than before in a given ratio, and by means of this jar electrified the middle plate by itself and approached the cork balls as before. In this manner I proceeded till I found how much it was necessary to diminish the quantity of redundant fluid in the jar in order that the corks might separate as much as before, and consequently how much less the quantity of redundant fluid in the middle plate when placed between the two other plates was than that which it would have received by the same degree of electrification if placed by itself*.

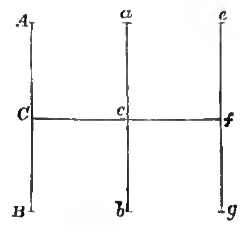


Fig. 19.

The result was that when the distance of the outer plates was $\begin{cases} 1·15 \\ 1·65 \end{cases}$ inches, the quantity of redundant fluid in the middle plate was about $\begin{cases} 8 \\ 7 \end{cases}$ times less than it would be if electrified in the same degree when placed by itself.

289] It is plain that according to the theory the quantity of redundant fluid in each of the outer plates should be the same, and that the quantity

* [Art. 542 and Note 23.]

in the middle plate should be such that the repulsion of AB and ab together on the column cf shall be equal to that of the plate eg thereon in the contrary direction, and the redundant fluid in each of the outer plates is not much more than one-half of that which it would receive by the same degree of electrification if placed by itself. Now it will appear by computing, according to the principles delivered in Prop. XXX [Art. 141], that the quantity of redundant fluid in the middle plate will be so excessively different according to the different manner in which the fluid is disposed in the plates that there is no forming any tolerable guess how much it ought to be; but if we suppose that part of the redundant fluid in each plate is spread uniformly and the rest collected in the circumference, and that in the outer plates the part that is spread uniformly is $\frac{11}{24}$ of the whole, as we supposed in Experiment V, the quantity of redundant fluid in the middle plate when the distance of the outer plates is 1·15 inches will not agree with observation, unless we suppose that not more than the 21st part of it is spread uniformly; but if we suppose that $\frac{4}{7}$ of the redundant fluid in the outer plates is spread uniformly the quantity in the middle plate will agree with observation, if we suppose that about $\frac{1}{9}$ of it is spread uniformly and the rest collected in the circumference.

When the distance of the outer plates is 1·65 inches there is no need of supposing so great a proportion of the fluid in the middle plate to be disposed in the circumference in order to reconcile the theory with observation.

N.B. The more uniformly we suppose the fluid to be spread in the outer plates and the less so in the middle, the greater should be the quantity in the middle plate.

The above computations were made on the supposition that the plates were circles of 14 inches diameter, that is, nearly of the same area that they actually were of.

290] It will appear by just the same method of reasoning that was used in the remarks on the 22nd Proposition [Art. 74], that a vastly greater proportion of the redundant fluid in the middle plate will be collected near its circumference than would be if the outer plates were taken away, and perhaps this circumstance may make the fluid in the outer plates be spread more uniformly than it would otherwise be, so that it seems not improbable that the fluid in the plates may be disposed in such manner as to make the experiment agree with the theory.

The circumstance of its being necessary to suppose a greater proportion of fluid in the middle plate to be lodged in the circumference when the plates are at the smaller distance from each other than when they are at the greater agrees very well with the theory, for it is plain that the nearer the outer plates are to each other the greater proportion of the fluid in the middle plate should be lodged in the circumference.

On the whole I see no reason to think that the experiment disagrees with the theory, though the middle plate was certainly more overcharged than I should have expected.

General Conclusion.

291] The 1st experiment shews that when a globe is electrified the whole redundant fluid therein is lodged in or near its surface, and that the interior parts are intirely, or at least extremely nearly, saturated, and consequently that the electric attraction and repulsion is inversely as the square of the distance, or to speak more properly, that the theory will not agree with experiment on the supposition that it varies according to any other law.

292] The 2nd experiment shews that this circumstance of the whole redundant fluid being lodged in or near the surface obtains also in other shaped bodies, as well as in the globe, conformably to the supposition made in the remarks at the end of Prop. IX [Art. 41]. These two experiments, at the same time that they determine the law of electric attraction and repulsion, serve in some measure to confirm the truth of the theory, as it is a circumstance which, if it had not been for the theory, one would by no means have expected.

293] From the 4th experiment it appears, first, that the charge of different bodies of the same shape and size, all ready conductors of electricity, is the same, whatever kind of matter they are composed of; and secondly, that the charge of thin plates is very nearly the same whatever thickness they may be of, provided it is very small in respect of their breadth or smallest diameter; but if their thickness bears any considerable proportion to their breadth, then their charge is considerably greater than if their thickness were very small. These two circumstances are perfectly conformable to the theory, and are a great confirmation of the truth of it.

294] The remaining experiments contain an examination whether the charges of several different sized and different shaped bodies bear the same proportion to each other, which they ought to do according to the attempts made in different parts of these papers to compute their charges by theory, supposing, as we have shewn to be the case, that the electric attraction and repulsion is inversely as the square of the distance: with regard to this it must be observed that, as in computing their charges I was obliged to make use of a supposition, which certainly does not take place in nature, it would be no sign of any error in the theory if their actual charges differed very much from their computed ones; but, on the other hand, if the observed charges agree very nearly with the computed ones, it not only shews that the actual charges of different bodies bear nearly the same proportion to each other that they would do if they were connected by canals of incompressible fluid, but is also a strong confirmation of the truth of the theory. Now this appears to be the case, for, first the charge of a tin plate was found to be nearly, though not quite, the same in whatever part it was touched by the electrifying wire, or in whatever direction it was placed in respect of the jar by which it was electrified. Secondly, the charge of a single plate or wire was found to bear nearly, though, in the first case, I believe, not quite the same proportion to two similar plates or wires of half the diameter or length which it ought to do according to computation. Thirdly, the proportion which the charges of a thin circular plate and of three cylindrical bodies of different lengths and diameters bear to that of a globe agree with computation; but it must be observed that, as the proportion of the charges of the bodies to that of the globe is determined by the theory within only very wide limits, their agreement cannot be looked upon as so great a confirmation of the theory as it would otherwise be, yet as their shapes are so very different I think that their agreement, even within those limits, may be considered as a considerable confirmation of it.

PART *.—[{III.} EXPERIMENTS ON COATED {DIELECTRIC} PLATES.]

[Hitherto unpublished: see Table of Contents at the beginning of this volume.]

295] This part consists chiefly of experiments made to determine the charges of plates of glass and other electric substances coated in the manner of Leyden vials. The method I used in doing this was nearly of the same nature as that by which I determined the charges of the other sort of bodies in the preceding part, but the apparatus was more compact and

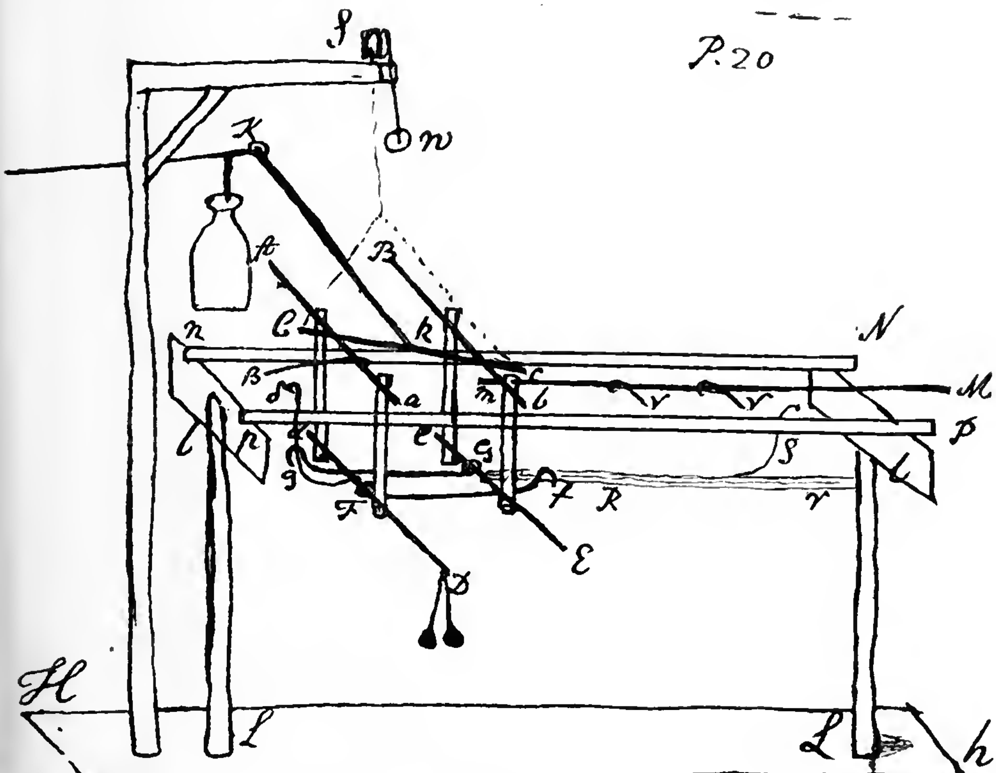


Fig. 20.

portable and is represented in Fig. 20, where *Hh* is a horizontal board lying on the ground, *Ll* and *Ll* are two upright pillars supporting the two horizontal bars *Nn* and *Pp*, both at the same height above the ground, and parallel to each other.

* [Not numbered by Cavendish.]

To these two bars are fastened four upright sticks of glass covered with sealing wax; they are represented in the figure and shaded black, but are not distinguished by letters to avoid confusion. To these sticks of glass are fastened four horizontal pieces of wire *Aa*, *Bb*, *Dd*, and *Ee*, and to *Bb* is fastened another wire *mM* supported at the further end by a stick of waxed glass.

Rr is a wooden bar reaching from the wire *Ee* to the pillar *Ll*, and along the upper edge of this bar runs a wire, one end of which is wound round the wire *Ee* and the other reaches to the ground and serves to make a communication between *Ee* and the ground. *Cc* and *Kk* are two wires fastened firmly together at *k* serving to electrify the plate. They are moveable upon *K* as a center where they communicate with the inside coating of one or more large glass jars, and the same electrometer that was used in the former experiments is fastened to the prime conductor by which the jars are electrified, in order that they may be charged to the same degree each time.

To the ends *C* and *c* of the wire *Cc* is fastened a silk string, as represented in the figure, passing over the pulley *S*, with a counterpoise *w* at the other end which serves to lift *Cc* from off the wires *Aa* and *Bb*, or to let it down upon them at pleasure. *Gg* is a wire the end *G* of which is bent into a ring, through which passes the wire *Ee*, so that *Gg* turns upon *Ee* as a center. *Ff* is a wire turning in the same manner on *Dd*. The ends *g* and *f* of these wires are fastened by silk strings to *C* and *c* as represented in the figure, in such manner that when *Cc* rests on the wires *Aa* and *Bb*, *Gg* and *Ff* rest on *Dd* and *Ee*, but on lifting up *Cc*, *Gg* and *Ff* are also lifted off from *Dd* and *Ee*.

The counterpoise *w* is so heavy as to overcome the weight of *Cc*, and to lift it up till the wires *Gg* and *Ff* bear against *Aa* and *Bb*, which prevents *Cc* from rising any higher.

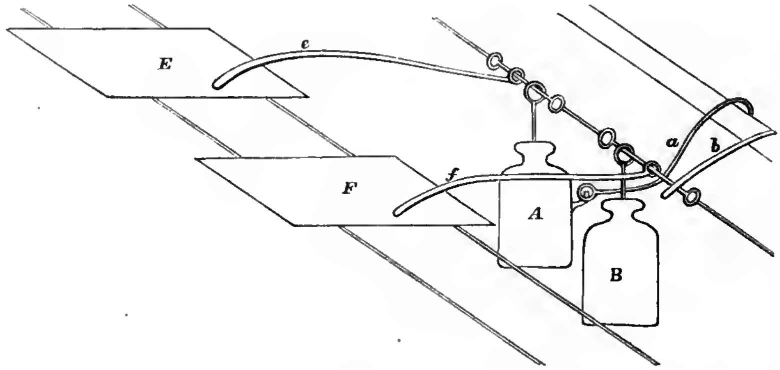


Fig. 20 *a*.

[Note. This Figure was found among the MS. It is not numbered, nor does any part of the MS. seem to refer to it, but it is inserted here to shew some of the details of a piece of apparatus similar to that described in the text.]

296] In making the experiment one of the plates whose charges we want to compare together, or the plate *B* as we will call it, is laid on the bars *Nn* and *Pp*, between the sticks of glass and end *N*, the upper coating thereof being made to communicate with *Bb* and *Mm* by a wire *V* resting on *Mm*, and the lower coating is made to communicate with the ground by a springing wire *S* fastened to *Rr*, and by its elasticity bearing against the lower coating of the plate.

Another coated plate is laid on the same bars between the sticks of glass and *n* by way of trial plate, the upper coating of which communicates with *Aa* by the wire β, and the lower coating communicates with *Dd* by the springing wire δ. A pair of pith balls also, such as were used in the former experiments, were suspended from *D* as represented in the figure.

In trying the experiments, the jars, and consequently the wire *Cc*, are charged, the wire *Cc* being all that time lifted up as high as it will go by means of the counterpoise. When the jars are charged to the proper degree as shewn by the electrometer, the wire *Cc* is let down on the wires *Aa* and *Bb* by lifting up the counterpoise. This instantly charges both the coated plates, for when *Cc* rests on *Aa* and *Bb*, and consequently *Ff* and *Gg* rest on *Ee* and *Dd*, the lower coatings of both plates communicate with the ground, and their upper coatings with *Cc*.

Immediately after this the counterpoise is let go, by which means *Cc* is lifted up, and *Gg* and *Ff* along with it, till the two last mentioned wires bear against *Aa* and *Bb*, so that immediately after the coated plates are charged, the communication between them and the wire *Cc*, by which they were electrified, is taken away, and at the same time the communication between the lower coating of the trial plate and the ground is taken away, and immediately after that a communication is made between the upper coating of the plate *B* and the lower coating of the trial plate, and also a communication is made between the upper surface of the trial plate and the ground, so that the upper coating of the trial plate and the lower coating of the plate *B* both communicate with the ground, and the upper coating of *B* and the lower coating of the trial plate communicate with each other and the wire *Dd*.

Consequently, if the quantity of redundant fluid communicated to the wires *Bb* and *Mm* and the upper side of the plate *B* together is equal to the deficient fluid on the under side of the trial plate, they and the wire *Dd* will be neither over nor undercharged after the operation is completed; but if the redundant fluid in them exceeds the deficient fluid on the lower side of the trial plate, *Dd* will be overcharged, and the pith balls will separate positively. On the other hand, if it is less than the deficient fluid, the pith balls will separate negatively.

297] The trial plate consisted of a flat plate of glass, or other electric substance, the lower surface of which was coated all over with tinfoil, but on the upper side there was only a small coating of tinfoil. I had also flat plates of brass of different sizes which I could lay on the upper surface, and slip backwards and forwards, and thereby increase or diminish the size of the upper coating at pleasure, for the area of the upper coating is equal to the area of the plate of brass added to that of so much of the tinfoil as is left uncovered by the brass*.

By this means I could increase or diminish the quantity of deficient fluid on the lower side of the trial plate at pleasure, for I could alter the size of the upper coating at pleasure, and the quantity of deficient fluid on the under side of the plate is not much greater than it would be if the lower coating was no greater than the upper, and consequently depends on the size of that upper coating.

As it is necessary that the trial plate should be insulated, it was not laid immediately on the bars *Nn* and *Pp*, but was supported by sticks of waxed glass fastened to those bars.

Having by these means found what size it was necessary to give to the upper coating of the trial plate in order that the pith balls should separate positively just sensibly, and what size it was necessary to give to it that they might separate as much negatively, I removed the plate *B* and placed the plate or plates which I intended to compare with it (or the plate *b* as I shall call it) in its room and repeated the experiment in just the same manner as before. Then, if I found that the size which it was necessary to give to the upper coating of the trial plate in order to exhibit the same phenomena was the same as before, I concluded that the charge of the plate *b* was the same as that of *B*. If, on the other hand, I found that it was necessary to make the area of the upper coating of the trial plate greater or less than before in any ratio, I concluded that the charge of *b* was greater or less than that of *B* in the same ratio, for the quantity of deficient fluid on the lower side of the trial plate will be pretty nearly in proportion to the area of the upper coating.

N.B. In the following experiments it was always contrived so that the charges of the plates to be compared together should be pretty nearly alike, so that if the quantity of deficient fluid on the lower surface of the trial plate was not exactly in this proportion, it would make very little error in the proportion of the charges.

298] The method above described is that which I made use of in my first experiment, but I afterwards made use of another method a little different from this, and which I found more exact, though rather more complicated, namely, for each set of plates that I wanted to compare

* N.B. In order to estimate how much of the tinfoil was left uncovered, I drew parallel lines upon it at small equal intervals from each other, and took notice which of these lines the edge of the brass plate stood at. [Arts. 442, 488.]

together I prepared two trial plates, which I shall call L and l, not coated as that above described, but in the usual way, namely, with the coatings of the same size on both sides*.

The first of these plates, or L, was of such a size that when used as a trial plate with the plate B or b on the other side, the quantity of deficient fluid in it was rather more than ought to be in order that the pith balls should just separate negatively, and the second plate l was rather greater than it ought to be in order that they should just separate positively.

I also prepared a sliding plate of the same kind as the trial plate used in the former method, but whose charge was many times less than that of the plate B or b. This sliding plate I placed along with the plate B or b on the side N, and on the other side I placed the trial plate L and found what size it was necessary to give to the coating of the sliding plate in order that the balls should just separate negatively. I then removed the plate B and put b in its room, and found what sized coating it was necessary to give to the sliding plate in order that the balls should separate the same as before. Having done this, I removed the trial plate L and put l in its room, and tried each of the plates B and b as before, finding what coating it was necessary to give to the sliding plate that the balls might just separate positively.

Having done this, if I found that it required the coating of the sliding plate to be of the same size in order to exhibit the same phenomena in trying the plate B as in trying b, it is plain that the charges of B and b must be both alike, but if I found that it was necessary to give less surface, one square inch for instance, to the coating of the sliding plate in trying B than in trying b, then it is plain that the charge of B exceeds that of b by a quantity equal to that of the charge of the sliding plate when its surface is one square inch, supposing, as is very nearly the case, that the charge of the sliding plate is in proportion to the surface of its upper coating.

In this way of trying the experiment, it is plain that, in order to determine the proportion which the charges of B and b bear to each other, we must first know what proportion the charge of the sliding plate, when its coating is of a given size, bears to that of B. This I found by finding what sized coating must be given to the sliding plate that its charge should be equal to that of another plate, the proportion of whose charge to that of B I was acquainted with.

It is plain that, if it is necessary to give one inch less surface to the coating of the sliding plate in trying B than in trying b when the trial plate L is made use of, it will be necessary to make the same difference in the surface of the sliding plate when the trial plate l is made use of,

* [Art. 457.]

so that I might have saved the trouble of making two trial plates. However, for the sake of more accuracy, I always chose to make two trial plates and to take the mean of the results obtained by means of each trial plate for the true result.

299] One reason why this method of trying the experiment is more exact than the former, or that by means of a sliding plate only, is that in the former method I was liable to some error from inaccuracy in judging how much of the tinfoil coating of the trial plate was left uncovered by the sliding brass plate, whereas in this method, as the charge of the sliding plate is but small in respect of that of B, it was not necessary to be accurate in estimating its surface. But I believe the principal reason is that an error which will be taken notice of by and by, and which proceeds from the spreading of the electricity on the surface of the glass, is greater in a sliding plate than in one coated in the usual manner.

In general I think it required scarcely so great an increase of the charge of the trial plate to make a sensible alteration in the degree of separation of the pith balls in the following experiments as in the preceding, and therefore it should seem as if these experiments were capable of rather more exactness than the former, but this was not the case, as the different trials were found not to agree together with quite so much exactness in these experiments as the preceding. For this reason, and also because they were attended with less trouble, I repeated the experiments oftener, as I not only compared each plate with the trial plate for more times together as I did in the preceding experiments, but in general I repeated the experiment on several different days.

300] The circumstance which gave me the most trouble in these experiments was the spreading of the electricity on the surface of the glass. To understand this, let *ABab*, Fig. 21, be a flat plate of coated glass, *cd* and *CD* being the two coatings, and let *CD* be positively electrified, and let *cd* communicate with the ground.

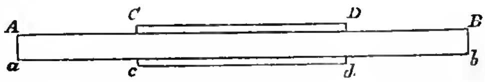


Fig. 21.

It is plain that the electric fluid will flow gradually from *CD* and spread itself all round on the surface of the glass, and nearly the same quantity of fluid will flow from the opposite side of the glass into *cd*, so that those parts of the glass which are not coated gradually become charged, those parts becoming so soonest which are nearest the edge of the glass.

On discharging the plate the uncoated part of the glass gradually discharges itself, as on the side *AB* the fluid will flow gradually from the uncoated part of the glass into CD, and on the opposite side it will flow into the uncoated part of the glass from cd.

301] There is a great deal of difference in this respect between different kinds of glass, as on some kinds it spreads many times faster than on others. The glass on which it spreads the fastest of any I have tried is a thin kind of plate-glass, of a greenish colour, much like that of crown-glass, and which I have been told is brought from Nuremberg*. On the English plate-glass it does not spread near so fast, but there is a great deal of difference in that respect between different pieces. On the crown-glass it spreads not so fast as on the Nuremberg, but I think faster than on the generality of English plate-glass. On white glass I think it spreads as slowly as any.

302] The way in which I compared the velocity with which it spread on different plates was as follows†. I took away the wire Ff (Fig. 20) and placed the plate which I wanted to try where the plates L or l used to be placed, the lower coating communicating as usual with Dd by the wire δ, but the wire β being drawn up by a silk string so as not to touch the upper coating. The wire Cc is suffered to rest on Aa and the jars electrified. When they are sufficiently charged β is let down on the upper coating, which instantly charges the plate to be tried, and immediately the wire Gg is lifted up from Dd, but not high enough to touch Aa. Consequently, immediately after the plate is charged, the communication between Dd and the ground is taken away, and consequently as fast as any fluid flows from the uncoated part of the under surface of the glass to the lower coating, some fluid will flow into Dd and overcharge it, and consequently make the pith balls separate.

303] In order to prevent, if possible, the ill effects proceeding from this spreading of the electricity, I took some coated plates of glass, and covered all the uncoated part with cement to the thickness of $\frac{1}{4}$ or $\frac{1}{2}$ an inch, as in Fig. 22, which represents a section passing through the middle of the

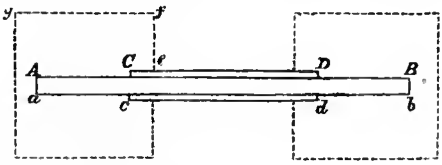


Fig. 22.

plate perpendicular to its plane, and in which the glass plate and coatings are represented by the same letters as before, and the dotted lines represent the cement‡. Thinking that it would be impossible for the electricity to spread between the cement and the glass, in which case

* [Art. 497.]
† [See Arts. 485, 486, 487. Also Arts. 494 to 499.]
‡ [Art. 484.]

this method must have been perfectly effectual, as it would be necessary for the electricity to spread itself not only on the perpendicular surface *ef*, but also to some distance on the horizontal surface *fg*, before the quantity of redundant fluid lodged on the surface of the cement could bear any sensible proportion to that in the coating *CD*.

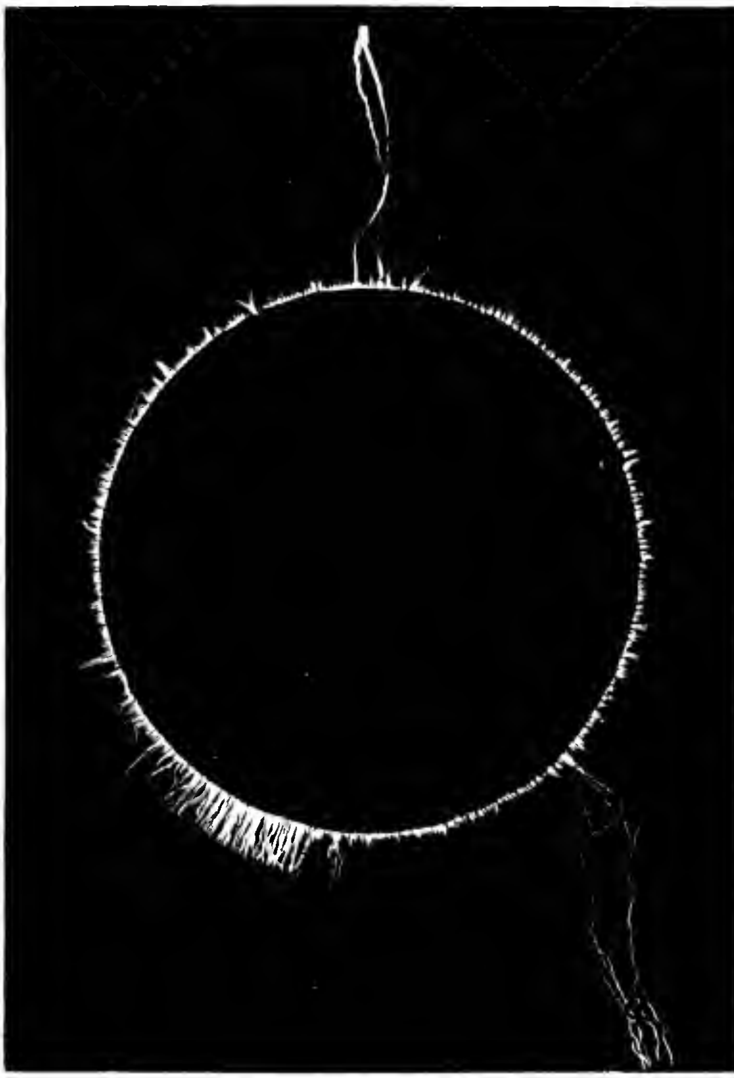
304] The result was that in dry weather the electricity seemed to spread as fast on those plates which were covered with cement as on the others, but in damp weather not so fast, the difference between dry and damp weather being less in those plates which were covered with cement than the others; and besides that there seemed as much difference between the swiftness with which it spread on the surface of the Nuremberg and English plates after they were covered with cement as before, which shews plainly that the electricity spread between the cement and the glass, and not on the surface or through the substance of the cement. It could not be owing, I think, to its passing through the substance of the glass, for if it was, there would hardly be much difference in the uncoated plates between damp and dry weather, whereas, in reality, there was a very great one.

I also tried what effect varnishing the glass plates would have, but I did not find that it did better, if as well, as covering them with cement.

305] As there seemed, therefore, to be very little advantage in covering the plates with cement or varnishing them, and as it was attended with a good deal of trouble, I did not make use of those methods, but trusted only to letting the wires down and up pretty quick, so as to allow very little time for the electricity to spread on the surface of the plates, and this I have reason to think was sufficiently effectual, as I never found much difference in the divergence of the pith balls, whether the wires were let down and up almost as quick as I could, or whether they were suffered to rest a second or two at bottom.

306] As the wire *Cc* is suffered to rest so short a time on *Aa* and *Bb*, it is plain that the lower coatings of the trial plate and plate to be tried must have a very free communication with the ground and the outside coating of the jars, or else there would not be time for them to receive their full charge. I accordingly took care that the wires which made the communication should be clean and should touch each other in as broad a surface as I could conveniently. As for the method I took to have a ready communication with the ground, it is described in [Art. 258].

307] Besides this gradual spreading of the electricity on the surface of the glass, there is another sort which is of much worse consequence, as I know no method of guarding against it, namely, the electricity always spreads instantaneously on the surface of the glass to a small distance from the edge of the coating, on the same principle as it flies through the air in the form of a spark. This is visible in a dark room, as one may see a faint light on the surface of the glass all round the edges of the coating, especially if the glass is thin, for if it is thick it is not so visible*.



[From a photograph taken in the Cavendish Laboratory of a plate of glass with a circular tinfoil coating on one side, a larger coating being applied to the other side of the glass. The electrification of the coatings was produced by an induction coil.]

308] There is another circumstance which shews this instantaneous spreading of the electricity, namely, after having charged and discharged a coated plate of glass a great many times together without cleaning it,

* [See Art. 532, Feb. 1, 1773.]

I have frequently seen a narrow fringed ring of dirt on the glass all round the coating, the space between the ring and the coating being clean, and in general about $\frac{1}{10}$ inch broad*. This must in all probability have proceeded from some dirt being driven off from the tinfoil by the explosions, and deposited on the glass about the extremity of that space over which the electricity spreads instantaneously, and therefore seems to show that the distance to which the electricity spreads instantaneously is not very different from $\frac{1}{10}$ of an inch.

309] From some experiments which will be mentioned by and by†, I am inclined to think that the distance to which the electricity spreads instantaneously is about $\frac{7}{100}$ of an inch when the thickness of the glass is about $\frac{1}{5}$ of an inch and about $\frac{9}{100}$ of an inch when its thickness is about $\frac{1}{15}$ of an inch; or more properly the quantity of redundant fluid which spreads itself on the surface of the glass is the same that it would be if the distance to which it spread was so much, and that the glass in all parts of that space was as much charged as it is in the coated part.

310] If I charged and discharged a coated plate several times running, in the dark, with intervals of not many seconds between each time, I commonly observed that the flash of light round the edges of the coating was stronger the first or second time than the succeeding ones, which seems to shew that the electricity spreads further the first or second time than the succeeding ones. Accordingly I frequently found in trying the following experiments that the pith balls would separate rather differently the first or second time of trying any coated plate than the succeeding ones. Observing that I now speak of the half dozen trials which, as I said in [Art. 299], I commonly took with the same plate immediately after one another.

311] Before I proceed to the experiments it may be proper to remind the reader‡ that if a plate of glass or other non-conducting substance, either flat or concave on one side and convex on the other, provided its thickness is very small in respect of its least radius of curvature, is coated on each side with plates of metal of any shape, of the same size and placed opposite to each other, its charge ought by the theory to be equal to that of a globe whose diameter is equal to the square of the semidiameter of a circle whose area equals that of the coating divided by twice the thickness of the glass, supposing the coated plate and globe to be placed at an infinite distance from any over or undercharged body, and to be connected to the jar by which they are electrified by canals of incompressible fluid; provided also that the electricity does not penetrate to any sensible depth into the substance of the glass, and that the thickness of

* [See Art. 538, Feb. 13, 1773.]

† [See below, Arts. 314 to 323.]

‡ [See Art. 166, Prop. XXXIV, Cor. VI.] {The generalisation, to values of capacity and distribution for condensers with varying thickness of dielectric, had to await Green's *Essay* of 1828, where it follows immediately from his use of the potential function. Cf. end of Note 3, p. 366.}

the glass bears so small a proportion to the size of the coating that the electricity may be considered as spread uniformly thereon.

312] It was before said that the electricity spreads instantaneously to a certain distance on the surface of the glass, so that the surface of the glass charged with electricity is in reality somewhat greater than the area of the coating. Therefore, if the plate is flat, let the area of the coating be increased by a quantity which bears the same proportion to the real coating as the quantity of redundant fluid spread on the surface of the glass beyond the extent of the coating does to that spread on the coated part of the glass. That is, let the area of the coating be so much increased as to allow for the instantaneous spreading of the electricity, and let a circle be taken whose area equals that of the coating thus increased. I call the square of the semidiameter of this circle, divided by twice the thickness of the glass expressed in inches, the computed charge of the plate, because, according to the above-mentioned suppositions, its charge ought to be equal to that of a globe whose diameter equals that number of inches.

313] In like manner, in what may more properly be called a Leyden vial, that is, where the glass is not flat, but convex or concave, let a circle be taken whose area is a mean between that of the inside and outside coatings, allowance being made for the spreading of the electricity. I call the square of the semidiameter of this circle, divided by twice the thickness of the glass, the computed charge of the vial. In like manner, if the real charge of any plate is found to be equal to that of a globe of x inches in diameter, I shall call its real charge x.

I now proceed to the experiments.

314] I procured ten square pieces of plate-glass all ground out of the same piece of glass, three of them 8 inches each way and about $\frac{21}{100}$ inch thick; three more of about the same thickness 4 inches each way, the rest were as near to $\frac{1}{3}$ of that thickness as the workman could grind them, one being 8 inches long and broad, and the other 4 inches. They were not exactly of the same thickness in all parts of the same piece, but the difference was not very great, being no where greater than $\frac{2}{9}$ of the whole. The mean thickness was found both by actually measuring their thickness in different parts by a very exact instrument and finding the mean, and also by computing it from their weight and specific gravity and the length and breadth of the piece*. The mean thickness, as found by these two different ways, did not differ in any of them by more than 2 thousandths of an inch.

315] All these plates were coated on each side with circular pieces of tinfoil, the opposite coatings being of the same size and placed exactly opposite to each other. The mean thickness of the plates, which for more

* A cubic inch of water was supposed in this calculation to weigh 253$\frac{1}{3}$ grains Troy. [See Arts. 592, 593.]

convenience I have distinguished by letters of the alphabet, together with the diameters of the coatings, and their computed charge, supposing the electricity not to spread on the surface of the glass, are set down in the following table*.

Plate	Mean thickness	Diameter of coating	Computed charge
A	·2112	6·57	25·5
B	·2132	6·6	25·5
C	·2065	6·5	25·6
D	·2057	2·155	2·82
E	·2065	2·16	2·82
F	·2115	2·175	2·80
H	·07556	6·8	76·5
K	·07712	2·265	8·31
L	·08205	2·335	8·31
M	·07187	2·195	8·38

316] The sizes of the coatings were so adjusted that the computed charges of *D*, *E*, and *F* are all very nearly alike. Those of *K*, *L* and *M* were intended to be three times as great as those of the former, and consequently the diameters of their coatings nearly the same. The computed charges of *A*, *B* and *C* were intended to be three times as great as those of *K*, *L* and *M*, and consequently the diameter of their coatings about three times as great, and the computed charge of *H* was intended to be three times as great as that of *A*. By some mistake, however, the coatings of *K*, *L* and *M* were made rather too small, but the error is very trifling.

317] My first trials with these plates were to examine whether the charge of the three plates *D*, *E*, and *F* together was sensibly less when they were placed close together than when they were placed at 6 inches distance from each other, that is at as great a distance as my machine would allow of. I could not perceive any difference. This is conformable to the theory, as is shewn in [Art. 185]. I chose to make the experiment with these three plates, as the difference should be more sensible with them than with any of the others.

318] Secondly. I compared together each of the plates *D*, *E* and *F*. I could not perceive any sensible difference in their charges†.

Thirdly. The charge of the plate *K* was found to exceed that of the three plates *D*, *E* and *F* together in the proportion of 1·016 to 1. The charge of *L* was not sensibly different from that of *K*, and that of *M* very little different.

Fourth. The charge of each of the plates *A*, *B* and *C* was to that of the three plates *L*, *K* and *M* together, as 0·905 to 1.

* [Art. 482.]

† [See Art. 489, Feb. 4, 1772.]

Fifth. The charge of *H* was equal to that of the three plates *A*, *B* and *C* together.

Therefore the charges of *D*, *K*, *A* and *H* were to each other as

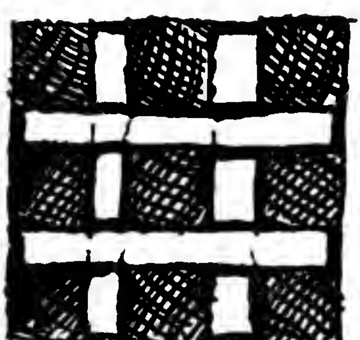
1, 3·05, 8·28 and 24·9*.

319] It appears, therefore, that the proportion which the charge of *K* bears to that of *D*, and which *H* bears to that of *A*, is very nearly the same as that of their computed charges, but the proportion which the charge of *A* bears to that of *K* is near $\frac{1}{10}$ part less than it ought to be.

This in all probability proceeds from the effect of the instantaneous spreading of the electricity bearing a greater proportion to the whole in the plate *K* than it does in *A*, the diameter of whose coating is near three times as great.

320] In order to form some judgment, if possible, how great the effect of this instantaneous spreading of the electricity was, I took off the coatings from the plates *A* and *B*†, and put on others of just the same area in the form of a rectangular parallelogram (that of *A* was 6·414 long and 5·310 broad, and that of *C* 6·398 long and 5·201 broad), and compared their charges with that of the plate *B*, whose charge, as was before said, was just the same as those of these two plates before their coatings were altered.

321] I then took off these coatings‡, and on *A* I put a square coating 6·388 each way with slits cut in it, as in Fig. 23, each $\frac{4}{10}$ broad, so as to divide it into 9 smaller squares, each 1·863 inches each way. The narrow communications marked in the figure between these squares were each $\frac{1}{10}$ of an inch broad.



$\frac{8}{64}$ $\frac{1}{8}$

Fig. 23

6388
18
3)5,588
1,863

On *C* I put an oblong coating 6·377 long and 6·343 broad, with four parallel slits cut in it, as in Fig. 24, each $\frac{4}{10}$ broad, the narrow space left between these slits and the outside being $\frac{1}{10}$ broad. Having done this, I compared their charges with that of the plate *B* as before.

* [See also Arts. 656 to 658.]

† [*C*, see Art. 536.]

‡ [Art. 537.]

It must be observed that the area of these slit coatings was somewhat less than that of the circular or oblong ones, but their whole circumference, including the circumference of the slits, is more than three times as great as that of the circular or oblong ones, so that the surface of glass charged

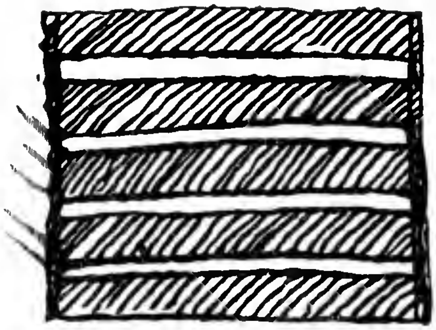


Fig. 24

by means of the instantaneous spreading of the electricity was more than three times as great in these coatings as the former, and consequently the quantity of that surface may be determined thereby, supposing that, if it was not for the spreading of the electricity on the surface, the charge of a coated plate would be the same whatever shape its coating is of, provided the area of the coated surface is given.

322. In order to find whether the electricity spread to the same distance upon thin glass as thick, I also took off the coatings from the plate *H*, and in its room put on first a square coating 6·03 inches each way, and then an oblong one 6·708 long and 6·514 broad, with four slits in it, as in Fig. 24, each $\frac{4}{10}$ broad, and ascertained the proportion which its charge with each of these coatings bore to that with the circular coating by comparing it with another plate, the proportion of whose charge to that of the circular coating I had before ascertained*.

323] It appeared from these experiments that if we suppose the electricity to spread instantaneously about ·07 of an inch on the thick glass plates such as *A* and *C*, and about ·09 on the thin ones, not only the charges of *A*, *C* and *H* with the three different coatings, but also the charges of all the plates will agree very well with the theory, as will appear by the following table; whereas, if we suppose that the electricity does not spread sensibly on the surface of the glass, the charge of the plate *H* with the slit coating would be greater in proportion to its charge with the circular or oblong coating than it ought to be in the ratio of 7 to 6, and the error in the plates *A* and *C* would not be much less.

* [Arts. 659–663.]

324] Plates with circular coatings.

Plates	Diameter	Increased diameter	Thickness	Computed charge	Observed charge
D	2·155	2·295	·2057	3·20	3·21
E	2·16	2·3	·2065	3·20	3·21
F	2·175	2·315	·2115	3·17	3·21
K	2·265	2·445	·07712	9·69	9·74
L	2·335	2·515	·08205	9·63	9·74
M	2·195	2·375	·07187	9·81	9·84
A	6·57	6·71	·2112	26·6	26·6
B	6·6	6·74	·2132	26·6	26·6
C	6·5	6·64	·2065	26·7	26·6
H	6·8	6·98	·07556	80·6	79·8

325] The same plates with other coatings.

Plates	Area of coating	Circumference	Area which electricity spreads over	Increased area	Computed charge	Observed charge
A with oblong	34·1	23·4	1·64	35·74	26·9	26·8
A with slits	31·8	73·5	5·15	36·95	27·8	27·8
C with oblong	33·3	23·2	1·62	34·92	26·9	27·0
C with slits	30·4	76·5	5·35	35·75	27·5	27·7
H with oblong	36·4	24·1	2·17	38·57	81·2	80·7
H with slits	33·3	80·1	7·21	40·51	85·3	85·5

By the observed charge in the foregoing table, I mean only the proportion which the observed charges bore to each other, not the real observed charges. [See Art. 671.]

326] From the circumstance of the light mentioned in [Art. 307], it appears plainly that the electricity does actually spread instantaneously to a small distance on the surface, and from the rings of dirt taken notice of in Art. 308 it seems likely that the distance to which it spreads is not very different from what we have here supposed; moreover, if the distance to which the electricity spreads is such as we have supposed, the charges of all these plates bear very exactly the same proportion to each other that they ought to do by theory, whereas if the distance to which the electricity spreads is different from that here assigned, and consequently the proportion of the charges of different plates to each other different from that furnished by theory, it seems very strange that their charges should all have happened to agree with computation, notwithstanding that their thickness and the size and shape of their coatings are so very different. I think therefore that we may fairly infer both that the distance here assigned to the spreading of the electricity is right, and that, if it was not for this spreading of the electricity, the charge of any plate of glass would be as the square of the radius of the circle equal in area to the coated surface divided by twice the thickness of the glass, that is, that the actual charges are in proportion to the computed ones.

327] Though it seems likely from these experiments that the electricity spreads further on the surface of thin glass than it does on thick, yet I can not be sure that it does, as the difference observed is not greater than what might proceed from the error of the experiment. However, as there seems nothing improbable in the supposition, I shall suppose in the following pages that it does really do so.

328] When I say that the electricity spreads $\frac{7}{100}$ of an inch on the surface of the glass, I mean that the quantity of electricity thereby spread on the uncoated part of the glass is the same that it would be if it actually spread to that distance, and if all that part of the glass which it spread over was charged in the same degree as the coated part, and consequently that the charge of the plate is the same as if the size of the coating was increased by a ring drawn round it ·07 of an inch broad, and that the electricity was prevented from spreading any further. But I would by no means be understood to mean that no part of the electricity spreads to a greater distance than that, as it seems very likely that it does so, but that the part furthest from the coating is less charged with electricity than that nearest to it.

329] What is said above must be understood of the distance to which the electricity spreads with that degree of strength which I commonly made use of in my experiments, but I also made some trials with the plates *A* and *C* to determine to what distance it would spread with two other degrees of electricity.

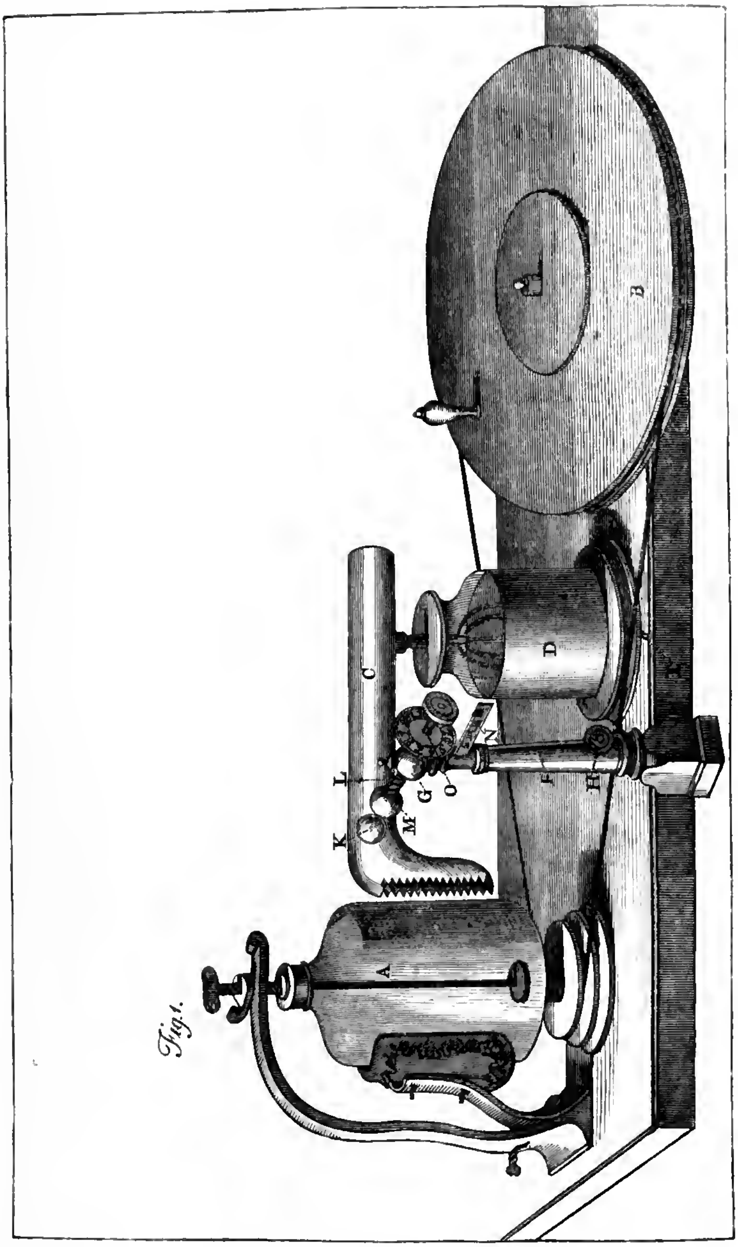
If a jar with Lane's electrometer fixed to it* was charged to the higher degree, it would discharge itself when the knobs of the electrometer were at ·053 inches distance; when it was charged to the lower degree, it discharged itself when they were at about half that distance, or at ·027 of an inch; and when it was charged to the usual degree, it discharged itself, as was before said, at ·04 of an inch, so that the usual degree of electricity was about a mean between these two†.

It seemed as if the electricity spread about $\frac{1}{30}$ of an inch further with the stronger degree of electricity than with the weaker, but the experiment was not accurate enough to determine it with certainty.

330] I made an experiment of the same kind to determine whether the electricity spread to the same distance on crown-glass as on this. It

* [Art. 540, Feb. 16, 1773.]

† [By Macfarlane's experiments (*Trans. R. S. Edin.* vol. XXVIII, Part II, 1878) the electromotive force required to produce sparks between flat disks at those distances would be 14, 11·8, and 9 units respectively.]



[Lane's electrical machine, with discharging electrometer. From his paper in the *Phil. Trans.* 1767, p. 451. For Cavendish's form of discharging electrometer, see Art. 405.]

seemed to spread about $\frac{8}{100}$ of an inch on it, that is, rather less than on the plate *H*, though its thickness was, of the two, rather less. But whether this difference is real, or owing to the error of the experiment, I cannot tell.

331] There seems no reason, from the foregoing experiments, to think that the charge of any of these plates is sensibly greater than it would be if the electricity was disposed uniformly on their coated surfaces, as their charges agree very well together without such a supposition. If we suppose that the charges of any of them are sensibly greater than they would be if the fluid was disposed uniformly, it will be necessary to suppose that there is a still greater difference between the distance to which the electricity spreads on the surface of thin plates and that of thick ones than what we have assigned. But I shall speak more on this subject at the end of Art. [365].

332] But though it appears from the foregoing experiments that the charges of plates of glass of different thicknesses with coatings of different shapes and sizes bear the same proportion to each other that they ought to do by theory, yet their charge is many times greater in proportion to that of a globe than it ought to be on a supposition that the electricity does not penetrate to any sensible depth into the substance of the glass, as will appear by the following experiment.

333] In order to compare the charge of the plate *D* with the globe of $12\frac{1}{10}$ inches used in the former part, I made two plates coated as a Leyden vial, the charge of each of which was about $\frac{1}{2}$ that of *D*, each consisting of two plates of glass cemented together and coated on their outside surfaces with circular pieces of tinfoil about $1\frac{4}{5}$ inch in diameter*.

I then compared the charge of each of these double plates with that of the globe in the same manner that I compared together the charges of different bodies in the former part, the only difference being that, in trying either of these double plates, I made a communication between the lower coating of the plate and the ground, the wires *Mm* and *Dd* (Fig. 14) being contrived so that they were sure to fall on the upper coating†.

By this means the charge of each of these double plates was found to be just equal to that of the globe. The charge of the plate *D* was then compared with that of the two double plates together, and was found to be less than that in the proportion of 263 to 272, and consequently the charge of the plate *D* is to that of the globe as 26·3 to 13·6.

334] Before we go further it will be proper to consider what effect the three circumstances taken notice of in Art. 277 will have in altering

* If they had been made of a single piece of glass, the coatings must have been so small as would have been inconvenient unless the glass had been of a greater thickness than could have been easily procured. [Arts. 446, 451, 649, 653, 654.]

† [Arts. 455, 456, 478.]

the proportion of the charge of the double plate to that of the globe. With regard to the two first, it appears that the charge of the globe and double plate will neither of them be sensibly different from what they would be if they were placed at an infinite distance from the jar by which they are electrified, and moreover, in trying the globe, the repulsion of the redundant fluid in the globe increased the deficience of fluid in the trial plate as much as the attraction of the trial plate increased the quantity of redundant fluid in the globe*, so that it required the same size to be given to the trial plate as it would have done if the globe and trial plate had exerted no attraction or repulsion on each other; and in trying the coated plate, the coated plate could not sensibly increase the deficience in the trial plate, nor could the attraction of the trial plate sensibly increase the redundance in the coated plate, so that neither of these two causes had any tendency to alter the proportion of the charges of the globe and coated plate to each other.

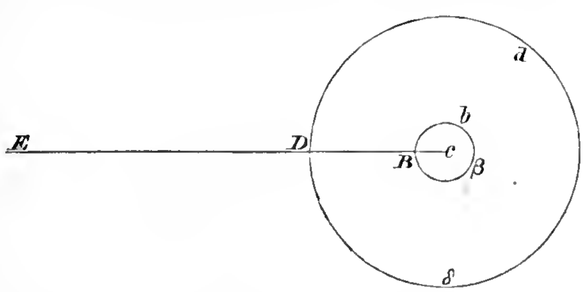
335] But the third cause will have a sensible effect, for in trying the globe the floor and sides of the room near it would be made undercharged, which would increase the charge of the globe, whereas in trying the coated plate the floor would not be made sensibly undercharged, nor, if it was, would it have any sensible effect in increasing the charge of the plate.

So that the charge of the globe bore a sensibly greater proportion to that of the coated plate than it would have done if it had been placed at an infinite distance from any other bodies.

How much the charge of the globe should be increased hereby I can not tell, but I should imagine it should be at least by $\frac{1}{15}$th part, for if the room had been spherical and 16 feet in diameter (about its real size) and the globe placed in its center, it should have been increased as much as that†, and as the globe was really placed three times as

* [Note 17.]

† Let the globe $Bb\beta$, whose centre is C, be insulated in the hollow globe $Dd\delta$ concentric with [it]. Let the inner globe be pos. electrified by the canal BE not



communicating with the outer globe, and let the outer globe communicate with the ground. The quant. defic. fluid in the outer globe must be equal to the redundant near to the floor as to the ceiling*, I suppose the effect to have been still greater.

336] In order to find out, if possible, how much the charge of the globe was increased hereby, I made four flat plates of a mixture of rosin and bees wax†, about 4 inches square and ·22 thick, and coated each of them with circles of about 1·8 inches in diameter, and compared the charge of each of them separately with that of a circular plate of tin, 9·3 inches in diameter. I then compared the charge of two of these plates together with that of a tin circular plate $18\frac{1}{2}$ inches in diameter, and lastly I compared the charge of all together with that of a circle of 36 inches diameter‡.

337] By a mean of the different experiments it appears that the charge of each of the rosin plates was alike, and that the charge of any one of them was to that of the circle of 9·3 inches as 10·34 to 9·3, that the charge of the circle of $18\frac{1}{2}$ inches was to that of two of the rosin plates together as 20·19 to 21·96, and that the charge of the circle of 36 inches was to that of all four plates as 43·75 to 42·06.

But the charge of the four plates together will not be exactly four times the charge of one plate singly, as some allowance must be made for the charge of the wire connecting their upper surfaces, and, besides that, the charge of the plates when placed close together will not be quite so great as if placed at a distance fron each other§.

in the inner globe, and the attraction of the outer globe on the canal BE is to the repulsion of the inner one thereon as

$$\frac{1}{CD} : \frac{1}{BC},$$

and therefore the quantity of redun. fluid in the inner globe is to that which it would contain if the outer globe were away as

$$\frac{1}{BC} : \frac{1}{BC} : \frac{1}{DC} :: DC : DC - BC.$$

If room was spherical, 16 feet in diameter, globe in middle of it, its charge should be increased in ratio of 16 to 15 by reason of undercharged floor, &c.

* [This is the only indication of the height of the room. The circles were suspended by silk strings from a horizontal bar (Art. 466) 87·5 inches from the floor. By Art. 474 the platform 14 inches high diminished the height of the bodies in the ratio of 2 to 3. Hence the height of the center of the bodies from the floor was 42 inches, and the height of the room 4 × 42 inches, or 14 feet. This would agree with the height of the top of the circle of 18 inches being 51 inches from the floor (Art. 472).]

† These plates are non-conductors of electricity, and may be charged as Leyden vials. The manner in which I made them will be described in the following pages [Arts. 373, 514]. My reason for making them of these materials is that the charge of such a plate is much less than that of a plate of glass of the same dimensions.

‡ It must be observed that, in the two last mentioned comparisons, the rosin plates were placed close together and their upper surfaces connected by a piece of wire.

§ [Art. 557.]

By trying the charge of all four rosin plates together by the machine, Fig. 20, both when placed close together and at as great a distance from each other as I could, I found their charge when close together to be to their charge when placed at a distance nearly as 41 to $41\frac{1}{2}$, and, from some other experiments I made, I am inclined to think that the charge of each of the wires which connected the upper coatings of the plates was to that of one plate alone as 28 to 930*.

From these circumstances, I am inclined to think that the charge of two plates together is to that of one plate alone as 21·96 to 10·34, and that the charge of the four plates together is to that of one alone as 42·06 to 10·34, and consequently that the charges of the tin circles of 9·3 inches, $18\frac{1}{2}$ inches and 36 inches are to each other as 9·3, 20·19 and 43·75†.

338] Though I do not know how to calculate how much the charge of the circles ought to be increased by the attraction of the undercharged ground, yet I think there can be little doubt but that if the charge of the plate of $18\frac{1}{2}$ inches is increased in any ratio whatever as that of x to $x - 18\frac{1}{2}$, the charge of the plate of 36 inches will be increased in the ratio of x to $x - 36$, and that of the plate of 9·3 inches in the ratio of x to $x - 9{\cdot}3$; therefore if we suppose that the charge of the $18\frac{1}{2}$ inch plate is increased in the ratio of 9 to 8, or of $166\frac{1}{2}$ to $166\frac{1}{2} - 18\frac{1}{2}$, the charges of the three plates should be to each other as

$$\frac{36 \times 166\frac{1}{2}}{130\frac{1}{2}}, \quad \frac{18\frac{1}{2} \times 166\frac{1}{2}}{148} \quad \text{and} \quad \frac{9{\cdot}3 \times 166\frac{1}{2}}{157{\cdot}2},$$

that is, as 43·37, 19·65 and 9·3,

which agrees very nearly with experiment, and nearer so than it would have done if we had supposed the charge of the $18\frac{1}{2}$ inch plate to have been increased in any other proportion which can be expressed in small numbers‡.

339] I think we may conclude therefore that the charge of the 12·1 inch globe was increased by the attraction of the undercharged ground nearly in the proportion of 9 to 8, for I think there can be little doubt but that the charge of the globe must be increased thereby in nearly the same ratio as that of the $18\frac{1}{2}$ inch plate, and therefore we may conclude that the charge of the plate D is to the charge which the 12·1 inch globe would receive, if it was placed at a great distance from any over or undercharged matter, nearly in the proportion of 26·3 to 12·1, or, in other words, the charge of the plate D is 26·3, which is rather more than eight times greater than it ought to be if the electric fluid did not penetrate into the glass. I shall speak further as to the cause of this in [Art. 349].

* [Arts. 555, 558, also 443.] † [Art. 649.]

‡ [Art. 652, and Note 24.]

340] In order to try the charge of what Æpinus* calls a plate of air, I took two flat circular plates of brass, 8 inches in diameter and ¼ thick, and placed them on the bars *Nn* and *Pp* of the machine (Fig. 20), the two plates being placed one over the other, and kept at a proper distance from each other by three small supports of sealing-wax placed between them, the supports being all of the same height, so that the plates were exactly parallel to each other. Care was also taken to place the plates perpendicularly over each other, or so that the line joining their centers should be perpendicular to their planes.

The lowermost plate communicated with the ground by the wire *RS*, and the uppermost communicated with *Mm* by the wire *V*, just as was done in trying the Leyden vials.

I then found its charge, or the quantity of redundant fluid in the uppermost plate, in the usual manner, by comparing it with the plate *D*, and found it to be to that of *D* as†....

341] As I was desirous of trying larger plates than these, and was unwilling to be at the trouble of getting brass plates made, I took two pieces of plate-glass‡ 11½ inches square, and coated each of them on one side with a circular plate of tinfoil 11·5 inches in diameter, and placed them on the machine as I did the brass plates in the former experiment, with the tinfoil coatings turned towards each other, and kept at the proper distance by supports of sealing-wax as before, care being taken that the tinfoil coatings should be perpendicularly over each other.

For the more easy making a communication between the circular coating of the lower plate and the ground, and between that of the upper plate and the wire *Mm*, I stuck a piece of tinfoil on the back of each plate, communicating by a narrow slip of the same metal with the circular coatings on the other side.

I then tried the charge as before, the lower plate communicating with the ground and the upper with the wire *Mm*.

As glass does not conduct electricity, it is plain that the quantity of electric fluid in the pieces of tinfoil will be just the same that it would be if the glass was taken away, and the pieces of tinfoil kept at the same distance as before.

The distance of the two circular coatings of tinfoil was measured by the same instrument with which I measured the thickness of the plates of glass, and may be depended on to the 1000th or at least to the 500th part of an inch§.

* [*Mém. Berl.* 1756, p. 119.]

† The memoranda I took of that experiment are lost, but to the best of my remembrance the result agreed very well with the following experiment.

‡ [Art. 517.]

§ [See Art. 459, "Bird's instrument," and "dividing machine," Art. 517. Also 594, 595.]

342] In this manner I made the experiment with the plates at four different distances, namely ·910, ·420, ·288 and ·256, and when I had made a sufficient number of trials with the plates at each distance, I took off these circular coatings and put on smaller, namely of 6·35 inches diameter, and tried the experiment as before with the plates at ·259 inches distance. The result of the experiments is given in the following table:

343*]

No. of Experiment	Distance of the tinfoil coatings	Diameter of the coatings corrected for the spreading of electricity	Computed charge	Observed charge	Observed charge by computed charge	Diameter of coatings by distance of ditto
1	·910	11·5	18·2	27	1·49	12·6
2	·420	......	39·4	52	1·32	27·4
3	·288	......	57·4	72·1	1·26	40
4	·256	......	64·6	78·3	1·21	45
5	·259	6·35	19·5	26·5	1·36	24·5

It is plain that some allowance ought to be made in these trials for the spreading of the electricity on the surface of the glass. In the above table I have supposed it to spread ·05 of an inch, but the effect is so small that it is of very little signification whether that allowance is made or not.

344] In my former paper [Art. 134] I expressed a doubt whether the air contained between the two plates in this experiment is overcharged on one side and undercharged on the other, as is the case with the plate of glass in the Leyden vial, or whether the redundant and deficient fluid is lodged only in the plates, and that the air between them serves only to prevent the electricity from running from one plate to the other, but the following experiment shews that the latter opinion is true.

I placed the two brass plates on the machine (Fig. 20), and tried their charge as before, except that, after having charged the plates†, I immediately lifted up the upper plate by a silk string so as to separate it two or three inches from the lower one, and let it down again in its place before I found its charge by making the communication between *Bb* and *Dd* and between *Aa* and *Ee*.

The way I did this was that as soon as I had let down the wire *Cc* on *Aa* and *Bb*, and thereby charged the plates, I lifted it up again half way so as to take away the communication between *Cc* and the upper plate &c., but did not lift it quite up, so as to make the communication between *Bb* and *Dd*, and between *Aa* and *Ee*, till after I had separated the upper plate from the lower, and put it back in its place.

* [See Arts. 669, 519.]

† [Arts. 511, 516, Dec. 18, 26, 1772.]

I could not perceive any sensible difference in the charge, whether I lifted up the upper plate in the above-mentioned manner, or whether I tried its charge without lifting it up.

345] It is plain that in lifting up the upper plate from the lower and letting it down again, the greatest part of the air contained between the two plates must be dissipated and mixed with the other air of the room, so that if the air contained between the two plates was overcharged on one side and undercharged on the other, the charge must have been very much diminished by lifting up the upper plate and letting it down again, whereas, as I said before, it was not sensibly diminished.

I think we may conclude, therefore, that redundant and deficient fluid is lodged only in the plates, and that the air between them serves only to prevent the electricity from running from one plate to the other.

346] As this is the case, the charge of these plates ought, according to the theory, to be equal to that of a globe whose diameter equals the square of the radius of the plate or circular coating divided by twice their distance, that is, to their computed charge, provided the electricity is spread uniformly on the surface of the plates, and therefore in reality the numbers in the last column but one ought to be rather greater than in the last but two, and moreover the less the distance of the plates is in proportion to the diameter of the coating, the less should be the proportion in which those numbers differed, and if the distance is infinitely small in proportion to the diameter, the proportion in which those numbers differ, should also be infinitely small.

347] This will appear by inspecting the table to be the case, only it seems from the manner in which the numbers decrease, that they would never become equal to unity though the distance of the plates was ever so small in respect of their diameter, and I should think, or rather I imagine, would never be less than 1·1, so that it seems as if the charge of a plate of air was rather greater in proportion to that of the globe than it ought to be, and I believe nearly in the proportion of 11 to 10*.

348] The reason of this, I imagine, is as follows. It seems reasonable to conclude from the theory that when a globe or any other shaped body is connected by a wire to a charged Leyden vial, and thereby electrified, the quantity of redundant fluid in the globe will bear a less proportion to that on the positive side of the jar than it would do if they could be connected by a canal of incompressible fluid†, but in all probability when a plate of air is connected in like manner to the Leyden vial, the quantity of redundant fluid on its positive side will bear nearly the same proportion to that in the vial that it would do if they were connected by a canal of

* [Art. 670.]

† This seems likely from Appendix, Coroll. V [Art. 184].

incompressible fluid, and consequently the charge of the plate of air in these experiments ought to bear a greater proportion to that of the globe than if they had been connected to the vial by which they were electrified by canals of incompressible fluid.

349] It was said in Art. 339 that the charges of the glass plates were rather more than eight times greater than they ought to be by the theory, if the electric fluid did not penetrate to any sensible depth into the glass. Though this is what I did not expect before. I made the experiment, yet it will agree very well with the theory if we suppose that the electricity, instead of entering into the glass to an extremely small depth, as I thought most likely when I wrote the second part of this work*, is in reality able to enter into the glass to the depth of $\frac{7}{16}$ of the whole thickness of the glass, that is, to such a depth that the space into which it can not penetrate is only $\frac{1}{8}$ of the thickness of the glass, as in that case it is evident that the charge should be as great as it would be if the thickness of the glass was only $\frac{1}{8}$ of its real thickness, and the electricity was unable to penetrate into it at all.

350] There is also a way of accounting for it without supposing the electricity to enter to any sensible depth into the glass, by supposing that the electricity at a certain depth within the glass is moveable, or can move freely from one side of the glass to the other.

Thus, in Fig. 25, let *ABDE* be a section of the glass plate perpendicular to its plane, suppose that the electricity from without can pene-

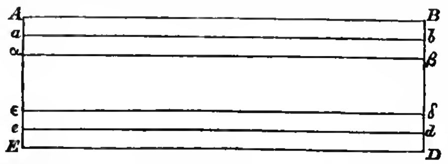


Fig. 25.

trate freely into the glass as far as the line *ab* or *ed* but not further, suppose too that within the spaces $ab\beta\alpha$ and $ed\delta\epsilon$ the electric fluid is immoveable, but that within the space $\alpha\beta\delta\epsilon$ it is moveable, or is able to move freely from the line $\alpha\beta$ to $\delta\epsilon$. Then will the charge of the plate be just the same as on the former supposition, provided the distances $a\alpha$ and $e\epsilon$ are each $\frac{1}{16}$ of the thickness of the plate†.

* [Refers to Art. 132.]

† The only reason why I suppose the electric fluid to be able to enter into the glass from without as far as the lines *ab* and *ed* is that Dr Franklin has shewn that the charge resides chiefly in the plate of glass and not in the coating, and consequently that the electricity is able to penetrate into the glass to a certain depth. Otherwise it would have done as well if we had supposed the fluid to be immoveable in the whole spaces $AB\beta\alpha$ and $ED\delta\epsilon$, and that the distance $A\alpha$ and $E\epsilon$ are each $\frac{1}{16}$ of AE.

351] But I think the most probable supposition is that there are a great number of spaces within the thickness of the glass in which the fluid is alternately moveable and immoveable.

Fig. 26.

Thus let *ABDE* (Fig. 26) represent a section of the plate of glass as before, and let the glass be divided into a great number of spaces by the parallel lines *ab*, *αβ*, *ed*, *εδ*, &c., and suppose that in the two outermost spaces *ABba* and *EDde* the fluid is moveable, that in the two next spaces *abβα* and *edδε* it is immoveable, and that in the two next spaces it is moveable, and so on. The charge will be the same as before, supposing the sum of the thickness of the spaces in which the electricity is immoveable to be $\frac{1}{8}$ of the whole thickness of the glass, as it is shewn that the charge of such a plate will be the same as that of a plate in which the electricity is entirely immoveable, whose thickness is equal to the sum of the thicknesses of those spaces in which we supposed the fluid immoveable*.

352] It must be observed that in those spaces in which we supposed the fluid to be moveable, as in the space *ABba* for example, though the fluid is able to move freely from the plane *Ab* to *ab*, that is, though it moves freely in the direction *Aa* or *aA*, or in a direction perpendicular to the plane of the plate, yet it must not [be] able to move lengthways, or from *A* to *B*, for if it could, and one end of the plate *AE* was electrified, some fluid would instantly flow from *AE* to *BD*, and make that end overcharged, which is well known not to be the case. The same thing must be observed also with regard to the two former ways of explaining this phenomenon.

353] The chief reason which induces me to prefer the latter way of accounting for it is that in the two former ways the thickness of the spaces in which the fluid is moveable must necessarily be very considerable. In thick glass, for example, in a plate of the same thickness as *D*, it must be not less than $\frac{9}{100}$ of an inch in the first way of explaining it, and in the second way it must be still greater. Now if the electric fluid is able to move through so great a space in the direction *AE*, it seems extraordinary that it should not be able to move in the direction *AB*, whereas in the latter way of accounting for it the thickness of the spaces in which the electricity is moveable may be supposed infinitely small, and conse-

* [Prop. XXXV, Art. 169, and Note 15.]

quently the distance through which the electricity moves in the direction AE also infinitely small.

354] Another thing which inclines me to this way of accounting for it is that there seems some analogy between this and the power by which a particle of light is alternately attracted and repelled many times in its approach towards the surface of any refracting or reflecting medium. See Mr Michell's explanation of the fits of easy reflection and transmission in Priestley's *Optics*, page 309.

355] To whichever of these causes it is owing that the charges of these plates are so much greater than they should be if the electric fluid was unable to enter into the plate, it was reasonable to expect that the greater the force with which the plate was electrified, the greater should be the depth to which the electric fluid penetrates into the glass, or the greater should be the thickness of the spaces in which we supposed the fluid to be moveable, and consequently in comparing the charge of the plate D with the circle of 36 inches diameter, or with any other body, the greater the force with which they are electrified the greater proportion should the charge of the glass plate bear to that of the circle.

356] I therefore compared the charge of the plate D with that of the circle of 36 inches with electricity of two different degrees of strength, namely the same which I made use of in [Art. 329], in trying whether the distance to which the electricity spread on the surface of glass was different according to the strength of the electricity.

The way in which I compared their charges was just the same that I made use of in comparing the rosin plate with the tin circles in [Art. 337]. The event was that I could not perceive that the proportion which their charges bore to each other with the stronger degree of electricity was sensibly different from what they did with the weaker*.

357] But it must be remembered that it seemed from the experiment related in [Art. 329], that the electricity spread $\frac{1}{30}$ of an inch further on the surface of the glass with the stronger degree of electricity than with the weaker. The difference of charge owing to this difference in the spreading of the electricity is $\frac{1}{16}$ part of the whole, so that it seems that if the electricity had been prevented from spreading on the surface of the glass, the proportion of the charge of the glass plate to that of the tin circle would have been less with the stronger degree of electricity than with the weaker, and that nearly in the proportion of 16 to 17.

358] I also made an experiment to determine whether the charge of a coated plate of glass bore the same proportion to that of another body when the electricity was very weak as when it was of the usual strength†.

* [Arts. 547, 551, 553, also Arts. 451, 463, 526, 535, 538, 664.]

† [Arts. 539, 666.]

For this purpose I first found what proportion the charge of a tin cylinder 15 feet long and 17 inches in circumference bore to that of the two plates *D* and *E* together when the electricity was very weak. This I did in the manner represented in Fig. 27, where *AB* is the tin cylinder

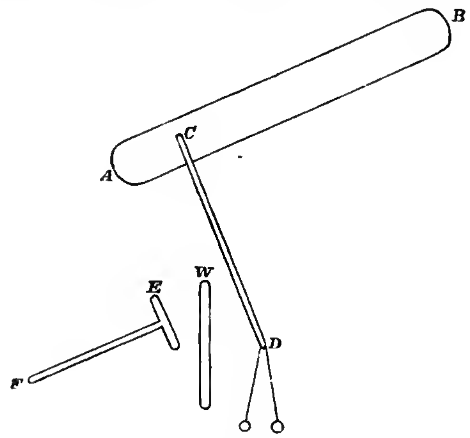


Fig. 27.

supported horizontally by non-conductors. *DC* is a brass wire 37 inches long and about $\frac{1}{8}$ inch in diameter supported also horizontally by non-conductors, the end *C* being in contact with the cylinder, and a pair of fine pith balls being suspended from the other end *D*. *FE* is a piece of wire communicating with the prime conductor, and between it and *DC* is suspended by a silk string the wire *W* in a vertical situation.

359] The cylinder *AB*, and consequently the wire *DC*, were first electrified negatively to such a degree as to make the pith balls separate to the distance of one diameter of the balls. The prime conductor and wire *FE* being then charged to the usual degree, as shewn by the usual electrometer hung down from it, one end of the wire *W* was brought in contact with *E* so as to be electrified by it, and was then immediately removed and brought in contact with *DC* so as to communicate its electricity to the cylinder*.

Now I found that if the wire *W* was 29 inches long, and $\frac{1}{8}$ in diameter, and its electricity was twice communicated in this manner to the cylinder, the pith balls would separate as much positively as they before did negatively, consequently the cylinder *AB* and the wires *DC* and *W* together, when electrified to such a degree as to make the pith balls separate one diameter, contain as much electricity as the wire *W* alone does when electrified in the usual degree.

The cylinder *AB* was then removed, and the two glass plates *D* and *E* placed under the wire *DC* in its room, their upper coatings communicating

* [See plan at Art. 539.]

with DC, and their lower coatings with the ground, and the operation performed as before. I found that I was obliged to change the wire W for one of the same thickness, and only 22 inches long, in order that the pith balls should separate the same as before.

360] Therefore the charge of the two plates D and E and the wire DC together is to that of the tin cylinder and wire DC together as the charge of a wire $\frac{1}{6}$ inch thick and 22 inches long to that of a wire of the same thickness and 29 inches long, that is, as 1 to 1·26, and consequently, as the charge of the wire DC is but small in comparison of that of the two plates, the charge of the two plates will be to that of the tin cylinder pretty nearly in the same proportion of 1 to 1·26*.

Having thus found what proportion the charges of the plates and cylinder bear to each other when electrified in a very weak degree, I tried what proportion they bore with the usual degree of electrification.

361] To this purpose I placed the two plates on the machine represented in Fig. 20 between M and m in the usual manner, and on the other side I placed a sliding coated plate, and found as usual what size must be given to the coating of this plate that the pith balls should just separate positively, and what size must be given to it that they should just separate negatively.

I then removed the two plates and suspended the tin cylinder so as to touch the wire Mm, but without touching any other part of the machine, and found what size it was necessary to give to the coating of the sliding plate that the pith balls should separate as before.

By this means the charge of the tin cylinder was found to be to that of the two plates as 1·33 to 1. Therefore the charge of the two plates seems to bear pretty nearly the same proportion to that of the cylinder whether the electricity is of the usual strength or very weak. But if we suppose that the electricity spreads ·07 inches on [the] surface of glass with the usual degree of electrification, and that it does not spread sensibly with the weak degree of electrification, then the proportion which the charge of the glass plates bears to that of the cylinder should be less with the usual degree of electrification than with the weak one, and that by about $\frac{1}{8}$ part.

This difference, however, is not more than what might very well proceed from the error of the experiment.

362] On the whole, I am uncertain whether the charge of a glass plate would really bear a rather less proportion to that of a globe or other body when the electricity is strong than when it is weak, provided the electricity

* I believe the true proportion is between that of 1 to 1·28 and that of 1 to 1·37, but as the experiment is not capable of much accuracy, I think it needless to trouble the reader with the computation. [See Art. 666, and Note 25, p. 416.]

was prevented from spreading on the surfaces as it should seem by these experiments, or whether it was not rather owing partly to the error of the experiment, and partly to there not being so much difference in the distance to which the electricity spreads on the surface of the glass according to the different degree in which it is electrified, as I imagined.

If the first of these suppositions is true, I do not know how to reconcile it with the theory, except by supposing that the greater the force with which the plate is electrified the less is the depth to which the electricity penetrates into the glass, or the less is the thickness of the spaces in which we supposed the fluid to be moveable.

Though it seemed natural to expect that the electric fluid should penetrate further into the glass, or that the fluid within the glass should move through a greater space when the glass was strongly electrified than when weakly, that is, when the force with which the fluid was impelled was great than when it was small, yet it is not strange that it should be otherwise, as it is very possible that the electric fluid may penetrate with great freedom to a certain depth within the glass, and that no ordinary force shall be able to impel it sensibly further, and in like manner it is very possible that the fluid may be able to move with perfect ease in the space $\alpha\epsilon$ (Fig. 25) and yet that no ordinary force shall be able to move the fluid at all beyond that space.

But it would be very strange that the fluid should penetrate to a less depth within the glass, or that the fluid within the glass should move through a less space when the glass is strongly electrified than when weakly.

363] The reader perhaps may be tempted from this circumstance to think that the reason of the actual charge of the glass plates so much exceeding their computed charge is not owing to the electric fluid penetrating into the glass, or to any motion of the fluid within the glass, but to some error in the theory. But I think the experiments on the plate of air [Art. 344] form a strong argument in favour of its being owing to the penetration of the electric fluid into, or its motion within the glass, for it appears plainly from these experiments that the electric fluid does not penetrate into the air, and on account of the fluidity of the air it seems very improbable that the electric fluid within the air should be able to move in the manner we supposed it to do within the glass; whereas it appears plainly from Dr Franklin's analysis of the Leyden vial, that the electric fluid does actually penetrate into the glass.

Therefore as this excess of the observed charge above the computed does not take place in the plate of air, where it could not do it consistently with the theory, but does in the glass plate, where it may do so consistently with the theory, I think there seems great reason to think that it is not owing to any defect in the theory, but to some such motion of the electricity as we have supposed.

364] I could not find that there was any difference in the proportion which the charge of a glass plate bore to that of another body whether they were electrified positively or negatively*.

365] It was said in Art. [331], that there seemed no reason to think that the charge of the plate D, or of any other of those glass plates was sensibly greater than it would be if the electricity was spread uniformly on their surfaces, whereas the charge of most of the plates of air was found very considerably greater than it would be on that supposition. But this is by no means inconsistent, for according to the first way of accounting for the great excess of the real charge of those plates above the computed, namely supposing that the electricity penetrates into the glass to the depth of $\frac{7}{16}$ of its thickness, the increase of its charge on account of the electricity being not spread uniformly, should be not greater than it would be if the glass was only $\frac{1}{8}$ of its real thickness, and the electricity was unable to penetrate into it at all, and therefore should not be greater than it is in a plate of air in which the thickness is $\frac{1}{84}$ of the diameter, and should therefore in all probability be quite imperceptible.

And by Prop. XXXVI [Art. 170], the increase of charge should hardly be much, if at all, greater according to the second or third way of accounting for this phenomenon.

366] In order to try† whether the charge of coated glass is the same when hot as when cold, I made use of the apparatus in Fig. 28, where *ABCba* represents a short thermometer tube with a ball *BCb* blown at the end and another smaller ball near the top. This is filled with mercury as high as the bottom of the upper ball, and placed in an iron vessel *FGMN* filled with mercury as high as *FN*. Consequently the ball *BCb* was coated as a Leyden vial, the mercury within it forming the inside coating, and that in the vessel *FGMN* the outer one.

In trying it, I set the vessel *FGMN* on the wooden bars of the machine represented in Fig. 20, near the end *NP*, and dipt a small iron wire bound round the wire *Mm* into the mercury within the tube, so as to make a communication between the wire *Mm* and the inside coating, the outside coating, or the mercury in *FGMN*, being made to communicate with the ground.

It was heated by a lamp placed under *FGMN*, and its charge was frequently tried while heating by comparing with a sliding coated plate placed on the other end of the wooden bars.

When it was sufficiently heated, the lamp was taken away, and the charge frequently tried in the same manner while cooling, a thermometer being dipt every now and then into the mercury in *FGMN* to find its heat.

367] As it was apprehended that the electricity might spread further on the surface of the glass while hot than while cold, a paper coating

* [Art. 463.] † [Art. 556, March 21, 1773. See also Arts. 548, 549, 680.]

~~could not find that there was any difference in the Ratio whether the charge of a glass plate bore the same proportion to that of another body whether they were electrified positively or negatively~~

~~to P~~ P 116

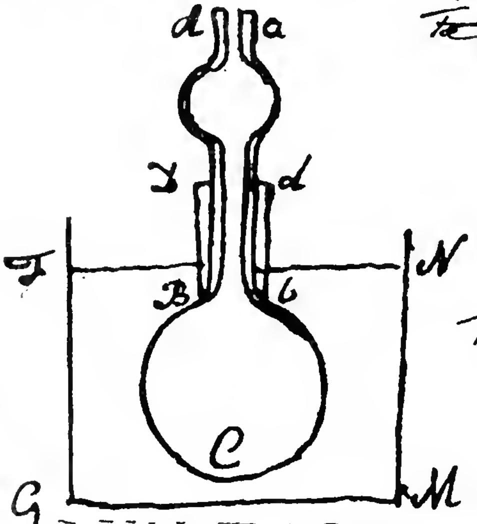


Fig 28

In order to try whether the charge of coated glass is the same when hot as when cold I made use of the appar. in Fig. 28 where ABCba represents a short thermom tube with a ball BCb blown at the end & another smaller ball near the top This is filled with ☿ as high as the bottom of the upper ball & placed in an iron vessel FGMN filled with ☿ as high as FN consequently the ball BCb was ~~charged~~ coated as a Leyden vial the ☿ within

P ...

DBbd was fastened on the tube, so that as the outside coating was made to extend as far as *Dd*, that is three or four inches above the mercury in *FGMN*, where the tube was very little heated, and as the inside coating reached still higher, that is to the bottom of the upper ball, no sensible error could proceed from thence.

The use of the upper ball was to prevent the mercury within the tube from overflowing when hot.

368] By a mean between the experiments made while the ball was heating and while cooling, its charge answering to the different degrees of heat was as follows.

Heat	Charge	Difference of heat	Difference of charge
55	100		
		102	4
157	104		
		65	12
222	116		
		73	20
295	136		
		10	5
305	141		

369] At 295° the electricity passed through the glass pretty freely, but at 305° much faster. It appears, therefore, that the charge of glass is considerably greater when heated to such a degree as to suffer the electricity to pass through than when cold, but that its charge does not begin to be sensibly increased till it is heated to a considerable degree*.

370] *On the charges of plates of several different sorts of glass, and also of plates of some other substances which do not conduct electricity, charged in the manner of Leyden vials.*

The result of the experiments I made on this subject is contained in the two following tables:—

TABLE OF GLASS PLATES†.

	Thickness	Diameter	Ditto corrected	Computed charge	Observed charge	Observed charge by computed charge	Specific gravity
Flint glass ground flat	·2115	2·23	2·37	3·32	26·3	7·93	3·279
Ditto a thinner piece	·104	2·215	2·385	6·84	52·3	7·65	3·284
Plate glass P	·127	2·85	3·02	8·98	71·9	8·01	2·752
W	·172	3·435	3·585	9·34	74·8	8·01	2·787
G	·1848	3·575	3·725	9·38	75·5	8·05	2·973
N	·106	2·12	2·29	6·18	51·4	8·31	2·682
O	·106	2·505	2·675	8·44	75	8·89	2·514
Q	·076	2·065	2·245	8·29	76·5	9·23	2·504
Crown glass	·0682	3·495	3·675	24·76	211·3	8·54	2·537
Ditto another piece	·0659	3·43	3·61	24·72	208·7	8·44	2·532
Crown glass ground	·07	2·035	2·215	8·76	76·5	8·73	} 2·535
Part of same piece	·0693	3·54	3·72	24·96	215·1	8·62	
Mean of the 10 pieces used in former experiments						8·22	2·678

* [Note 26.]

† [See Art. 673.]

371] Plates of other substances*.

		Thickness	Diameter	Computed charge	Observed charge	Observed charge by computed
Gum Lac		·125	4·23	17·89	80	4·47
Mixture of rosin and bees wax. Plate	1	·4845	3·75	3·63	13·5	3·72
	2	·192	3·355	7·22	25·2	3·49
	3	·103	4·247	21·89	69	3·15
	4	·103	4·525	24·85	78·9	3·18
	5	·103	1·79	3·89	13	3·34
Dephlegmated bees wax. Plate	1	·303	3·78	5·90	24·5	4·16
	2	·120	3·525	12·95	46·1	3·56
	3	·063	2·74	14·90	50·5	3·39
Plain bees wax		·119	3·475	12·69	51·3	4·04

372] The coatings of all these plates were circular.

In computing the charge of the glass plates, the diameter of the coating was corrected on account of the spreading of the electricity as in the fourth column, the electricity being supposed to spread ·07 of an inch if the thickness is ·21 and ·09 if the thickness is ·08, and so on in proportion in other thicknesses. But no correction is made in computing the charges of the other plates, as I was uncertain how much to allow.

373] The method I used in making all the plates of the second table was this. I first cast a round plate of the substance, three or four times as thick as I intended it should be, and rather thinner near the edges than in the middle, taking care to cast it as free from air bubbles as I could.

I then heated it between two thick flat plates of brass, till it was become soft, and then pressed it out to the proper thickness by squeezing the plates together with screws†. In order to prevent its sticking to the brass plates, I put a piece of thin tinfoil between it and each plate, and I found the tinfoil did not stick to it so fast but what I could get it off without any danger of damaging them.

374] The heat necessary to melt shell lac is so great as to make it froth and boil; which makes it impossible to cast a plate of it free from air bubbles. The plate mentioned in the preceding table was as free from them as I could make it. It contained, however, a great quantity of minute bubbles, but no large ones.

375] Bees wax melts with a heat of about 145°. If it is then heated to a degree rather greater than that of boiling water, it froths very much, and seems to lose a good deal of watery matter, and if it is kept at this heat till it has ceased frothing, it will then bear being heated to a much higher degree without frothing or boiling. Bees wax thus prepared I call dephlegmated.

In order that the plates of dephlegmated bees wax should all be equally so, I dephlegmated some bees wax with a pretty considerable heat, and

* [See Art. 674.] † [Art. 514.]

suffered it to cool and harden, and out of this lump I made all three plates, taking care in casting them not to heat them more than necessary.

I used the same precautions also in casting the plates of a mixture of rosin and bees wax, the proportion of the rosin to the bees wax was forgot to be set down.

What are called in the table the 4th and 5th plate of rosin and bees wax are in reality the same plate as the 3rd, only with a smaller coating.

376] It appears from these experiments, first, that there is a very sensible difference in the charge of plates of the same dimensions according to the different sort of glass they consist of, the charge of the plates *O* and *Q*, which consisted of the greenish foreign plate glass mentioned in [Art. 301], being the greatest in proportion to their computed charge of any, next to them the crown glass, and the flint glass being the least of all.

Secondly. The charge of the Lac plate is much less in proportion to its computed charge than that of any glass plate, and that of a plate of bees wax, or of the mixture of rosin and bees wax, still less.

But it must be observed that there is a very considerable difference between the three different plates of dephlegmated bees wax in that respect. The same thing, too, obtains in the mixture of rosin and bees wax*.

377] As the proportion of the real charge to the computed is greater in the thick plates than the thin ones, one might be inclined to think that this was owing to the electricity being not spread uniformly. But as the difference seems to be greater than could well proceed from that cause, I am inclined to think that it must have been partly owing to some difference in the nature of the plates. Perhaps it may have been owing to some of the plates having been less heated, and consequently having suffered a greater degree of compression in pressing out than the others.

378] The piece of ground crown glass mentioned in the first of the foregoing tables was made out of a piece of crown glass about $\frac{1}{4}$† of an inch thick, and ground down to the thickness mentioned in the table, care being taken by the workman to take away as much from one side as the other, so that the plate consisted only of the middle part of the glass.

My reason for making it was that as there appears to be a considerable difference in the charge of different sorts of glass, it was suspected that there might possibly be a difference between the inside of the piece and the outside, and if there had, it would have affected the justness of the experiments with the ten pieces of glass ground out of the same piece.

* [Note 27, p. 418.]

† There are pieces of that thickness sometimes blown for the use of the Opticians.

But by comparing the charges of the plates of crown glass with those of the two other pieces of crown glass in the table, there does not seem to be any difference which can be depended on with certainty.

The experiment indeed would have been more satisfactory if the piece of ground glass and the pieces with which it was compared had been all made out of the same pot. But as it would have been difficult procuring such pieces, and as I have found very little difference in the specific gravity of different pieces of crown glass, and as I am informed it is all made at the same glass house, I did not take that precaution.

379] Let two or more flat plates of different non-conducting substances, as *AabB*, *BbcC* and *CcdD*, (Fig. 29) be placed close together and coated in the manner of a single plate with the coatings *Ee* and *Ff*. Let the charge of the plate *AabB*, supposing it placed by itself and coated in the usual manner, be equal to that of a plate of glass whose thickness is *A* and whose coatings are of the same size as those of *AabB*.

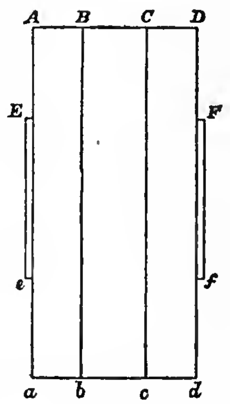


Fig. 29.

In like manner let the charge of *BbcC* be equal to that of a plate of the same glass whose thickness is equal to *B*, and let that of *CcdD* equal that of one whose thickness is *C*.

Then whichever of the three ways of accounting for the excess of the real charge of glass plates above the computed we prefer, it is a necessary consequence of our theory that the charge of this compound plate *AadD* should be equal to that of a single plate of glass whose thickness equals $A + B + C$, and whose coatings are of the same size as *Ee* and *Ff*.

380] In like manner if two or more plates of the same kind of glass are placed together and coated as above, the charge of this compound plate should be equal to that of a single plate of the same glass whose thickness is equal to that of all the plates together. This appears from the following experiments to be the case, for

1st. I took the three plates of glass *A*, *B* and *C**, and laid them on one another, having first taken off their old coatings and coated the outside surfaces as in Fig. 29 with circles of tinfoil 6·6 inches in diameter. The charge of this compound plate was found to be to that of the three plates *D*, *E* and *F* together as ·944 to 1. The sum of the thicknesses of *A*, *B* and *C* together is ·6309, and the computed charge of a plate of that thickness with coatings 6·6 in. diameter is to that of *D*, *E* and *F* together, allowing in the same manner as in [Art. 328] for the instantaneous spreading of the electricity, as ·94 to one. So that the charge of this compound plate is exactly the same that it ought to be according to the foregoing rule.

* [Arts. 534, 544, 546, 677.]

381] 2ndly, I made a plate of a mixture of rosin and bees wax*, about 8 inches square and somewhat more than ·12 thick, and coated it with circles 6·61 in. diameter. Its charge was found to be to that of the plates *K*, *D* and *E* together as 56 to 55, and therefore should be equal to that of a plate of glass of the same kind as *K* whose thickness is ·345 and the diameter of whose coatings is the same as those of the rosin plate, namely 6·61 inches.

This plate was then inclosed between the glass plates *B* and *H* †, the coatings being first taken off, and the outside surfaces of *B* and *H* coated with circles 6·6 inches in diameter. Its charge was found to be to that of *K* as 7·56 to 8.

According to the foregoing rule, its charge should be the same as that of a plate of glass of the same kind as *B* ·634 of an inch thick with coatings 6·6 inches in diameter, and should therefore be to that of *K* as 7·34 to 8, which is very nearly the same that it was actually found to be.

382] On the charges of such Leyden vials as do not consist of flat plates of glass.

These experiments were made with hollow cylindrical pieces of glass, open at both ends, and coated both within and without with pieces of tinfoil surrounding the cylinder in the form of a ring, the breadth of the ring being everywhere the same, and the inside and outside coatings being of the same breadth, and placed exactly opposite to each other. Only as the inside diameter of the two thermometer tubes was too small to admit of being coated in this manner, they were filled with mercury by way of inside coating.

The thickness of the glass was found by suspending the cylinder by one end from a pair of scales with its axis in a vertical position, and the lower part immersed in a vessel of water, and finding the alteration of the weight of the cylinder according as a greater or less portion of it was under water ‡.

383] The result of the experiments is contained in the following table §.

	Mean thickness	Mean outside semi-diameter	Length of coating	Computed charge	Observed charge	Observed charge by computed	Specific gravity	Outside diameter by thickness
Part of a jar of flint glass	·084	1·62	4·4	85·9	717	8·35	3·254	19·3
A cylinder of ditto	·0704	·645	9·86	87·1	650	7·46	3·281	9·2
Thermometer tube I.	·094	·14	11	11·0	80·2	7·31	3·098	1·5
,, ,, II.	·130	·16	15·5	11·1	80·7	7·26	3·243	1·24
Cylinders of green bottle glass 1	·045	·50	7·16	77·2	754	9·77	2·665	11·3
Cylinders of green bottle glass 2	·060	·53	8·55	76·6	690	9	2·664	8·8
Cylinders of green bottle glass 3	·078	·48	7	40·8	353	8·65	2·665	6·2

* [Arts. 548, 678.]

† [Arts. 552, 679.]

‡ [Art. 594.]

§ [See Art. 676, and Note 28.]

The lengths of the coating here set down are the real lengths. But in computing the charges of the white jar and cylinder and the three green cylinders, these lengths were increased on account of the spreading of the electricity according to the same supposition as was used in computing the charges of the flat plates.

But in computing the charges of the thermometer tubes no correction was made, as I was uncertain how much to allow, but as the length of their coatings is so great, this can hardly make any sensible error.

384] It should seem from these experiments as if the proportion of the real to the computed charge was rather less in a cylinder in which the thickness of the glass is $\frac{1}{6}$ of the semidiameter than in one in which it is only $\frac{1}{11}$, and most likely rather less in that than in a flat plate, but then it seems to be not much less in a cylinder in which the inside diameter is many times less than the outside, that is, in which the thickness of the glass is almost equal to the outside semidiameter, than it is in the first mentioned cylinder.

Nothing certain, however, can be inferred as to this point, as in all probability the four pieces of flint glass used in these experiments and the two flat pieces used in [Art. 370] did not consist exactly of the same kind of glass, as indeed appears from their specific gravities.

385] The three green cylinders, indeed, were all made at the same time and out of the same pot, so that it seems difficult to suppose that there should be any difference of that kind between them*. But then I had no flat plates to compare them with.

On the whole, I think we may with tolerable certainty infer that the ratio of the real to the computed charge is not very different from what it is in flat plates, whatever is the proportion which the thickness of the glass bears to the diameter of the cylinder, though it seems to be not exactly the same.

* Though it seems not likely that there should be any difference in the nature of the glass of which the three green cylinders consisted, yet I am not sure that there was not, for the inside of the glass, that is, that part which was nearest to the inside surface, was manifestly more opaque and of a different colour from the outside, and the separation between these two sorts of glass appeared well defined, so that the cylinder seemed to consist of two different coats of glass lying one over the other. The distinction was the most visible in those cylinders which consisted of the thickest glass and in the thickest part of those cylinders. The specific gravities, however, do not indicate any difference in the nature of the glass. What was the reason of the above-mentioned appearance I cannot tell.

{IV.} WHETHER THE FORCE WITH WHICH TWO BODIES REPEL IS AS THE SQUARE OF THE REDUNDANT FLUID, TRIED BY STRAW ELECTROMETERS*.

[From MS. N°. 8: hitherto unpublished. See Table of Contents at the beginning of this volume.]

386] If two bodies, *A* and *B*, placed near to each other, are both connected to the same overcharged Leyden jar, and the force with which this jar is electrified is varied, everything else remaining unaltered, the force with which *A* and *B* repel each other ought by the theory to be as the square of the quantity of redundant fluid in the jar, supposing the distance of the bodies *A* and *B* to remain unaltered. For the quantity of redundant fluid in *A* is directly as the quantity of redundant fluid in the jar, and therefore the force with which each particle of redundant fluid in *B* is repelled by *A* is also directly as the quantity of redundant fluid in the jar, and therefore as the number of particles of redundant fluid in *B* is also as the quantity of redundant fluid in the jar, the force with which *B* is repelled by *A* is as the square of the quantity of redundant fluid in the jar.

387] In order to try whether this was the case, I made use of the following apparatus †.

CD (Fig. 31) is a wooden rod 43 inches long, covered with tinfoil and supported horizontally by non-conductors. At the end *C* is suspended, as

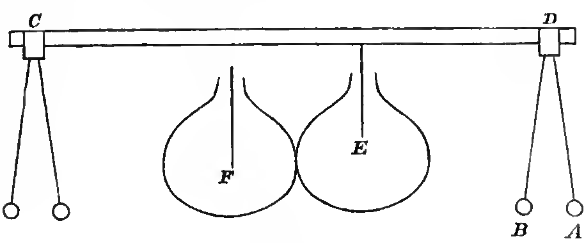


Fig. 31.

in the figure, the electrometer described in Art. 249, and at the other end *D* is suspended a similar electrometer, only the straws reached to the bottom of the cork balls *A* and *B*, but not beyond them, and were left open so as to put in pieces of wire, and thereby increase their weight and the force with which they endeavoured to close. The lower ends of these

* [Title supplied from Cavendish's Index to {the results of} his experiments, Art. 563.]

† [Arts. 563, 567, also Art. 525.]

wires when used were just even with the bottom of the cork balls, and were kept in that situation by wax, the wax being cut off even with the bottom of the corks, so as to leave no roughnesses to carry off the electricity. In like manner, when the wires were not used, the ends of the straws were closed up with wax.

388] The proportion which the force with which the balls of this electrometer endeavoured to close when the wires were inserted bore to that with which they endeavoured to close without the wires was thus found. The weight of the straw $\begin{cases} A \\ B \end{cases}$ with its ball and centre pin but without its wire was found to be $\begin{cases} 7{\cdot}6 \\ 6{\cdot}65 \end{cases}$ grains, and the distance of its center of gravity from the center of suspension was $\begin{cases} 5{\cdot}36 \\ 5{\cdot}285 \end{cases}$ inches, as was found by balancing it on the edge of a knife. Consequently the force with which this straw, when put in its place, endeavours to descend towards the perpendicular, supposing it to be removed to a given distance from it, was as $\begin{cases} 7{\cdot}6 \times 5{\cdot}36 \\ 6{\cdot}65 \times 5{\cdot}285 \end{cases}$.

The weight of the wire inserted was $\begin{cases} 12{\cdot}05 \\ 10 \end{cases}$ grains, and half its length was $\begin{cases} 1{\cdot}23 \\ 1{\cdot}00 \end{cases}$ inches, so that as the distance of the bottom of the cork balls from the center of suspension was 11·1 inches, the distance of its center of gravity from the center of suspension was $\begin{cases} 9{\cdot}87 \\ 10{\cdot}1 \end{cases}$ inches, and therefore the excess of the force with which the ball endeavours to descend towards the perpendicular when the wire is inserted above that with which it endeavours to descend without [the wire] is to the force with which it endeavours to descend without the wire as $\begin{cases} 12{\cdot}05 \times 9{\cdot}87 \\ 10 \times 10{\cdot}1 \end{cases}$ to $\begin{cases} 7{\cdot}6 \times 5{\cdot}36 \\ 6{\cdot}65 \times 5{\cdot}285 \end{cases}$, or as $\begin{cases} 2{\cdot}92 \\ 2{\cdot}88 \end{cases}$ to one. Therefore the force with which the electrometer endeavours to close when the wires are inserted is to that with which it endeavours to close without the wires as 3·9 to 1.

389] *E* and *F* are two coated Leyden vials, nearly of the same size. The outside coatings of both communicate with the ground, and the inside coating of *E* communicates with *CD*, but not that of *F*.

390] The way in which I tried the experiment was as follows. I first compared the electrometer *C* with the electrometer *D* without the wires, and found that when the jar *E* was electrified to such a degree as to make *D* separate $\begin{cases} 13 \\ 12 \end{cases}$ divisions, *C* separated $\begin{cases} 14\frac{1}{4} \\ 13\frac{1}{4} \end{cases}$ divisions, so that the same degree of electrification which made C separate $\left\{\begin{matrix}13\\12\end{matrix}\right.$ divisions made D separate $\left\{\begin{matrix}14\frac{1}{4}\\13\frac{3}{4}\end{matrix}\right.$ divisions.

I then put the wires into the electrometer D, and put the larger of the two vials in the place of E, and electrified E and consequently the rod CD and the two electrometers till D separated $\left\{\begin{matrix}13\\12\end{matrix}\right.$ divisions.

The wire by which E was electrified was then immediately taken away and a communication made between E and F, so that the redundant fluid in E and CD and the electrometers was communicated to F.

It was found that the electrometer C then separated $\left\{\begin{matrix}15\frac{1}{4}\\14\end{matrix}\right.$ divisions.

The experiment was then repeated in the same manner, except that the smaller vial was placed at E. It was found that if E was electrified till D separated $\left\{\begin{matrix}13\\12\end{matrix}\right.$ divisions, then on making a communication between E and F, C separated $\left\{\begin{matrix}13\frac{1}{4}\\12\frac{1}{2}\end{matrix}\right.$ divisions.

391] From hence we may conclude that if the vials had been exactly equal and E had been electrified till D separated $\left\{\begin{matrix}13\\12\end{matrix}\right.$ divisions, then on making a communication between E and F, C would have separated $\left\{\begin{matrix}14\frac{1}{4}\\13\frac{1}{8}\end{matrix}\right.$ divisions.

But it appears from the first mentioned part of the experiment, that the same degree of electrification which makes C separate $\left\{\begin{matrix}14\frac{1}{4}\\13\frac{1}{8}\end{matrix}\right.$ divisions is sufficient to make D without the wires separate $\left\{\begin{matrix}13\\11\frac{7}{8}\end{matrix}\right.$ divisions. From whence it appears that if the jars are exactly equal, and one of them is electrified till the electrometer D with the wires separates $\left\{\begin{matrix}13\\12\end{matrix}\right.$ divisions, and its electricity is then communicated to the other vial, the electricity will be of that degree of strength which is necessary to make the same electrometer without the wires separate $\left\{\begin{matrix}13\\11\frac{7}{8}\end{matrix}\right.$ divisions, that is, very nearly the same as before, or as it did with the wire before the communication of the electricity.

But if the vials are equal, the quantity of redundant fluid in the first vial, after its electricity is communicated to the second, will be very little more than half of what it was before the communication, for the quantity of redundant fluid in the rod DC and the electrometers is trifling in comparison of that in the vial*, and consequently it appears that the distance to which the electrometer with the wires in it separates with a given quantity of redundant fluid in the vial is very nearly the same as that to which it separates without the wires when there is only half that quantity of redundant fluid in the vial.

Therefore as the force with which the electrometer endeavours to close by its weight when the wires are in is to that with which it endeavours to close without the wires as 3·9 to 1, it appears that the force with which the balls of the electrometer are repelled with a given quantity of redundant fluid in the vial, is to that with which they are repelled when there is only half that quantity of redundant fluid in the vial as 3·9 to 1 (supposing the distance of the balls to be the same in both cases), that is, very nearly as the square of the quantity of redundant fluid in the vial, the difference being not more than what might very easily be owing to the error of the experiment. So that the experiment agrees very well with the theory.

392] It was found that if the communication was made between the two vials by a piece of metal, the electricity was diminished so suddenly as to set the straws a vibrating, and it was some time before they stopt, for which reason the communication was made by a piece of moist wood, which, though it communicates the electricity of one vial to the other very quickly, did not do it so instantaneously as to make the straws vibrate much.

393] The electricity of the vial was found to waste very slowly, so that it could not be sensibly diminished during the small time spent in communicating the electricity from one vial to the other and reading off the divisions, so that no sensible error could proceed from that cause.

394] I tried the experiment before in the same manner, and with the same electrometers, except that the straws were not gilt, but only moistened with salt. It then seemed as if the force with which the balls of the electrometer were repelled with a given quantity of redundant fluid in the vial was to that with which they were repelled with only half that quantity in the vial as 4 to $\frac{9}{8}$.

As I suspected that this small difference from the theory was owing to the straws not conducting sufficiently readily, I gilt the straws, when, as was before shewn, the experiment agreed very well with theory.

It must be observed that if the straws do not conduct sufficiently readily, the balls of the electrometer will not be so strongly electrified

* [In a sentence which Cavendish has scored out in his MS. we read—]
The charge of the two vials together was found to be 2168 inches. The diameter of the rod *CD* was at a medium about $\frac{3}{4}$ of an inch. [This would make the computed charge of the rod 9·7 inches.—Ed.]

and will not separate so much as they ought to do, and in all probability the difference will be greater in the stronger degree of electricity, in which the electricity wastes much faster, than it is in the weaker, and will therefore diminish the degree of separation more in the stronger degree of electricity than in the weaker, and will therefore make the force with which the balls repel with the stronger degree of electricity appear to be less in proportion to that with which they repel with the weaker degree than it ought to be.

PHILOSOPHICAL TRANSACTIONS. VOL. 66, 1776, PART I, pp. 196–225

An account of some attempts to imitate the effects of the Torpedo by Electricity

Read Jan. 18, 1775.

{See Table of Contents at the beginning of this volume.}

395] Although the proofs brought by Mr Walsh*, that the phenomena of the torpedo are produced by electricity, are such as leave little room for doubt; yet it must be confessed, that there are some circumstances, which at first sight seem scarcely to be reconciled with this supposition. I propose, therefore, to examine whether these circumstances are really incompatible with such an opinion; and to give an account of some attempts to imitate the effects of this animal by electricity.

396] It appears from Mr Walsh's experiments, that the torpedo is not constantly electrical, but hath a power of throwing at pleasure a great quantity of electric fluid from one surface of those parts which he calls the electrical organs to the other; that is, from the upper surface to the lower, or from the lower to the upper, the experiments do not determine which; by which means a shock is produced in the body of a person who makes any part of the circuit which the fluid takes in its motion to restore the equilibrium.

397] One of the principal difficulties attending the supposition, that these phenomena are produced by electricity, is, that a shock may be perceived when the fish is held under water; and in other circumstances, where the electric fluid hath a much readier passage than through the person's body. To explain this, it must be considered, that when a jar is electrified, and any number of different circuits are made between its positive and negative side, some electricity will necessarily pass along each; but a greater quantity will pass through those in which it meets with less resistance, than those in which it meets with more. For instance, let a person take some yards of very fine wire, holding the end in each hand, and let him discharge the jar by touching the outside with one end of the wire, and the inside with the other; he will feel a shock, provided the jar is charged high enough; but less than if he had discharged it without

* [*Philosophical Transactions*, 1773, pp. 461–477. Of the Electric Property of the Torpedo. In a letter from John Walsh, Esq., F.R.S., to Benjamin Franklin, Esq., LL.D., F.R.S., &c. *Read* July 1, 1773.

holding the wire in his hands; which shews, that part of the electricity passes through his body, and part through the wire. Some electricians indeed seem to have supposed that the electric fluid passes only along the shortest and readiest circuit; but besides that such a supposition would be quite contrary to what is observed in all other fluids, it does not agree with experience. What seems to have led to this mistake is, that in discharging a jar by a wire held in both hands, as in the above-mentioned experiment, the person will feel no shock, unless either the wire is very long and slender, or the jar is very large and highly charged. The reason of which is, that metals conduct surprisingly better than the human body, or any other substance I am acquainted with; and consequently, unless the wire is very long and slender, the quantity of electricity which will pass through the person's body will bear so small a proportion to the whole, as not to give any sensible shock, unless the jar is very large and highly charged.

398] It appears from some experiments*, of which I propose shortly to lay an account before this Society, that iron wire conducts about 400 million times better than rain or distilled water; that is, the electricity meets with no more resistance in passing through a piece of iron wire 400,000,000 inches long, than through a column of water of the same diameter only one inch long. Sea water, or a solution of one part of salt in 30 of water, conducts 100 times, and a saturated solution of sea salt about 720 times, better than rain water.

399] To apply what hath been here said to the torpedo; suppose the fish by any means to convey in an instant a quantity of electricity through its electric organs, from the lower surface to the upper, so as to make the upper surface contain more than its natural quantity, and the lower less; this fluid will immediately flow back in all directions, part over the moist surface, and part through the substance of its body, supposing it to conduct electricity, as in all probability it does, till the equilibrium is restored: and if any person hath at the time one hand on the lower surface of the electric organs, and the other on the upper, part of the fluid will pass through his body. Moreover, if he hath one hand on one surface of an electric organ, and another on any other part of its body, for instance the tail, still some part of the fluid will pass through him, though much less than in the former case; for as part of the fluid, in its way from the upper surface of the organ to the lower, will go through the tail, some of that part will pass through the person's body. Some fluid also will pass through him, even though he does not touch either electric organ, but hath his hands on any two parts of the fishes body whatever, provided one of those parts is nearer to the upper surface of the electric organs than the other.

400] On the same principle, if the torpedo is immersed in water, the fluid will pass through the water in all directions, and that even to great distances from its body, as is represented in Fig. 1, where the full lines

* [Arts. 576, 577, 684, 687.]

represent the section of its body, and the dotted lines the direction of the electric fluid; but it must be observed, that the nearer any part of the water is to the fishes body, the greater quantity of fluid will pass through it. Moreover, if any person touches the fish in this situation, either with one hand on the upper surface of an electric organ, and the other on the lower, or in any other of those manners in which I supposed it to be touched when out of the water, some fluid will pass through his body; but evidently

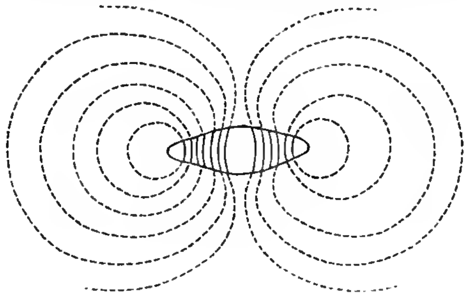


Fig. 1.

less than when the animal is held in the air, as a great proportion of the fluid will pass through the water: and even some fluid will pass through him, though he does not touch the fish at all; but only holds his hands in the water, provided one hand is nearer to the upper surface of the electric organs than the other.

401] The second difficulty is, that no one hath ever perceived the shock to be accompanied with any spark or light, or with the least degree of attraction or repulsion. With regard to this, it must be observed, that when a person receives a shock from the torpedo, he must have formed the circuit between its upper and lower surface before it begins to throw the electricity from one side to the other; for otherwise the fluid would be discharged over the surface of the fishes body before the circuit was completed, and consequently the person would receive no shock. The only way, therefore, by which any light or spark could be perceived, must be by making some interruption in the circuit. Now Mr Walsh found, that the shock would never pass through the least sensible space of air, or even through a small brass chain. This circumstance, therefore, does not seem inconsistent with the supposition that the phenomena of the torpedo are owing to electricity; for a large battery will give a considerable shock, though so weakly charged that the electricity will hardly pass through any sensible space of air; and the larger the battery is, the less will this space be. The principle on which this depends will appear from the following experiments.

402] I took several jars of different sizes, and connected them to the same prime conductor, and electrified them in a given degree, as shewn by a very exact electrometer; and then found how near the knobs of an instrument in the nature of Mr Lane's electrometer must be approached, before the jars would discharge themselves. I then electrified the same jars again in the same degree as before, and separated all of them from the conductor except one. It was found, that the distance to which the knobs must be approached to discharge this single jar was not sensibly less than the former. It was also found, that the divergence of the electrometer was the same after the removal of the jars as before, provided it was placed at a considerable distance from them: from which last circumstance, I think we may conclude, that the force with which the fluid endeavours to escape from the single jar is the same as from all the jars together*.

403] It appears, therefore, that the distance to which the spark will fly is not sensibly affected by the number or size of the jars, but depends only on the force with which they are electrified; that is, on the force with which the fluid endeavours to escape from them: consequently, a large jar, or a great number of jars, will give a greater shock than a small one, or a small number, electrified to such a degree, that the spark shall fly to the same distance; for it is well known, that a large jar, or a great number of jars, will give a greater shock than a small one, or a small number, electrified with the same force.

404] In trying this experiment, the jars were charged very weakly, insomuch that the distance to which the spark would fly was not more than the 20th of an inch. The electrometer † I used consisted of two straws, 10 inches long, hanging parallel to each other, and turning at one end on steel pins as centers, with cork balls about $\frac{1}{4}$ of an inch in diameter fixed on the other end. The way by which I estimated the divergence of these balls, was by seeing whether they appeared to coincide with parallel lines placed behind them at about 10 inches distance; taking care to hold my eye always at the same distance from the balls, and not less than thirty inches off. To make the straws conduct the better, they were gilded, which causes them to be much more regular in their effect. This electrometer is very accurate; but can be used only when the electricity is very weak. It would be easy, however, to make one on the same principle, which should be fit for measuring pretty strong electricity.

405] The instrument by which I found to what distance the spark would fly is represented in Fig. 2; it differs from Mr Lane's electrometer‡

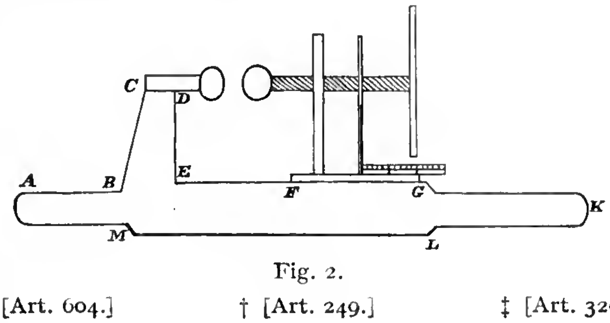


Fig. 2.

* [Art. 604.]   † [Art. 249.]   ‡ [Art. 329.]

no otherwise than in not being fixed to a jar, but made so as to be held in the hand. The part *ABCDEFGKLM* is of baked wood, the rest of brass; the part *GKL* being covered with tinfoil communicating with the brass work at *FG*; and the part *ABM* being also covered with a piece of tinfoil, communicating with the brass work at *CD*.

406] I next took four jars, all of the same size; electrified one of them to a given degree, as shewn by the electrometer; and tried the strength of the shock which it gave; and found also to what distance the spark would fly. I then took two of the jars, electrified them in the same degree as before, and communicated their electricity to the two remaining. The shock of these four jars united, was rather greater than that of the single jar; but the distance to which the spark would fly was only half as great*.

407] Hence it appears, that the spark from four jars, all of the same size, will not dart to quite half so great a distance as that from one of those jars electrified in such a degree as to give a shock of equal violence; and consequently the distance to which the spark will fly is inversely in a rather greater proportion than the square root of the number of jars, supposing them to be electrified in such a degree that the shock shall be of a given strength. It must be observed, that in the last mentioned experiment, the quantity of electric fluid which passed through my body was twice as great in taking the shock of the four jars, as in taking that of the single one; but the force with which it was impelled was evidently less, and I think we may conclude, was only half as great. If so, it appears that a given quantity of electricity, impelled through our body with a given force, produces a rather less shock than twice that quantity, impelled with half that force; and consequently, the strength of the shock depends rather more on the quantity of fluid which passes through our body, than on the force with which it is impelled.

408] That no one could ever perceive the shock to be accompanied with any attraction or repulsion, does not seem extraordinary; for as the electricity of the torpedo is dissipated by escaping through or over the surface of its body, the instant it is produced, a pair of pith balls suspended from any thing in contact with the animal will not have time to separate, nor will a fine thread hung near its body have time to move towards it, before the electricity is dissipated. Accordingly I have been informed by Dr Priestley, that in discharging a battery he never could find a pair of pith balls suspended from the discharging rod to separate. But, besides, there are scarce any pith balls so fine, as to separate when suspended from a battery so weakly electrified that its shock will not pass through a chain, as is the case with that of the torpedo.

409] In order to examine more accurately, how far the phenomena of the torpedo would agree with electricity, I endeavoured to imitate them

* [Arts. 573, 610, 613.]

by means of the following apparatus. *ABCFGDE*, Fig. 3, is a piece of wood, the part *ABCDE* of which is cut into the shape of the torpedo, and is $16\frac{3}{4}$ inches long from *A* to *D*, and $10\frac{3}{4}$ broad from *B* to *E*; the part *CFGD* is 40 inches long, and serves by way of handle. *MNmn* is a glass tube let into a groove cut in the wood. *Ww* is a piece of wire passing through the glass tube, and soldered at *W* to a thin piece of pewter *Rr*

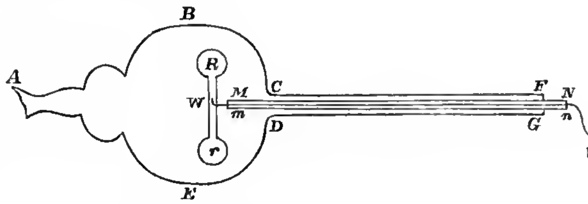


Fig. 3.

lying flat on the wood, and intended to represent the upper surface of the electric organs. On the other side of the wood there is placed such another glass tube, not represented in the figure, with a wire passing through it, and soldered to another piece of pewter of the same size and shape as *Rr*, intended to represent the lower surface of those organs. The whole part *ABCDE* is covered with a piece of sheep's skin leather.

410] In making experiments with this instrument, or artificial torpedo as I shall call it, after having kept it in water of about the same saltness as that of the sea, till thoroughly soaked, I fastened the end of one of the wires, that not represented in the drawing for example, to the negative side of a large battery, and when it was sufficiently charged, touched the positive side with the end of the wire *Ww*; by which means the battery was discharged through the torpedo: for as the wires were inclosed in

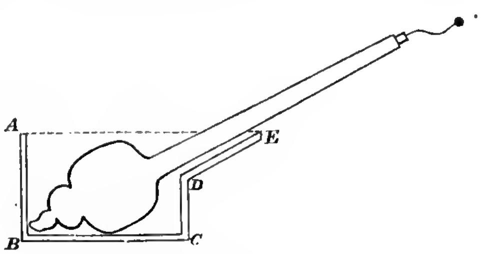


Fig. 4.

glass tubes, which extended about an inch beyond the end of the wood *FG*, no electricity could pass from the positive side of the battery to the negative, except by flowing along the wire *Ww* to the pewter *Rr*, and thence either through the substance of the wood, or along the wet leather, to the opposite piece of pewter, and thence along the other wire to the negative side. When I would receive a shock myself, I employed an assistant to charge the battery, and when my hands were in the proper position, to discharge it in the above mentioned manner by means of the wire *Ww*. In experiments with this torpedo under water, I made use of a wooden trough; and as the strength of the shock may, perhaps, depend in some measure on the size of the trough, and on the manner in which the torpedo lies in it, I have, in Fig. 4, given a vertical section of it; the torpedo being placed in the same situation as in the figure. *ABCDE* is the trough; the length *BC* is 19 inches; the depth *AB* is 14; and the breadth is 13; consequently, as the torpedo is two inches thick in the thickest part, there is about $5\frac{1}{2}$ inches distance between its sides and those of the trough.

411] The battery was composed of 49 jars, of extremely thin glass, disposed in 7 rows, and so contrived that I could use any number of rows I chose. The outsides of the jars were coated with tinfoil; but as it would have been very difficult to have coated the insides in that manner, they were filled with salt water. In a battery to answer the purpose for which this was intended, it is evidently necessary that the metals serving to make the communications between the different jars should be joined quite close: accordingly care was taken that the contacts should be made as perfect as possible. I find, by trial, that each row of the battery contains about $15\frac{3}{4}$ times as much electricity, when both are connected to the same prime conductor, as a plate of crown glass, the area of whose coating is 100 square inches, and whose thickness is $\frac{55}{1000}$ of an inch; that is, such that one square foot of it shall weigh 10 oz. 12 dwts.; and consequently, the whole battery contains about 110 times as much electricity as this plate*.

412] The way by which this was determined, and which, I think, is one of the easiest methods of comparing the quantity of electricity which different batteries will receive with the same degree of electrification, was this: First of all, supposing a jar or battery to be electrified till the balls of the above-mentioned electrometer separated to a given distance, I found how much they would separate when the quantity of electricity in that jar or battery was reduced to one-half. To do this, I took two jars, as nearly equal as possible, and electrified one of them till the balls separated to a given degree, and then communicated its electricity to the other; and observed to what distance the balls separated after this communication. It is plain, that if the jars were exactly equal, this would be the distance sought for; as in that case the quantity of electricity in the first jar would be just half as much after the communication as before; but as I could not be sure that they were exactly equal, I repeated the experi-

* I find, by experiment, that the quantity of electricity which coated glass of different shapes and sizes will receive with the same degree of electrification, is directly as the area of the coating, and inversely as the thickness of the glass; whence the proportion which the quantity of electricity in this battery bears to that in a glass or jar of any other size, may easily be computed. [See Art. 584. The charge of the first row of jars was 64,538, and that of the whole battery about 481,000 inches of electricity.]

ment by electrifying the second jar, communicating its electricity to the first, and observing how far the balls separated; the mean between these two distances will evidently be the degree of separation sought, though the jars were not of the same size. Having found this, I electrified one row of the battery till the balls separated to the first distance, and repeatedly communicated its electricity to the plate of coated crown glass, taking care to discharge the plate each time before the communication was made, till it appeared by the electrometer, that the quantity of electricity in that row was reduced to one-half. I found it necessary to do this between 11 or 12 times, or $11\frac{1}{4}$ times as I estimate it. Whence the quantity of electric fluid in the row may be thus determined.

413] Let the quantity in the plate be to that in the row as x to 1; it is plain, that the electricity in the row will be diminished each time it is communicated to the plate, in the proportion of 1 to $1 + x$, and consequently after being communicated $11\frac{1}{4}$ times will be reduced in the proportion of 1 to $(1 + x)^{11\frac{1}{4}}$; therefore, $(1 + x)^{11\frac{1}{4}} = 2$; and $1 + x = \overline{2}|^{\frac{1}{11\frac{1}{4}}}$. Whence the value of x may easily be found by logarithms. But the readiest way of computing it, and which is exact enough for the purpose, is this: multiply the number of times which you communicated the electricity of the row to the plate, by 1,444; and from the product subtract the fraction $\frac{1}{2}$; the remainder is equal to $\frac{1}{x}$, or the number of times by which the electricity in the row exceeds that in the plate*.

414] The way by which I estimated the strength of the charge given to the battery, was taking a certain number of jars, and electrifying them till the balls of the electrometer separated to a given distance, and then communicating their electricity to the battery. This method proved very convenient; for by using always the same jars, I was sure to give always the same charge with great exactness; and by varying the number and size of the jars, I could vary the charge at pleasure, and besides could estimate pretty nearly the proportion of the different charges to each other. It was also the only convenient method which occurred to me; for I could not have done it conveniently by charging the whole battery till an electrometer suspended from it separated to a given distance; because in most of the experiments the electricity was so weak, that a pair of fine pith balls suspended from the battery would separate only to a very small distance; and counting the number of revolutions of the electrical machine is a very fallacious method.

415] I found, upon trial†, that though a shock might be procured from this artificial torpedo, while held under water, yet there was too great a disproportion between its strength, when received this way, and

* [Arts. 441, 582.] † [Art. 596.]

in air; for if I placed one hand on the upper, and the other on the lower surface of the electric organs, and gave such a charge to the battery, that the shock, when received in air, was as strong as, I believe, that of the real torpedo commonly is; it was but just perceptible when received under water. By increasing the charge, indeed, it became considerable; but then this charge would have given a much greater shock out of water than the torpedo commonly does. The water used in this experiment was of about the same degree of saltness as that of the sea; that being the natural element of the torpedo, and what Mr Walsh made his experiments with. It was composed of one part of common salt dissolved in 30 of water, which is the proportion of salt usually said to be contained in sea water. It appeared also, on examination, to conduct electricity not sensibly better or worse than some sea water procured from a mineral water warehouse. It is remarkable, that if I used fresh water instead of salt, the shock seemed very little weaker, when received under water than out; which not only confirms what was before said, that salt water conducts much better than fresh; but, I think, shews, that the human body is also a much better conductor than fresh water: for otherwise the shock must have been much weaker when received under fresh water than in air.

416] As there appeared to be too great a disproportion between the strength of the shock in water and in air, I made another torpedo*, exactly like the former, except that the part *ABCDE* instead of wood was made of several pieces of thick leather, such as is used for the soles of shoes, fastened one over the other, and cut into the proper shape; the pieces of pewter being fixed on the surface of this, as they were on the wood, and the whole covered with sheep skin like the other. As the leather, when thoroughly soaked with salt water, would suffer the electricity to pass through it very freely, I was in hopes that I should find less difference between the strength of the shock in water and out of it, with this than with the other.

417] For suppose that in receiving the shock of the former torpedo under water, the quantity of electricity which passed through the wood and leather of the torpedo, through my body, and through the water, were to each other as T, B, and W†; the quantity of electricity which would pass through my body, when the shock was received under water, would be to that which would pass through it, when the shock was received out of water, as $\frac{B}{B+T+W}$ to $\frac{B}{B+T}$; as in the first case, the quantity which would pass through my body would be the $\frac{B}{B+T+W}$ part of the whole; and in the latter the $\frac{B}{B+T}$ part. Suppose now, that the latter torpedo conducts N times better than the former; and consequently, that

* [Arts. 599, 600.]

† [Arts. 597, 598.]

in receiving its shock under water, the quantity of electricity which passes through the torpedo, through my body, and through the water, are to each other as NT, B, and W; the quantity of electricity which will now pass through my body, when the shock is received under water, and out of water, will be to each other as $\frac{B}{B+NT+W}$ to $\frac{B}{B+NT}$; which two quantities differ from each other in a less proportion than $\frac{B}{B+T+W}$ and $\frac{B}{B+T}$: consequently, the readier the body of the torpedo conducts, the greater charge will it require to give the same shock, either in water or out of it; but the less will be the difference between the strength of the two shocks. It should be observed, that this alteration, so far from making it less resembling the real torpedo, in all probability makes it more so; for I see no reason to think, that the real torpedo is a worse conductor of electricity than other animal bodies; and the human body is at least as good, if not a much better conductor than this new torpedo.

418] The event answered my expectation; for it required about three times as great a charge of the battery, to give the same shock in air, with this new torpedo as with the former; and the difference between its strength when received under water and out of it, was much less than before, and perhaps not greater than in the real torpedo. There is, however, a considerable difference between the feel of it under water and in air. In air it is felt chiefly in the elbows; whereas, under water, it is felt chiefly in the hands, and the sensation is sharper and more disagreeable. The same kind of shock, only weaker, was felt if, instead of touching the sides, I held my hands under water at two or three inches distance from it.

419] It is remarkable, that I felt a shock of the same kind, and nearly of the same strength, if I touched the torpedo under water with only one hand, as with both. Some gentlemen* who repeated the experiment with me thought it was rather stronger. This shews, that the shock under water is produced chiefly by the electricity running through one's hand from one part to the other; and that but a small part passes through one's body from one hand to the other. The truth of this will appear with more certainty from the following circumstance; namely, that if I held a piece of metal, a large spoon for instance, in each hand, and touched the torpedo with them instead of my hands, it gave me not the least shock when immersed in water; though when held in air, it affected me as strongly if I touched it with the spoons as with my hands. On increasing the charge, indeed, its effect became sensible: and as well as I could judge, the battery required to be charged about twelve times as high to give the same shock when the torpedo was touched with the spoons under water as out of it.

* [See Art. 601, 27 May, 1775. Mr Ronayne, Mr Hunter, Dr Priestley, Mr Lane, Mr N[airne].]

It must be observed, that in trying this experiment, as my hands were out of water, I could be affected only by that part of the fluid which passed through my body from one hand to the other.

420] The following experiments were made with the torpedo in air. If I stood on an electric stool, and touched either surface of the electric organs with one hand only, I felt a shock in that hand; but scarcely so strong as when touching it in the same manner under water. If I laid a hand on one surface of the electric organs, and with the other touched the tail, I felt a shock; but much weaker than when touching it in the usual manner; that is, with one hand on the upper surface of those organs, and the other on the lower. If I laid a thumb on either surface of an electric organ, and a finger of the same hand on any part of the body, except on or very near the same surface of the organs, I felt a small shock.

In all the foregoing experiments, the battery was charged to the same degree, except where the contrary is expressed: they all seem to agree very well with Mr Walsh's experiments.

421] Mr Walsh found, that if he inclosed a torpedo in a flat basket, open at the top, and immersed it in water to the depth of three inches, and while the animal was in that situation, touched its upper surface with an iron bolt held in one hand, while the other hand was dipped into the water at some distance, he felt a shock in both of them. I accordingly tried the same experiment with the artificial torpedo; and if the battery was charged about six times as high as usual, received a small shock in each hand*. No sensible difference could be perceived in the strength, whether the torpedo was inclosed in the basket or not. The trough in which this experiment was tried was 36 inches lorg, 14½ broad, and 16 deep; and the distance of that hand which was immersed in the water from the electric organs of the torpedo, was about 14 inches. As it was found necessary to charge the battery so much higher than usual, in order to receive a shock, it follows, that unless the fish with which Mr Walsh tried this experiment were remarkably vigorous, there is still too great a disproportion between the strength of the shock of the artificial torpedo when received under water and out of it. If this is the case, the fault might evidently be remedied by making it of some substance which conducts electricity better than leather.

422] When the torpedo happens to be left on shore by the retreat of the tide, it loosens the sands by flapping its fins, till its whole body, except the spiracles, is buried; and it is said to happen sometimes, that a person accidentally treading on it in that situation, with naked feet, is thrown down by it. I therefore filled a box, 32 inches long and 22 broad, with

* As well as I could judge, the battery required to be charged about 16 or 20 times as high, to give a shock of the same strength when received this way as when received in the usual manner with the torpedo out of water. [Art. 615.]

sand, thoroughly soaked with salt water, to the depth of four inches, and placed the torpedo in it, intirely covered with the sand, except the upper part of its convex surface, and laid one hand on its electrical organs, and the other on the wet sand about 16 inches from it. I felt a shock, but rather weak; and as well as I could judge, as strong as if the battery had been charged half as high, and the shock received in the usual way*.

423] I next took two thick pieces of that sort of leather which is used for the soles of shoes, about the size of the palm of my hand; and having previously prepared them by steeping in salt water for a week, and then pressing out as much of the water as would drain off easily, repeated the experiment with these leathers placed under my hands. The shock was weaker than before, and about as strong as if received in the usual way with the battery charged one-third part as high. As it would have been troublesome to have trod on the torpedo and sand, I chose this way of trying the experiment. The pieces of leather were intended to represent shoes, and in all probability the shoes of persons who walk much on the wet sand will conduct electricity as well as these leathers. I think it likely, therefore, that a person treading in this manner on a torpedo, even with shoes on, but more so without, may be thrown down, without any extraordinary exertion of the animal's force, considering how much the effect of the shock would be aided by the surprise.

424] One of the fishermen that Mr Walsh employed assured him, that he always knew when he had a torpedo in his net, by the shocks he received while the fish was at several feet distance; in particular, he said, that in drawing in his nets with one of the largest in them, he received a shock when the fish was at twelve feet distance, and two or three more before he got it into his boat. His boat was afloat in the water, and he drew in the nets with both hands. It is likely, that the fisherman might magnify the distance; but, I think, he may so far be believed, as that he felt the shock before the torpedo was drawn out of water. This is the most extraordinary instance I know of the power of the torpedo; but I think seems not incompatible with the supposition of its being owing to electricity; for there can be little doubt, but that some electricity would pass through the net to the man's hands, and from thence through his body and the bottom of the boat, which in all probability was thoroughly soaked with water, and perhaps leaky, to the water under the boat: the quantity of electric fluid, however, taking this circuit, would most likely bear so small a proportion to the whole, that this effect cannot be accounted for, without supposing the fish to exert at that time a surprizingly greater force than what it usually does.

425] Hitherto, I think, the effects of this artificial torpedo agree very well with those of the natural one. I now proceed to consider the circum-

* [Art. 608.]

stance of the shock's not being able to pass through any sensible space of air. In all my experiments on this head, I used the first torpedo, or that made of wood; for as it is not necessary to charge the battery more than one-third part as high to give the same shock with this as with the other, the experiments were more likely to succeed, and the conclusions to be drawn from them would be scarcely less convincing: for I find, that five or six rows of my battery will give as great a shock with the leathern torpedo, as one row electrified to the same degree will with the wooden one; consequently, if with the wooden torpedo and my whole battery, I can give a shock of a sufficient strength, which yet will not pass through a chain of a given number of links, there can be no doubt, but that, if my battery was five or six times as large, I should be able to do the same thing with the leathern torpedo.

426] I covered a piece of sealing wax on one side with a slip of tinfoil, and holding it in one hand, touched an electrical organ of the torpedo with the end of it, while my other hand was applied to the opposite surface of the same organ. The shock passed freely, being conducted by the tinfoil; but if I made, with a penknife, as small a separation in the tinfoil as possible, so as to be sure that it was actually separated, the shock would not pass, conformably to what Mr Walsh observed of the torpedo.

427] I tried the experiment in the same manner with the Lane's electrometer described in Art. 405, and found that the shock would not pass, unless the knobs were brought so near together as to require the assistance of a magnifying glass to be sure that they did not touch.

428] I took a chain of small brass wire, and holding it in one hand, let the lowest link lie on the upper surface of an electric organ, while my other hand was applied to the opposite surface. The event was, that if the link, held in my hand, was the fifth or sixth from the bottom, and consequently, that the electricity had only four or five links to pass through besides that in my hand, I received a shock; so that the electricity was able to force its way through four or five intervals of the links, but not more. One gentleman, indeed, found it not to pass through a single interval; but in all probability the link which lay on the torpedo happened to bear more loosely than usual against that in his hand. If instead of this chain I used one composed of thicker wire, the shock would pass through a great number of links; but I did not count how many. It must be observed, that the principal resistance to the passage of the electrical fluid is formed by the intervals of the lower links of the chain; for as the upper are stretched by a greater weight, and therefore pressed closer together, they make less resistance. Consequently the force required to make the shock pass through any number of intervals, is not twice as great as would be necessary to make it pass through half the number. For the same reason it passes easier through a chain consisting of heavy links than of light ones.

429] Whenever the electricity passed through the chain, a small light was visible, provided the room was quite dark. This, however, affords no argument for supposing that the phenomena of the torpedo are not owing to electricity; for its shock has never been known to pass through a chain or any other interruption in the circuit; and consequently, it is impossible that any light should have been seen.

430] In all these experiments, the battery was charged to the same degree; namely, such that the shock was nearly of the same strength as that of the leathern torpedo, and which I am inclined to think, from my conversation with Mr Walsh, may be considered as about the medium strength of those of a real one of the same size as this. It was nearly equal to that of the plate of crown glass in Art. 411, electrified to such a degree as to discharge itself when the knobs of a Lane's electrometer were at ,0115 inches distance; whence a person, used to electrical experiments, may ascertain its strength*. The way I tried it was by holding the Lane's electrometer in one hand, with the end resting on the upper surface of the plate, and touching the lower surface with the other hand, while an assistant charged the plate by its upper side till it discharged itself through the electrometer and my body. There is, however, a very sensible difference between the sensation excited by a small jar or plate of glass like this, and by a large battery electrified so weakly that the shock shall be of the same strength; the former being sharper and more disagreeable. Mr Walsh took notice of this difference; and said, that the artificial torpedo produced just the same sensation as the real one.

431] As it appeared, that a shock of this strength would pass through a few intervals of the links of the chain, I tried what a smaller would do. If the battery was charged only to a fourth or fifth part of its usual height, the shock would not pass through a single interval; but then it was very weak, even when received through a piece of brass wire, without any link in it. This chain was quite clean and very little tarnished; the lowest link was larger than the rest, and weighed about eight grains. If I used a chain of the same kind, the wire of which, though pretty clean, was grown brown by being exposed to the air, the shock would not pass through a single interval, with the battery charged to about one-third or one-half its usual strength.

432] It appears, that in this respect the artificial torpedo does not completely imitate the effects of the real one, though it approaches near to it; for the shock of the former, when not stronger than that of the latter frequently is, will pass through four or five intervals of the links of a chain; whereas the real torpedo was never known to force his through a single interval. But, I think, this by no means shews, that the phenomena

* [Charge of plate = 4100 inches of electricity = 5207 centimetres capacity. Electromotive force = 5·5. See Note 10.]

of the torpedo are not produced by electricity; but only that the battery I used is not large enough. For we may safely conclude, from the experiments mentioned in Arts. 402, 406, 407, that the greater the battery is, the less space of air, or the fewer links of a chain, will a shock of a given strength pass across. For greater certainty, however, I tried, whether if the whole battery and a single row of it were successively charged to such a degree, that the shock of each should be of the same strength when received through the torpedo in the usual manner, that of the whole battery would be unable to pass through so many links of a chain as that of a single row*. In order to which I made the following machine†.

433] *GM*, Fig. 5, is a piece of dry wood; *Ff*, *Ee*, *Dd*, *Cc*, *Bb*, and *Aa*, are pieces of brass wire fastened to it, and turned up at bottom into the

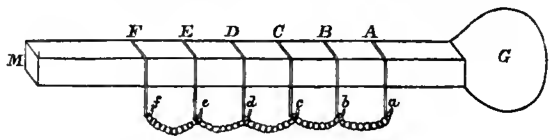


Fig. 5.

form of a hook, on which is hung a small brass chain, as in the figure, so as to form five loops, each loop consisting of five links; the part *G* is covered with tinfoil, which is made to communicate with the wire *Aa*. If I held this piece of wood in one hand, with my thumb on either of the wires *Ff*, *Ee*, &c. and applied the part *G* to one surface of an electric organ, while with a spoon, held in the other hand, I touched the opposite surface, I received a shock, provided the battery was charged high enough, the electricity passing through all that part of the chain between *Aa*, and my thumb; so that I could make the shock pass through more or fewer loops, according to which wire my thumb was placed on; but if the charge was too weak to force a passage through the chain, I felt no shock, as the wood was too dry to convey any sensible quantity of electricity. The event of the experiment was, that if I charged the whole battery to such a degree that the shock would but just pass through two loops of the machine, and then charged a single row to such a degree as appeared, on trial, just sufficient to give a shock of the same strength as the former, it passed through all five loops; whether it would have passed through more I cannot tell. If, on the other hand, I gave such a charge to the whole battery, and also to the single row, as was just sufficient to force a passage through two loops of the chain, the shock with the whole battery was much stronger than that with the single row.

434] It must be observed, that in the foregoing machine, each loop consisted of the same number of links, and the links of each loop were stretched by the same weight; so that it required no more force to impel

* The battery, as was before said, was divided into seven rows, each of which could be used separately.

† [Arts. 605, 607.]

the electricity through one loop than another, which was my reason for using this machine rather than a plain chain. Considerable irregularities occurred in trying the above experiments, and indeed all those with a chain; for it frequently happened, that the shock would not pass with the battery charged to a certain degree, when perhaps a minute after, it would pass with not more than three-fourths of the charge. The irregularity, however, was not so great but that, I think, I may be certain of the truth of the foregoing facts; especially as the experiments were repeated several times. The uncertainty was at least as great in the experiments with Lane's electrometer, when the knobs were brought so close together, as is necessary in experiments of this kind.

435] It appears therefore, that if the whole battery, and a single row of it, are both charged in such a degree as to give a shock of the same strength, the shock with the whole battery will pass through fewer loops of the chain than that with the single row; so that, I think, there can be no doubt, but that if the battery had been large enough, I should have been able to give a shock of the usual strength, which yet would not have passed through a single interval of the links of a chain.

436] On the whole, I think, there seems nothing in the phenomena of the torpedo at all incompatible with electricity; but to make a compleat imitation of them, would require a battery much larger than mine. It may be asked, where can such a battery be placed within the torpedo? I answer, perhaps it is not necessary that there should be anything analogous to a battery within it. The case is this; it appears, that the quantity of electric fluid, transferred from one side of the torpedo to the other, must be extremely great; for otherwise it could not give a shock, considering that the force with which it is impelled is so small as not to make it pass through any sensible space of air. Now if such a quantity of fluid was to be transferred at once from one side to the other, the force with which it would endeavour to escape would be extremely great, and sufficient to make it dart through the air to a great distance, unless there was something within it analogous to a very large battery. But if we suppose, that the fluid is gradually transferred through the electrical organs, from one side to the other, at the same time that it is returning back over the surface, and through the substance, of the rest of the body; so that the quantity of fluid on either side is during the whole time very little greater or less than what is naturally contained in it; then it is possible, that a very great quantity of fluid may be transferred from one side to the other, and yet the force with which it is impelled be not sufficient to force it through a single interval of the links of a chain. There seems, however, to be room in the fish for a battery of a sufficient size; for Mr Hunter* has shewn, that each of the prismatical columns of which

* "Anatomical observations on the Torpedo." By John Hunter, F.R.S. *Phil. Trans.* LXIII (1773), p. 485. See Art. 614.

the electrical organ is composed, is divided into a great number of partitions by fine membranes, the thickness of each partition being about the 150th part of an inch; but the thickness of the membranes which form them is, as he informs me, much less. The bulk of the two organs together in a fish $10\frac{1}{3}$ inches broad, that is, of the same size as the artificial torpedos, seems to be about $24\frac{1}{2}$ cubic inches; and therefore the sum of the areas of all the partitions is about 3700 square inches. Now 3700 square inches of coated glass $\frac{1}{150}$ of an inch thick will receive as much electricity as 30,500 square inches ,055 of an inch thick*; that is, 305 times as much as the plate of crown glass mentioned in Art. 411, or about $2\frac{3}{4}$ times as much as my battery, supposing both to be electrified by the same conductor; and if the glass is five times as thin, which perhaps is not thinner than the membranes which form the partitions, it will contain five times as much electricity, or near fourteen times as [much as] my battery.

437] It was found, both by Dr Williamson† and by a committee appointed by the Philosophical Society of Pennsylvania, that the shock of the *Gymnotus* would sometimes pass through a chain, though they never perceived any light. I therefore took the same chain which I used in the foregoing experiments, consisting of 25 links, and suspended it by its extremities from the extreme hooks of the machine described in Art. 433, and applying the end of the machine to the negative side of the battery, touched the positive side with a piece of metal held in the other hand, so as to receive the shock through the chain without its passing through the torpedo; the battery being charged to such a degree that the shock was considerably stronger than what I usually felt in the foregoing experiments. I found that if the chain was not stretched by an additional weight, the shock did not pass at all: If it was stretched by hanging a weight of seven pennyweights to the middle link, it passed, and a light was visible between some of the links; but if fourteen pennyweights were hung on, the shock passed without my being able to perceive the least light, though the room was quite dark; the experiment being tried at night, and the candle removed before the battery was discharged‡. It appears, therefore, that if in the experiments made by these gentlemen the shock never passed, except when the chain was somewhat tense, which in all probability was the case, the circumstance of their not having perceived any light is by no means repugnant to the supposition that the shock is produced by electricity§.

* Vide note in p. 200.

† "Experiments and Observations on the Gymnotus Electricus, or Electrical Eel." By Hugh Williamson, M.D. Communicated by John Walsh, Esq., F.R.S. *Phil. Trans.* LXV (1775), p. 94.

‡ [Art. 613.]

§ [See Note 29, p. 419 and Introduction.]

EXPERIMENTS, 1771

{The Journal of Experiments, of which a formal description is contained *ante* in Art. 217 onward. From MS. N°. 12. See Table of Contents at the beginning of this volume. The footnotes refer back to the formal account of results.}

1st Night*.

438] East plate neg[ative] standing east and west. West plate pos[itive] north and south. East plate touched perpend[icularly] by wire near midd[le]. West wire bearing against north side of west plate.

East	West	
10½	12	a small matter positive.
12	10½	nearly same perhaps rather less negative.
12	10	a good deal more.

East plate touched flat near west side.

12	10½	separ[ated] very little, scarce enough to say whether positive or negative.
10½	12	I thought rather more pos.

Position of east and west plates reversed. Plates touched by wires as last time.

10½	12	Seemed to separate rather more than before positive.
12	10½	did but just separate.

439]

2nd Night.

West plate positive	East plate negative	Both plates east and west, wires straight.
10½		Tin, pasteboard, and tinfoil, each 12 inches; separated a little and equally negative. With paper of 12 inches, in one or two first trials it seemed to separate much the same. Afterwards it did not separate at all, owing, as was supposed, to its being too dry to conduct well, but after being moistened it seemed to separate like the rest.
13½		The same things being tried, the corks separated more than before and were positive, and I believe pretty equally.

* [Probably the first trials of the apparatus described in Art. 240.]

440] 3RD NIGHT.

West pos.	East neg.	
10½	12	Separated visibly, I guess about 1 diameter.
—	11¾	Seemed to separate rather less.
—	11½	Scarcely separated.
	paper of 12	Separated much the same as tin of 12.
12	10½	Nearly the same as in first experiment but of [the] 2 separated rather more.
11¾	—	
11½	—	
13½	12	Nearly the same as in first experiment.
	11¾	Seemed as if it was rather more.
	11½	Sensibly more than with 12.

From the two other nights' experiments it seemed as if the positive bottle electrified the plates sensibly stronger than the negative one: why there was not the same difference this night I cannot tell.

Plates east and west. Wires straight.

441] Two pair of large corks were made, each of which was found to separate with the same force. The weight of one pair of them was then made four times as great by the addition of lead to them.

The quantity of electricity in 3rd made vial was then compared by means of these corks with that of a glass plate with circular coating 2·4 inches in diameter and about ·06 thick, by touching the glass 8 or 9 times the electricity was reduced from strength requisite to make heavy corks separate to that requisite to make light corks separate, or was reduced to ½, therefore the vial should contain 12 times as much electricity as the glass plate and wire by which communication was made, which was about 12 inches long*.

442] Three coated plates were made

	C	D	F
Thickness	·06031	·05908	·05914 inches.
Diameter of coating	1·82	1·79	1·785
Therefore square of diameter of coating by thickness, or computed power of plate	54·92	54·23	53·88

Mean 54·34. (D is cased with cement.)

A circular coating 5·39 inches diameter was made to thick plate in place where its thickness seemed ·178, therefore its computed power is equal to the sum of foregoing three plates. The proportion of thickness to diameter is nearly the same.

Two sliding coated plates were made for trying the foregoing, the trial plates being electrified negatively, the others positively.

* [See Arts. 413, 582.]

	Breadth of trial plate	
C	12	separated pos.
D	—	rather more.
D	13	scarce at all.
F	12	same as C.
C	17	separate neg.
C	16	do.
C	15	not at all.
D	16	scarce sensibly.
D	17	about as much as C at 16.
F	17	about as much as C.

Therefore F seems to contain about as much electricity as C, and D to contain about $\frac{1}{18}$ more.

443] The three foregoing plates

	Large plate		No.
placed close together	23	separated pos.	1
placed as far asunder as possible	23	a trifle more	2
The above mentioned large plate	23	rather less than No. 1	3
[Art. 442]	21	same as No. 1.	
	20	same as No. 2.	
	30	separated a very little neg.	

444] Three coated plates were made on thick plate each 1·8 inches diameter, the mean thickness of glass being supposed ·18, therefore the computed power of all three together = 54.

All 3 plates together	4	separated pos.
	5	did not separate.
	11	sep. neg.
	10	did not separate.
With C	12	sep. neg.
	11	did not.
	4	sep. neg.
	5	did not.

N.B. The breadth of the sliding plate is not known.

445] Small sliding plate not drawn out 14 × 9·4.
Large 19 × 13.

Globe hung on silk strings negative.
Sliding plates on waxed glass positive.

			[Equivalent*]
Globe—plate 19 ×	13	did not separate	15·7
	14	doubtful	16·3
	15	separated pos.	16·9
	16	seemed rather more	17·4
Globe—plate 14 ×	10·4	separated neg.	12
	11·4	doubtful	12·6
	12·4	did not separate	13·2

* [This column gives the side of a square equivalent to the trial plate. See Art. 465.]

Pasteboard circle 19·4 inches diameter hung on silk strings.

			[Equivalent]
Circle—plate 19 ×	14	did not separate	16·3
	15	did rather doubtful	16·9
	16	did very little	17·4
	17	did more	18·
Circle—plate 14 ×	12·7	did	13·2
	13·4	did rather doubtful	13·7
	14·4	did not	14·2*

With circle 1·8 inches diameter on glass ·18 thick it separated a little negatively with plate 19 × 19, and would most likely not separate at 19 × 21 or 19 × 22 = 20 or 20½. Therefore quantity of electricity therein most likely is to that of globe as 20·2 to 12·4 or as 10 : 6.

446] Thickness of double plate of glass at centre of circle = ·285. Diameter of coating = 1·75.

Being tried against small plate not drawn out, separated considerably positive, therefore quantity of electricity therein might perhaps be to that in globe as 11 to 18, and therefore its actual power would be to that of thick plate as 6·6 to 18. The computed power is to that of thick plate as 10·8 to 18.

A coating 1·45 inches diameter was made on thick plate where the thickness is supposed = ·168, therefore computed power = 12·5. This being tried against sliding plates was as follows:

Small sliding plate	Equivalent		Large sliding plate		Equivalent
3	13·2	separated negative	2	did not sep.	16·9
4	13·7	separated	3	doubtful	17·4
5	14·2	did not	4	separ. pos.	18

therefore quantity of electricity therein seems to be to that of globe as 13·7 to 12·6, or 17·4 to 16·3, *id est* as 14 to 13,

therefore actual power = 11·6

In thick plate 1·8 diam., $\frac{\text{diam. plate}}{\text{thickness}} = 10$

do. 1·45 · 8·1

double plate 6·14

TRIALS OF WIRES.

447] The wires placed horizontally and parallel to each other, one end supported by silk, the other by waxed glass.

The trial wire consisted of iron wires ·14 thick sliding on each other, supported in [the] same manner.

* [The charges of the globe and the circle of 19·4 inches appear from these numbers to be as 28·9 : 30·7. The diameter of the tin circle, 18·5, was probably calculated from these experiments so that its charge might be equal to that of the globe. The correct diameter would have been 19 inches.]

Single wire ·19 inch thick, 96 inches long.

Trial wire drawn out	8 inches	separated neg.
	$10\frac{1}{2}$	did not.
	32	did not.
	34	separated very little pos.

Two wires ·1 inch thick, 48 inches long, placed 36 inches asunder.

Trial wire drawn out	24 inches	separated pos.
	22	did not.
	0	sep. very little neg.
	2	did not.

The same wires at 18 inches distance.

	$17\frac{1}{2}$	sep. pos. rather doubtful.
	18	did.
	16	did.
	$13\frac{1}{2}$	did not.

By these it should [seem] as if trial wire required to be drawn out 9 less with the wires at 36 inches distance than with single wire, and 17 less with two wires at 18 inches, whence I should suppose that [the quantity of] el[ectricity] in these three cases was as 96, 87 and 79.

The trial wire not drawn out was 70 inches, but the straight part of it was only $51\frac{1}{2}$.

448] Wires of half that length tried in the same manner with a shorter trial wire.

Two wires ·1 thick, 24 long, at 18 inches distance.

Trial wire drawn out	1 inch	sep. neg.
	3	very little.
	5	rather doubtful.
	7	did not.
	12	did not.
	14	sep. pos. very little.

The same at 36 inches distance.

[trial wire] at	20	sep. pos.
	18	doubtful.
	16	did not.
	11	did not.
	9	did not.
	7	did a good deal.

Wire 48 inches long, touched by end of touching wire.

[trial wire] at	9	did not.
	7	sep. neg.
	20	did.
	18	did not.

Same wire touched by middle of touching wire.

18	doubtful.
20	did.
9	did not.
7	doubtful.
5	did.

449] From these experiments the quantity of electricity in

long wire touched at end	should	96
... ... middle	seem	94
short wires 36 dist.	to	96
do. 18 —	be as	87

450] Experiments to determine whether the quant. el. in the large circle was the same whether it was supported on waxed glass* or on silk strings, the trial plates, which were of wood covered with tinfoil, being supported on waxed glass, the large trial plate drawn out to *n* inches being expressed by $L - n$, the small ditto by $S - n$.

Large circle supported on silk strings.

$L - 5$ sep. pos. very sensibly if I staid some time before letting down the wires, but scarce sensibly if I did not.

$L - 4$ seemed to separate, but rather doubtful if I staid, but not if I did not.

$S - 5$ sep. neg. if I did not stay, but not if I did.

$L - 5$ tried again, sep. very little whether I staid or not.

The circle supported on waxed glass.

$L - 5$ sep. very little whether I staid or not.

$S - 5$ sep. very little whether I staid or not.

From these experiments there seems no reason to think that there is any sensible difference in the quantity of electricity whether the circle is supported on silk or on waxed glass. I believe the air was moderately but not very dry when these experiments were tried. The next experiment was made the same night.

451] Experiment to determine whether quantity of electricity in coated glass bears the same proportion to that in a non-electric body whether electrification is strong or weak†.

Two pair of corks were made; each separated with rather a less degree of electrification than those used in former experiments. Some lead was then added to those of one pair, so as to double their weight and consequently to make them require 2ce the force to make them separate.

The plate of glass used was the double plate called A in the following experiments, but with coating 1·78 inches diameter.

Tried with light corks.

$L - 3$ sep. a little pos.

$S - 4$ as much neg.

* [See Art. 255.] † [See Art. 355.]

Tried with heavy corks.

L − 2 separated pos.
S − 5¾ as much neg.

If these experiments could be depended on as perfectly exact the coated plate should contain $\frac{1}{48}$th part more electricity in proportion when electrified with heavy corks than with light, but this difference is much too small to be depended on.

452] Comparison of two tin circles* 9·3 inches diameter with one of 18·5, the tin plates supported on waxed glass and touched in the same manner as wires, the trial plates supported on silk strings.

The two circles at 36 in. distance.

		Side of square equivalent to trial plate.
S − 1	sep. very little neg.	11·26
S − 2	did not	
L − 1	sep. very little	15·03
L − ¼	doubtful	

Large circle touched by middle of touching wire.

L − 2	sep. very little pos.	15·57
S − 1½	sep. very little neg.	11·83

Do. circle touched by extremity of touching wire.

S − 3½	very little neg.	12·62
L − 4	very little pos.	16·64

Small plates at 36 inches distance tried again.
sep. very little with L − 1, which is the same as before.

Small plates at 24 inches.

S − 7 very little pos. equivalent to 14·26.
Do. at 18 inches S − 5½ very little pos. ...... 13·55.

453] A brass wire†, 72 inches long and ·19 thick, was then tried, touched by middle of touching wire.

L − 2 sep. pos. 15·57
S − 2½ very little neg. 12·07

454] From these experiments it should seem as if el[ectricity] in

Large circle touched at extremity	were as	14·63
... ... at middle		13·55
Two small circles at 36 inches		13·15
do. at 24		12·26
do. at 18		11·55

If the two circles were placed at the same distance from each other in the

* [Art. 273 and Notes 11 and 21.]

† [Art. 279.]

same manner as in coated plates, and were electrified by wires touching their centers perpendicularly, the quantity of electricity should be

Large circle	14·02
Two at 36	13·15
24	12·72
18	12·28

The quant[ity of] el[ectricity] in the wire 72 inches long and ·19 thick seems to be nearly equal to that in the circle of 18·5 inches. Therefore if we suppose quantity of electricity in a cylinder to be proportional to its length divided by the logarithm

of $\frac{\text{length}}{\text{thickness}}$,	quantity of electricity in cylinder is to that in globe whose diameter = length of cylinder as	·4266	—	·982	—
of $\frac{\text{length}}{\frac{1}{2}\text{ thickness}}$,		·4761 to tab. log.	— or as	1·096 to N. log.	—
of $\frac{\text{length}}{\frac{1}{4}\text{ thickness}}$,		·5259	—	1·211	—

and the quantity of electricity therein is to that in a circle of the same diameter as

·6627	1·526	—*
·74 to tab. log. or as	1·704 to N. log.	—
·8173	1·882	—

455] A trial plate for Leyden vials consisting of two plates with rosin between.

S – 2½	sep. neg. rather doubtful		3½
L – 1	pos. rather doubtful		

Double plate A, computed power = 11·04.

L – 3½ sep.	a little pos.	7½
S – 4	a little neg.	

Double plate B, computed power = 11·1.

L – 3	a little pos.	7¼
S – 4¼	a little neg.	

Large circle on silk strings.

L – 3½	a little pos.	8
S – 4½	a little neg.	

Globe on silk strings.

L – 4½	a little pos.	9¼
S – 4¾	a little neg.	

456] Therefore the quant. el. in these bodies seems as follows:

Trial plate	17½
A	18·4
B	18·3
circle	18·5
globe	18·8

* [See Note 12.]

Diameter of the globe = 12·1, therefore quantity of electricity in globe is to D° in circle of same diameter as 1·56 to 1*.

457] Two trial plates were made on a piece of the large bit of ground glass, one 2·37 inches diameter on place where the thickness = 1·80, computed power = 31·2; the other 2·57 inches diameter where thickness = 1·90, computed power = 34·8.

The first is called S the other L.

†The plates of ground glass E and F were each coated on one side with a circle 7·95 inches diameter communicating with coating on the other side. These plates were kept from touching by three bits of sealing-wax. When the coatings were kept at distance ·39 from each other this is called plate of air ·39 thick, &c.

A piece of wire of the same thickness as the other was made to slide thereon.

When the plate of air was tried against trial-plate S with wire drawn out 12 inches it is expressed

plate air − S + 12 &c.

Double plates A and B	S + 29½	sep. a little pos.
	L + 17	sep. a little neg.
plate air ·343	S + 0	did not sep.
	[L] + 3	a little pos.
plate air ·39	S + 18	sep. a little pos.
	L + 3	sep. a little neg.
same plate air	L + 38	sep. pos.¶
	S + 18	did same.

Tried again in afternoon of the same day.

A and B	S + 27	sep. a little pos.
plate air ·39	S + 19	do.
A and B	S + 29	do.
A and B	L + 15	sep. a little neg.
plate air ·39	L + 4	do.

458] The wire not drawn out is about 40 inches, and may therefore contain about 10 cyl.‡ inc. of electricity, *id est*, as much electricity as is contained in circle of 10 inches diameter. Quantity of electricity in additional wire is supposed to be equal to its length [divided] by 4·4.

Both the trial plates together, whose computed power = 66, is equivalent to 2A + 2B + 80 inches of wire + 45 of additional wire, *id est*, to 73·4 + 20 + 10·1 = 103·5 inches of electricity, therefore 1 inch of computed power in the glass of which trial plates are made should be equivalent to 1·41 inches of electricity.

By the experiment marked ¶ in [457], a difference of computed power in the trial plates = 3·6, which is equivalent to 5·08 inches of electricity, was

* [See Art. 653 and Preface.]

† [See Art. 341.]

‡ [Probably "circ." See Art. 648.]

equivalent to drawing out wire 20 inches, which is supposed = 4·54 inches of air, which is as near an agreement as can be expected.

By a medium of the experiments, the plate of air ·39 thick required wire to be drawn out $11\frac{1}{2}$ inches less than A and B, the different experiments varying from 9 to 14, therefore the plate of air contains 2·6 inches more electricity than A and B, *id est*, it contains 39·3 inches of electricity. The plate of air ·343 seemed by 1 experiment to contain 42·7 of electricity.

Therefore plate of air ·39 contains 4·94 times more electricity than a circle of same diameter, therefore quantity of electricity therein is to that in circle of same diameter as radius to thickness × 2·06 or quantity of electricity = computed power × ·243.

459] Four irregular pieces of glass, N, O, P, Q, were coated with circles. The thickness, specific gravity of glass and diameter of circles are marked in [Art. 370], the thickness of glass being found by taking thickness with calipers at center of proposed circle, and finding a part of outside of same thickness and measuring that part by Bird's instrument*; the computed power of all being just 40. The experiments were tried with sliding wire as former[ly].

Tried with large trial plate.

N	. 2	+ 0	separated constantly neg.
		+ 3	sep. but not certain.
P	. 1	+ 6	sep.
		+ 9	doubtful.
P	. 1	+ 0	did not.
Q	. 1	+ 0	did not.
N	. again	+ 3	sep.
		+ 6	rather doubtful.

With small trial plates.

N	.	+ 9	sep. pos.
		+ 6	did not.
Q		+ 0	sep. considerably.
O		+ 0	sep. considerably, but not so much as Q.
P		+ 6	doubtful.
		+ 9	sep. plainly.

The afternoon when these were tried, hygrometer corks closed in about 20 seconds.

The trial plates being inlarged, tried with large trial plate.

P	+ 42	doubtful.
	+ 39	do.
	+ 24	sep.
	+ 28	doubtful.
O	+ 0	very little, rather doubtful.
Q	+ 0	did not sep.
N	+ 21	sep. a little.
P	+ 24	sep. a little.

* [Arts. 341, 517.]

With small trial plate.

P	+ 28	did not.
	+ 36	did.
Q	+ 0	separated rather more.
O	+ 9	sep. a little.
	+ 18	sep. about as much as Q at 0.
N	+ 28	sep. a little.

These experiments were tried in the morning. In the afternoon hygrometer corks closed in about 30 seconds.

460] The plate B was coated with a circle 2·79 inches diameter, computed power = 40, and the plate D was coated with a circle 2·73, computed power = 46.

A piece of the white glass was also coated with a circle 2·85 in. diameter where the thickness was ·182, computed power 44·6.

They were tried with the same trial plates.

With large trial plate.

D	+ 0	sep. neg.
	+ 3	very little, rather doubtful.
B	+ 33	very little.
N	+ 21	very little.
D	+ 3	rather doubtful.

With small plate.

B	+ 48	very little.
D	+ 15	do.
N	+ 39	do.
White	+ 32	sep. a little pos.
B	+ 48	did not quite sep.
White	+ 18	nearly same as B.
	+ 24	sep. supposed nearly same as 1st time.
N	+ 27	sep. very little.
	+ 30	nearly same or rather more than W at 24 with large plate.
N	+ 14	sep. a little neg.
W	+ 10	do.
B	+ 32	do.
W	+ 8	do.
N	+ 14	do.

461] The plate A was coated with a circle 2·16 inches diameter, computed power = 22·6; a plate of rosin also, the first which was pressed out after hardening, was coated with a circle 2·51, thickness ·102, computed power = 2·51*; they were tried with the trial plates described in p. 16 [Art. 457].

Tried with small plate.

Rosin	+ 19	sep. a little pos.
A	+ 36	do.
Double plates A & B	+ 36	do.
Rosin	+ 16	do.

* [Should be 61·7.]

	With large plate.
Rosin	+ 0 sep. very little, rather uncertain.
A	+ 17 sep. a little, rather uncertain.
	+ 14 sep. a little.
Double plate	+ 15 do.

462] Hence it appears that A contains as much electricity as the two double plates. The rosin plate required the wire to be drawn out 18 inches less than them, therefore rosin plate contains 40·7 inches of electricity, and therefore quantity of electricity therein = comp. power × *.

A contains 36·7 inches of electricity, and therefore as A and B are of the same kind of glass, the quantity of electricity in them = computed power × 1·62 = ·21056, and B contains 64·96 inches of electricity.

The whitish glass plate required the wire to be drawn out 27 inches less than B, D requires 33 less and N requires 14 less, P requires 3 more than N, O 21 less, and Q 37 less than N, therefore W contains 71·2 of electricity, D 72·5, N 68·2, P 67·5, O 73 and Q 76·7.

Therefore

$$\frac{\text{Quant. el. in.}\dagger}{\text{comp. power}} \begin{cases} D = 1\cdot58 \\ W = 1\cdot60 \\ B = 1\cdot62 \\ P = 1\cdot69 \\ N = 1\cdot71 \\ O = 1\cdot83 \\ Q = 1\cdot92 \end{cases} \quad \text{spe. gra.} \begin{cases} 2\cdot973 \\ 2\cdot787 \\ 2\cdot674 \\ 2\cdot752 \\ 2\cdot682 \\ 2\cdot514 \\ 2\cdot504 \end{cases}$$

463] Experiments to determine whether the quantity of electricity in coated plates bore the same proportion to that in other bodies whether el. was weak or strong, or whether it was positive or negative ‡.

On the side of corks was placed plate A with circle 2 inches in diameter, containing 31 inches of electricity. On the other side there was no coated plate, but the wire was drawn out 23 inches and made to rest at further end on the sliding wooden plates. The heavy corks required more than 2ce the force to make them separate than the light ones.

With light corks	S − 0	sep. a very little neg.
heavy	S − 5	sep. a little.
——	L − 5½	sep. a little pos.
light	L − 7	do.

* [So in MS. See note to Art. 464.]

† [The "real charges" here given are in "circular inches," and the computed power is 8 times the true value, so that the numbers here given must be multiplied by 8/1·57 = 5·1 to compare them with those given in Art. 370. The diameters of the coatings in these experiments are not the same as those in Art. 370 which are taken from Arts. 508–515 and 672.]

‡ [Art. 355.]

Tried with the usual corks.

With the electricity neg. L − $2\frac{1}{2}$ sep. a little.
pos. L − $3\frac{1}{2}$ do.
neg. L − $2\frac{1}{2}$ do.

According to these experiments the plate should seem to contain

$$\frac{3\frac{1}{2}}{4 \times 31} \times \frac{4}{3} = \frac{7}{186} = \frac{1}{27}\text{th part}$$

more electricity in proportion when electrified by heavy corks than light, and about $\frac{1}{60}$th more when electrified pos. than neg.

464] A plate ·345 inches thick was pressed out of exper. rosin and coated with circle 3·41 inches diameter, therefore computed power = 33·7. This was compared with double plate B by help of the sliding coated plate mentioned in [Art. 442].

	Breadth of coating on sliding plate	
Rosin	29	sep. a little neg.
—	22	sep. a little pos.
B	20	sep. a little pos.
	$26\frac{1}{2}$	sep. a little neg.

Therefore the plate contains $18{\cdot}3 \times \frac{51}{46\frac{1}{2}} = 20$ inches of electricity*.

465] Side of square equivalent to trial plate.

Small plate drawn out to	0 = 10·72	·54
	1 = 11·26	
	2 = 11·80	
	3 = 12·35	
	4 = 12·83	·48
	5 = 13·31	
	6 = 13·78	
	7 = 14·26	
	8 = 14·74	
Large plate drawn out to	0 = 14·49	·54
	1 = 15·03	
	2 = 15·57	
	3 = 16·10	
	4 = 16·64	
	5 = 17·12	·48
	6 = 17·60	
	7 = 18·08	
	8 = 18·56	
	9 = 19·04	
	10 = 19·52	
	11 = 20·00	

* [This would make the specific capacity of rosin 20 × 5·1/33·7 = 3. The numbers in Art. 462 make it 3·3.]

EXPERIMENTS, 1772*

{Journal: from MS. N°. 13. See Table of Contents at the beginning of this volume. The footnotes refer back to the formal account of results.}

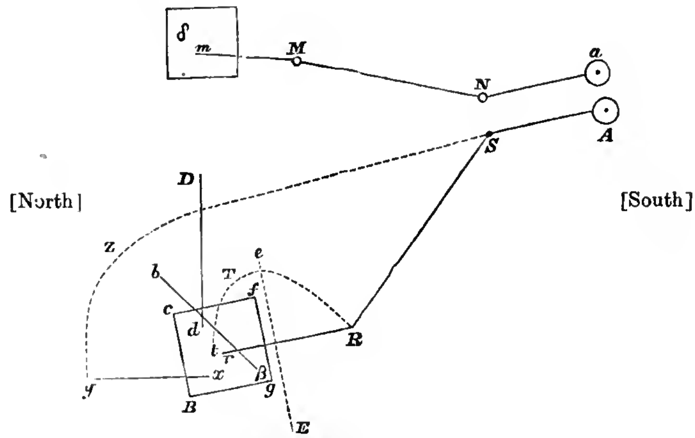
466] Plan of usual disposition of vials and bodies to be tried &c^a drawn in the true proportion and shape†.

δ is the trial plate, B the body to be tried, A and a the vials, mM and rR the touching wires.

$$rm = 83 \text{ inches}, \quad \left.\begin{matrix} mM \\ rR \end{matrix}\right\} = 27, \quad \begin{matrix} MN = 41, \\ RS = 61, \end{matrix} \quad Na = 24, \quad AC = 10.$$

Height of body and trial plate above ground = 4·2
below horizontal bar = 3·1½.

All the wires were about ·07 thick.



[West. Scale 1/18]

467] Comparison of quantity of electricity in a tin plate one foot square, according to the different situations in which it was electrified. The trial plate was suspended on waxed glass, the plate to be tried on silk strings‡.

Description of the different ways in which it was tried.

Fore Observation—The plate horizontal and placed as in figure, the touching wire also as in figure but extending to different distances upon the plate [called 2nd and 3rd way in Art. 266].

* [This is the heading of this bundle of the Journal, though the dates up to Art. 475 belong to 1771.]

† [See Art. 240.]

‡ [See Exp. III, Art. 265.]

Bent Wire—the same as former except that the touching wire was bent into the shape rTR, the distance rR remaining as before, the arch rTR being vertical and its greatest distance from the straight line rR being about 15 inches [5th way].

Cross Wire—The same except that the touching wire rR had a cross wire Ee placed horizontally fastened on within 3 inches of r, the touching wire being of the same length as before, and Ee 23 inches long. The touching wire was made to extend so much on the plate that Ee was about 1 inch distant from the edge of the plate [4th way].

Back Observation—The touching wire removed into the situation xy, the wire yzS being 13 feet 5 inches long and passing nearly perpendicularly over B and at the height of 3′.7″ above it [6th way].

Plate Vertical—The plate hanging in a vertical plane nearly perpendicular to the right line joining it and the vial. The touching wire touched it about the middle of the upper side [1st way].

468] Sat. Dec. 14 [1771]. Th. 53°. S. H. 19. C. H. + 7*.

Back observation. Touching wire extends 4 inches.

		Side of equivalent square	diff.	½ sum
B − 2	sep. a little neg.	12·33		
1½	very little	12·06		
1	very little, rather doubtful	11·78	2·85	10·35
D 3	do.	8·93		
2	sep. pos.	8·45		

Fore observation. Touching wire extends 9 inches over.

D − 5	very little, rather doubtful	9·84		
4	sep.	9·39	2·75	11·21
B − 3	sep. a little	12·85		
2½	very little, rather doubtful	12·59		

Fore observation, wire extends very little over.

B − 3	very little, rather doubtful	12·85		
B − 3½	sep. a little	13·10	3·01	11·34
D − 5	very little, rather doubtful	9·84		
4	sep.	9·39		

Touched by bent wire near middle.

D − 3½	sep. a little	9·17		
4	very little, rather doubtful	9·39		
B − 3½	extremely little	13·10	3·46	11·12
3	rather doubtful	12·85		

Th. 55°. Smeaton's Hygrometer 18½. Common Hygrometer + 5.

* [Th.—Fahrenheit's Thermometer: S. H.—Smeaton's Hygrometer. See "Description of a new Hygrometer by John Smeaton." *Phil. Trans.* 1771, p. 198. C. H.—Common Hygrometer.]

469] Monday, Dec. 16 [1771]. Trials of time in which electricity of stone square, &c. was destroyed.

The squares were supported on glass, and a piece of tinfoil about 1½ inch square fastened on each corner. On one of these pieces was fastened a wire from which the pith balls were suspended. The square was then electrified by applying a charged vial, and then a wire communicating with the wall was applied to the other piece of tinfoil.

With slate the corks closed in	10″
Portland	15
Bremen	8
gummed glass	5

The stones had been kept in fore room for several days. The gummed glass had been kept in fore room till the gum began to crack. It was then kept in back room for about 5 hours, and then kept in fore room about 1½ hour.

Th. 54. S. H. 22. C. H. + 14.

Hygrometer corks closed in 4′. The glass being then wiped they closed in 7′. Being then suffered to stand uncovered for 2 or 3 hours, the corks closed in 5′.

Th. 54. S. H. 20½. C. H. 11.

470] Tuesday, Dec. 17 [1771]. Th. 53. S. H. 20. C. H. + 9.

Experiments of [Art. 468] continued.

		Side sq. equiv.	diff.	½ sum
Plate vertical.				
C − 2	sep. a little pos.	10·09		
B − 3½	do. neg.	13·10	3·14	11·53
D − 5	do. pos.	9·84		
Fore observation. Wire extends very little over.				
C − 2	sep. a little pos.	10·09		
D − 5	more	9·84	3·25	11·71
B − 4	sep. a little	13·34		
Fore observation. Wire extending 9 inches over.				
B − 4	do.	13·34	3·78	11·45
C − 1	do.	9·56		
Bent wire.				
C − ½	do.	9·28	3·82	11·24
B − 3½	do.	13·10		
Cross wire.				
B − 2	do.	12·33	3·64	10·51
D − 2½	do.	8·69		
Back observation.				
D − 2½	do.	8·69	3·64	10·51
B − 2	do.	12·33		

Th. 55. S. H. 18½. C.H. + 6.

	This night		1st night	
Plate vertical	11·71 −	·18		
Fore obs. at extremity	−	·0	11·34 −	0
do. wire 9 inches over	−	·26	−	·13
Bent wire	−	·47	−	·22
Cross wire	−	1·20		
Back observation	−	1·20	−	·99

471] Wednesday, Dec. 18 [1771]. Th. 50°. S. H. $17\frac{1}{2}$. C. H. + 3. Trials of flat plates of different substances about 1 foot square*.

			Side of equiv. square	diff.	½ sum
Tin plate	C − 2	sep. about $\frac{1}{10}$ in.	10·09	3·73	11·95
	B − 5	do.	13·82		
Slate	B − $5\frac{1}{2}$	do.	14·06	3·71	12·20
	C − $2\frac{1}{2}$	do.	10·35		
Portland stone	C − 3		10·59	3·70	12·44
	B − 6		14·29		
Bremen stone	B − 6		14·29	3·70	12·44
	C − 3		10·59		
Glass coated with tinfoil	C − $1\frac{3}{4}$		9·96	3·86	11·89
	B − 5		13·82		
Pasteboard	B − $5\frac{1}{4}$		13·94	3·98	11·95
	C − $1\frac{3}{4}$		9·96		
Glass coated with salt and gum-water	C − 2		10·09	3·73	11·95
	B − 5		13·82		
Do. with charcoal powder	B − 5		13·82	3·73	11·95
	C − 2		10·09		
Hollow tin	C − $2\frac{1}{4}$		10·22	3·72	12·08
	B − $5\frac{1}{4}$		13·94		
Tin plate same as first tried	B − 5		13·82	3·86	11·89
	C − $1\frac{3}{4}$		9·96		

Th. 50°. S. H. 17. C. H. + $2\frac{1}{2}$.

The subject is continued in Art. 480.

472] Comparison of two tin circles 9·3 inches in diameter with one of 18·5; the two circles being placed in vertical planes parallel to each other and perpendicular to the vertical plane joining their centers and the trial plate, their centers being both in the above-mentioned plane†.

There was a distinct touching wire to each plate meeting each other at R, the two wires were kept asunder by a slender glass tube, and about 1 inch of the end of the wires bent at right angles horizontally in order to touch the plates by being let fall on their edges. When the large circle was tried, this

* [Exp. IV, Art. 269.]

† [Exp. V, Art. 273.]

double touching wire was removed and a single one used in its room, which was sometimes fastened to the middle of the glass tube, and sometimes used without it, as will be expressed.

The height of the top of the circles above floor = 4′. 3″.

The center of the large circle when that was used, or the middle point between the centers of the two small circles when they were used, was $\begin{Bmatrix} 8'.\ 10'' \\ 7'.\ \ 3'' \end{Bmatrix}$ from $\begin{Bmatrix} \text{vial} \\ \text{middle of trial plate} \end{Bmatrix}$.

The circles were suspended by silk strings. The length of the touching wires for the circles was 36 inches.

473] Monday Dec. 30 [1771]. Th. 50°. S. H. 18.

Two small circles at 18 inches from each other.

		Equivalent	diff.	½ sum	Proportion
B − 5	sep. about $\frac{1}{10}$	13·82	3·47	12·08	1·000
C − 2½	do.	10·35			
The same at 26 inches distance.					
C − 4	do.	11·07	3·44	12·79	1·059
B − 6½	do.	14·51			
at 36 distance.					
C − 4½	do.	11·31			
B − ¼	do.	11·37	4·08	13·38	1·108
A − 14½	do.	15·42			
Large circle, touching wire being fastened to glass tube.					
A − 17½	do.	16·94	4·09	14·89	1·233
B − 3	do.	12·85			
Do. without glass.					
B − 3		12·85	4·09	14·89	1·233
A − 17½		16·94			

The Proportions by theory, vide P. 14 of calculations*, are as follows†.

	Calculation	Experiment
Small circles at 18	1·000	1·000
at 26	1·044	1·059
at 36	1·074	1·108
single plate	1·160	1·233

474] The same experiments repeated in the same manner except that the distance of the center of the large circle, or of the middle point between the

* ["P. 14 of Calculations" refers to a rough calculation in parcel No. 6, which is an early form of Props. XXIX and XXX. See Arts. 140–143. "P. 14" contains the following remark, which fixes its date after Art. 456, "By exp. P. 15 [Art. 456] quant. el. in circle is to that in globe of same diam. as 1 : 1·56 :: ½ : ·78, therefore $\frac{2+n}{2+2n}$ = ·78." Here n is the reciprocal of p in Art. 140.]

† [See Art. 681 and Notes 11 and 21.]

centers of the small ones, was 5′. 3″ from the vial, and the middle point of the trial plate 8′. 2″ from vial, and that some boards forming a floor about 4 or 5 feet square was placed under the circles 14 inches from the ground, and that a perpendicular bar of the same breadth as those of the frame was placed 5 inches nearer to the circles than the other, so that the distance of the center of the large circle from the vial and the ground, and also the distance of the nearest small circle from the perpendicular bar when they were placed at 36 inches distance, were diminished in about the ratio of 2 to 3*.

475] Tu. Dec. 31 [1771]. Th. 51°. S. H. 18.

Small circles at 18 inches distance.

				Proportion
B − 6 sep.	14·29	3·22	12·68	1·000
C − 4 do.	11·07			
Do. at 26.				
B − ½ sep.	11·51	3·66	13·34	1·052
B − 8 do.	15·17			
Do. at 36.				
B − 2 do.	12·33	3·74	14·20	1·120
A − 15¾	16·07			
Large circle with glass.				
A − 18¾	17·54	3·72	15·68	1·237
B − 5	13·82			
Do. without glass.				
B − 5½	14·06	4·05	16·08	1·268
A − 20	18·11			
Th. 53.	S. H. 15⅓.			

476] Comparison of 2 wires 3 feet long and $\frac{1}{10}$ inch in diameter with 1 of 6 feet long and ·185 in diameter†.

The wires were placed parallel to each other, horizontal and perpendicular to the horizontal bar. They were touched almost close to one extremity by the same wires and in the same manner as the circles in the former experiment.

That end of the wires near the part which was touched was suspended by silk, the other end was supported on waxed glass. The distances were the same as in [Art. 472].

477] Fr. Jan. 3 (1772). Th. 50°. S. H. 19½. C. H. + 2.

Short wires at 18 inches distance.

				Proportion
C − 4 sep.	11·07	4·10	13·12	·847
B − 8 do.	15·17			
at 24.				
B − ½	11·51	3·64	13·33	·860
A − 14	15·15			

* [Art. 275.]

† [Exp. VI, Arts. 279 and 683.]

Short wires at 36 inches distance.

				Proportion
A – 15½	15·94	3·88	14·00	·903
B – 1½	12·06			

Single wire without glass.

B – 3½ sep.	13·10	4·79	15·50	
A – 19½ do.	17·89			1·000
A – 19 sep. less	17·66	4·32	15·50	
B – 4 do.	13·34			

Two wires at 18, repeated.

C – 4 sep.	11·07	4·35	13·24	·854
A – 14½ do.	15·42			

Th. 52°. S. H. 18. C. H. 4½.

By theory [Art. 152], the proportions should be between those of

	1	·9323	·9053	·8827
and	1	·8926	·8597	·8353

478] Comparison of different substances tried in the usual manner*.

The large tin circle suspended by silk.

				[Article]
B – 4½ sep.				
B – 3 sep. about $\frac{1}{10}$	13·82	4·07	15·85	[1]
A – 19½ do.	17·89			
B – 5 do.				

The globe suspended on silk.

B – 5 do.	13·82	4·07	15·85	[2]
A – 19½ do.	17·89			

Coated double glass plate A†.

A – 20 do.	18·11	4·05	16·08	[3]
B – 5½ do.	14·06			

Double plate B.

B – 5 do.	13·82	4·29	15·96	[4]
A – 20 do.	18·11			

The large circle supported on waxed glass.

A – 21	18·56	4·05	16·53	[5]
B – 6½	14·51			

A tin plate 15·5 square, on do.

B – 5	13·82	4·29	15·96	[6]
A – 20	18·11			

A tin plate 17·9 by 13·4, on do.

A – 20	18·11	4·05	16·08	[7]
B – 5½	14·06			

* [See Arts. 653, 654, 682.]

† Double plate ground glass A, thickness ·3, diam. coating 1·82, comp. power 11·04.
——————— B, ——— ·31, ——— 1·855, ——— 11·1.

A tin cylinder 35·9 inches long and 2·53 in diameter, on do.

				[Art.]
B – 7 A – 21	14·73 18·56	3·83	16·64	[8]

A tin cylinder 54·2 long and ·73 in diameter, on do.

A – 19½ B – 5	17·89 13·82	4·07	15·85	[9]

Brass wire 72 inches long and ·185 in diameter, on do.

B – 4½ A – 19	13·58 17·66	4·08	15·62	[10]

Th. 50°. S. H. 16½. C. H. – 8*.

479] According to the 5th and 6th articles of last page, the quantity of electricity in the square is to that in a circle of the same area as 1·08 to 1, and that in square to that in oblong of the same area as ·991 to 1.

By comparing the 2nd article with the 3 last, the quantity of electricity in $\left\{\begin{matrix}\text{thick cylinder}\\ \text{thin cylinder}\\ \text{wire}\end{matrix}\right.$ may be to that in a globe whose diameter equals the length of $\dfrac{\text{cylinder}}{\text{wire}}$ as $\begin{matrix}\cdot 939\\ \cdot 962\\ \cdot 988\end{matrix}$ to N. log $\dfrac{\text{length}}{\text{thickness}}$, or $\begin{matrix}1\cdot 184\\ 1\cdot 116\\ 1\cdot 103\end{matrix}$ to N. L. $\dfrac{2^{ce}\text{ length}}{\text{thickness}}$, or as $\begin{matrix}1\cdot 429\\ 1\cdot 271\\ 1\cdot 218\end{matrix}$ to N. L. $\dfrac{4\text{ times length}}{\text{thickness}}$.

Therefore, if we suppose that the real quantity of electricity in any cylinder is to that in the globe whose diameter equals the length of the cylinder as $1\frac{1}{7}$ to N. L. $\dfrac{2^{ce}\text{ length}}{\text{thickness}}$, or as ·4964 to tab. log $\dfrac{2^{ce}\text{ length}}{\text{thickness}}$, it will agree very well both with theory and experiment.

Or by comparing this with the first article, the quantity of electricity in any cylinder is to that in a circle whose diameter is equal to the length of the cylinder as ·759 to tab. log $\dfrac{2^{ce}\text{ length}}{\text{thickness}}$.

Comparative charges of bodies tried in the former experiment.

By means of this experiment and that of 1771. [Arts. 455, 456.] If the charge of the globe is called 1, that of the circle will be ·992, therefore, by comparing 6th and 7th articles with 5th, the charges of the square and oblong will be ·957 and ·965.

By comparing arts. 1 and 5, the charge of circle on waxed glass is greater than on silk strings in ratio 1·042 to 1, and therefore if charge of cylinders and wire on waxed glass are supposed greater than on strings in the ratio ·1·021 to 1 †, the charges of thick cylinder, thin cylinder and wire will be

1·028 ·980 and ·966.

* [Exp. VII, Art. 281.]

† [The cylinders and wire were supported on waxed glass at one end only.]

480] Sat. Jan. . Th. 53°. S. H. 23. C. H. 11½.

Comparison of different substances tried in the usual way* except that in the first experiment the touching wire *r*R and the wire RS were of brass ·185 thick.

Tin plate with thick touching wire.

C – 1	9·56	3·78	11·45
B – 4	13·34		

The same plate with the common touching wire.

B – 4	13·34	3·51	11·58
C – 1½	9·83		

Hollow tin plate, 1·01 thick.

C – 2½	10·35	3·71	12·20
B – 5½	14·06		

Glass covered with thick coat of tinfoil.

B – 4½	13·58	3·49	11·83
C – 2	10·09		

———Thin coat of do.

C – 1	9·56	3·78	11·45
B – 4	13·34		

———gold leaf.

B – 4½	13·58	3·62	11·77
C – 1¾	9·96		

———gum.

C – 2	10·09	3·97	12·07
B – 5½	14·06		

———Water with a little gum.

B – 4½	13·58	3·75	11·70
C – 1½	9·83		

The same tin plate as before.

C – 2	10·09	3·97	12·07
B – 5½	14·06		

Th. 55° ·5. S. H. 22·5. C. H. 10.

481] Result of this and Art. 471. [Same as Table, Arts. 269, 270.

* [Exp. IV, Art. 269.]

Trials of Leyden Vials.

482] The plates from Nairne made out of the same piece of glass were coated with circles of tinfoil as below*.

Plates	Thickness	Diameter of coating	Computed power
D	·2057	2·16	22·74
E	·2065	2·16	22·60
F	·2115	2·19	22·68
G	·2022	2·14	22·65
H	·07556	6·79	610·2
I	·07797	2·299	67·78
K	·07712	2·286	67·78
L	·08205	2·358	67·75
M	·07187	2·207	67·78
A	·2112	6·55	203·1
B	·2132	6·586	203·4
C	·2065	6·482	203·4

The old ground glass plates $\frac{A}{B}$ were coated with tinfoil $\frac{2{\cdot}123}{2{\cdot}27}$ square, computed power $\frac{27{\cdot}8}{33{\cdot}66}$, to be used as trial plates.

483] Friday, Jan. . Th. 52°. S. H. 15. C. H. – 9.

The plates D, E, F, G of Nairne were compared with the double plates A and B by means of the trial plates A and B and an additional wire sliding on the electrifying wire Mm.

The $\frac{\text{length}}{\text{thickness}}$ of the wire Mm is $\frac{30}{{\cdot}15}$ inches, the additional wire is of the same thickness. The wire Bb is 9½ inches long.

Plates tried	Additional wire	Trial plate		
2 double plates	15	A	sep. near 1 diam. closed soon. Called	1st way.
	18	A	rather more, closed soon,	2nd way.
	3	B	sep. 1 diam. closed much slower,	3rd way.
D	6	B	3rd way.	
	15	A	2nd way.	
E	15	A	2nd way.	
	6	B	3rd	
F	3	B	3rd	
	18	A	2nd	
G	18	A	2nd	
	6	B	3rd	

* [See Art. 315. The computed power as given in this part of the Journal is the square of the diameter divided by the thickness, which is eight times the computed power as defined in Art. 311, and calculated in Art. 315.]

Plates tried	Additional wire	Trial plate	
Double plates	3	B	sep. full 1 diam. did not close soon.
D	6		do.
E	6		do.
F	6		do.
G	6		do.
G	0		seemed more, but not quite certain.
G	12		seemed pretty certainly less.
G	18	A	sep. about 1 diam. closed faster.
F	18		do.
E	18		do.
D	18		do.
D	12		sep. but certainly less.
Double plates	15		sep. about 1 diameter*.

484] Two of the old ground glass plates H, I, K, L of Nuremberg glass†, were coated with oblong squares to serve for trial plates to the plates I, K, L, M of Nairne, but the observations were found to be so irregular that nothing could be made of them, owing, as was supposed, to the spreading of the electricity on the surface of the glass.

To prevent this, all the four plates H, I, K and L were coated with oblong squares, and cased in cement composed of 2 parts rosin, 1 of bees' wax, and 3 of brick dust†.

In making it the bees' wax was first melted and imperfectly dephlegmated, the rosin was then added and melted with as little heat as possible, and then the brick dust, previously heated so as to be very dry, was added. By this means the cement is more safe and sticky than if more heat is used in making it. In some of the mixtures also a small part of the rosin, never exceeding $\frac{1}{8}$th of the whole, was exchanged for as much pitch, which was added after the rest was melted and mixed.

The plates E, F, G, and I, K, L of Nairne were cased in the same cement, about ·165 thick.

A plate of the same cement was also cast by pouring it out on a tin plate. This was coated with circles about 2·2 in diameter.

485] The spreading of the electricity on the surface of the trial plates seemed not to be prevented by casing them in cement, for putting the plate L of Nairne on the positive side, and the trial plate H on the negative, then if the apparatus was let down and drawn up again immediately‡, the pith balls separated about half an inch negatively, but if the apparatus was suffered to rest at the bottom about half a minute, and then drawn up immediately, they separated considerably more than 1 inch, and if it was suffered to rest at the

* [See Art. 655.] † [See Art. 303.] ‡ [See Art. 302.]

bottom but a very short time, and then kept mid way for ½ minute, and then drawn up, the balls at first separated positively but closed very soon, and after a long time separated negatively.

If a sliding plate containing about ⅛ part of the electricity in the plate L was put on the positive side as an additional plate, and the apparatus was let down and drawn up immediately, the balls separated about 1 diameter negatively, but if it rested at the bottom ½ a minute and was then drawn up immediately they separated about 1 inch negatively.

486] In order to see how fast the electricity spread on the surface of the glass, the heavy paper cylinders* were placed in the usual place, and the light ones on the wire *Bb*, the wires *Gg* and *Ff* were detached from *Cc* and rested at bottom, and a coated plate on the positive side. The wire *Cc* was suffered to rest on *Aa* and *Bb* while the jars were charging, and the wire *V* drawn up so as not to rest on the coated plate†.

When the heavy cylinders, and *a fortiori* the light ones, separated, the wire *V* was let down on the plate and the wire *Cc* immediately drawn up, and the time elapsed till the closing of the light cylinders counted, which was as follows:

G, E, F	of Nairne all inclosed in cement.	20″, 25, 20	D, M	of Nairne not inclosed in cement.	20″, 35	L reversed	50″
F	reversed	23	M	reversed	36	Trial plate H	7
			L	of N. in cement	50	D° reversed	5

487] Another way was taken to try the same thing, namely, the wire *Ff* was taken off and *Gg* placed so as to lye below the wire *Dd* and to be drawn up against it by a string. The coated plate to be tried was placed on the negative side, the wire δ touching its bottom coating. The jars were then charged, the wire *Cc* resting all the while on *Aa* and *Bb*, and *Gg* drawn against *Dd*, and β drawn up so as not to rest on the plate. When the jars were sufficiently charged β was let down on the plate, and the wire *Gg* dropt immediately after, so as to take away the communication between *Dd* and the ground, so that the pith balls which were hung to *D* shewed whether much electricity passed round to under side of plate or spread on the surface.

When the pith balls communicated with the ground they separated about $\frac{3}{10}$ of an inch negatively by the repulsion of the wires on them.

Coated plates	Balls closed in seconds	Separated again in	
D of Nairne not in cement	3″ or 4″	30″	in 3′ separated about $\frac{7}{10}$
E of Nairne in cement	25″	2′. 30″	in 4′ separated about $\frac{4}{10}$
F do.	25	2′	
G do.	20	1′. 40″	in 4′ separated about $\frac{3}{10}$
I do.	35		

* [Art. 248.] † [See Fig. 20, Art. 295.]

Coated plates	Balls closed in seconds	Separated again in
K of Nairne in cement	35	
L	10	did not separate again in several minutes
Double plates A	15	1′. 15″
Double plates B	10	1′. 30″
Plate of cement	closed almost instantly, separated again in about 10″.	

488] Three sliding coated plates were made, each covered all over with tinfoil on one side, with a slip of tinfoil 1·8 by ·9 in. on the other. Two flat pieces of brass were also prepared, one 1·8 by ·9 and the other 1·8 square.

The tinfoil was divided in breadth into 6 equal parts, and the breadth of the coated surface is expressed in those divisions or in 6th parts of the breadth*.

The first plate was one of exper. rosin ·345 thick.

The 2nd, 2 plates of glass with rosin between.

The 3rd, a bit of the large piece of whitish plate glass.

The rosin sliding plate when the breadth of coating = 24, 2nd = 12, contains as much electricity as the double plate A.

The 3rd sliding plate when its breadth = $10\frac{2}{3}$ contains as much electricity as plate F of Nairne.

Therefore 1 division on $\left\{\begin{array}{l}1^{st}\\2^{nd}\\3^{rd}\end{array}\right\}$ sliding plate contains $\left\{\begin{array}{c}\frac{1}{48}\\\frac{1}{24}\\\frac{1}{10\frac{2}{3}}\end{array}\right\}$ of the electricity in plate D, E, F, or G of Nairne.

7 inches of additional wire answers to 1 inch of computed power in plates of Nairne.

2 trial plates were made for the plates I, K, L and M of Nairne out of 2 of the ground plates first got from Nairne. The dimensions of the coating of the small one was about 3·3 by 3·1, and that of the large one 3·7 by 3·4, the thickness of glass unknown.

Two trial plates were also made for the plates A, B and C of Nairne out of the old ground plates E and F. F, the smallest, was 5·7 square, and E was 6·3 by 6 nearly.

Two trial plates were made of crown glass for the plate H of Nairne, the small one 5·7 by 5·1, the other 6 by 5·9.

489] Tuesday, Feb. 4 [1772]. Th. 47°. S. H. 17. C. H. – 5.

Trial of plates D, E, F and G of Nairne, and of the 2 double plates, the plates E, F and G being cased with cement: tried by means of additional wire†.

* [See Art. 297.] † [Art. 318.]

Plates tried	Length of additional wire	Trial plate	
D	9	B	If let down and up immediately, sep. neg. about $\frac{1}{10}$ inch. If it rested at bottom 2″ or 3″, rather less.
G	0	B	do.
F	0	B	do.
G	2	B	do.
2 double plates	9	B	do., but closed sooner.
D	9	B	do.
D	16	A	sep. about $\frac{1}{10}$ pos.; much the same if it rested at bottom 2″ or 3″.
2 double plates	18	A	do. if let down and up immediately, rather more if resting at bottom 2 or 3″.
G	9	A	do.
F	7	A	do.
G	7	A	do.
2 double plates	19	A	do.

490] Feb. 4 continued. Comparison of the three plates E, F and G together with the plates I, K, L and M, tried by the two above-mentioned trial plates and the 1st and 2nd sliding plate*.

Plates tried	Sliding plate and breadth of coating thereon		Trial plate	
E, F, G	1st	9	S	sep. pos. about $\frac{1}{10}$. D° if resting 2 or 3″.
I	—	24		sep. rather less than $\frac{1}{10}$, rather more if resting 2 or 3″.
E, F, G	—	9		same as before.
M	—	16		sep. about $\frac{1}{10}$. D° if resting 2 or 3″.
K	—	24		D°
L	—	24		D°
L	2nd	17	Large.	Sep. neg. about $\frac{1}{10}$: rather less if resting 2 or 3″, the separation more after a little time than at first, and closed very slow.
K		17		D°
I		15		D°
E, F, G		7		D°
M		9		D°
E, F, G		7		D°

* [Art. 318.]

491] Trials of the same kind as those in [Art. 487].

Large trial plate for the experiments of this page—balls closed in 13″

Small do.	10″ or 15″
Trial plate A	15
B	15
E of Nairne	20
F	20
G	20
D	5
Double plate B	5
A	7
M of Nairne	20
I —	20
K —	35
L —	25

492] Wed. Feb. 5 [1772]. Th. 49°. S. H. 17½. C. H. 4½.

Comparison of I, K, and L together, with A, B and C by means of the trial plates E and F, and of A, B and C together, with H by means of the two crown glass trial plates*.

Plates to be tried	Sliding plate and breadth of coating		Trial plates	
I, K, L	2nd	8	F	sep. about $\frac{1}{10}$ pos. Much the same if resting at bottom 2 or 3″.
B	3rd	8	Do	
A	3rd	8	Do	
C	Do		Do	
I, K, L	2nd	10	Do	
C	3rd	8	Do	
C	3rd	10	E	sep. 1 diam. neg. Much the same if kept at bottom 2 or 3″. Kept increasing for a short time.
A	3rd	10		do.
B	—	11		do.
K, L	—	6		the same, only rather less if resting at bottom 2 or 3″.
C	—	11		the same as before.
I, K, L	—	6		the same as before.

* [Art. 318.]

493] Comparison of A, B, C together, with H*.

Plates to be tried	Sliding plate and breadth of coating		Trial plates
A, B, C	3^{rd}	18	Large. Sep. neg. about 1 diam., the same if it rested at bottom 2 or 3″.
H	Do		Do
A, B, C	Do		Do
H	Do		Do
H	3^{rd}	6	visibly rather more.
—	—	24	very visibly less than last, and seemingly less than former.
H	3^{rd}	24	Small—sep. near $\frac{1}{10}$ inch pos. Same if it rested at bottom 2 or 3″.
A, B, C	Do		Do
H	Do		Do
—	3	18	sensibly less.

Trials of same kind as those of [Art. 487].

I closed in	65″	H in	55″	Trial plate	E in	35″
L	20	B	20		F	7
K	20	A	20	Large trial plate for	H	5
		C	35	Small —		5

* [See Art. 658.]

EXPERIMENTS, 1773*

{From MS. N°. 14: see Table of Contents at the beginning of this volume. The footnotes refer back to the formal account of results.}

494] *Spreading of electricity on surface of glass plates.*

4 plates of English glass were cut out of same piece and coated with bits of tinfoil of the same size. One of these plates was covered at different times with thick solution of lac, which ran into heaps in drying, another with transparent varnish which also ran into heaps, another with solution of lac and vermilion which lay smooth, and the other left as it was.

They were all done in the end of the summer and suffered to dry in the open air. The spreading of the electricity on their surface was tried in the manner described 1772, p. 22 [Art. 486].

Tu. Oct. 13 [1772]. Th. 63. N. 20¾. C. – 7.

D of Nairne, corks closed immediately.
G do. in cem. in 3″ or 4″.
Plate with lac closed immediately, sep. again in 3″ or 4″.
Lac and verm. D°, but much more.
Transparent varnish same as lacquer.
Plate not varnished closed immediately, sep. again in 5″ or 6″.

495] *To see whether the machine used for trying Leyden vials† conducted fast.*

The heavy and light paper cylinders being both hung to conducting wire, and globe‡ turned till heavy ones separated, the light ones closed in 1′. 1″ after the others when the conducting wire communicated with machine, and about 1′. 20″ when it did not.

496] The plate covered with solut. lac was undone, and that and the plate not varnished were lacquered in Nairne's manner, one with vermilion, the other without. Neither of them were dried after the operation.

The old Lac and vermilion and the transparent varnish were dried before fire, heat uncertain.

Frid. Oct. 16 [1772]. Th. 63. N. 20. C. – 10.

The two plates lacquered in Nairne's manner discharged the electricity of the jars presently.

Lac and verm. in 1st manner closed in about 10″.
Transparent varnish uncertain, from 5″ to 20″.

* [This is the heading of this bundle of the Journal, though many of the dates belong to 1772.]

† [Art. 295.]

‡ [Of electrical machine. See Arts. 248, 563, 568, 569.]

Sat. Oct. 17 [1772]. The last varnished plates being dried before fire. Th. 65. N. 21. C. – 7.

1st lac and vermil. closed in 2″ or 3″, did not sep. in 1′.

2 D° closed rather sooner.

Transparent closed and sep. again in about 4″.

Tu. Oct. 19 [1773*]. The varnished glasses were baked over stove, the heat being kept for 2 hours at about 170.

Wed. Oct. 20 [1773*]. Th. 64. N. 22. C. – 5.

Lac & verm. closed in about 15″.

Old lac & verm. D°.

Transparent in 1 or 2″.

D° rubbed with cloth, closed and sep. again in 1 or 2″.

On Wednesday, the varnished plates were baked for above 2 hours, the heat the greatest part of the time about 210, but part of the time the ☿ rose a little way into the ball. I suppose must be at least 235 or 240.

Fr. . Th. 60. N. 22. C. – 5.

Transparent closed and sep. again immed.

Last lac & verm. closed & sep. again almost immed.

1st D° closed and sep. again in about 2″.

497] Glasses for exper. on spreading of elect.

9 plates of English glass & 8 of Nuremberg coated with plates of same size.

Sun. Oct. 24 [1773*]. Th. 63. Comm. – 6. N. 21½.

Closing of corks.

E. 9 closed in about 2″.
8 rather sooner.
7 D°.
6 D°.
5 D° 4, 3, 2, 1 D° 1 did not sep. in 1′.

N. 8 closed and sep. again in less than 2″ but did not sep. much.
7 closed in 1 or 2″ but did not sep. again soon.
6 closed presently and sep. again more than 8.
5, 4, 3, 2, 1 D° but seemed to sep. at first, to sep. more before it began to close, it was pos. after closing.

Sun. & Mon. N 1, 2, 3, 4 & 8 were baked with heat from 130 to 200. The tinfoil of the lowest plate was blistered.

Sun. Th. 62. Com. – 15. N. 18.

E 2, 3, 4, 5 closed in about 2″, 2 and 3 sep. again in 30 or 4[0]. E 1 closed not quite so fast.

E 9 not baked closed in about 7″, 7 & 6 in about 4″, & 8 in about 3″.

The 5 baked Nuremberg immediately separated wide, some without closing first, others with.

* [Probably 1772. See note to Art. 502.]

N 6 closed and sep. almost immediately, 7 closed and sep. almost immed. but did not sep. wide. 5 closed and sep. again in 5″ or 7″. N 8 washed with sp[ts] of wine did not close so soon as before.

N 8 & E 4 were washed with sp. wine & a little ros. varnish & then varnished with rosin.

Sun. eve. Th. 60. Com. – 20. N. 16.

E. 4 did not close in 1′.

N. 8 closed in 2 or 3″.

N. 3 closed and sep. again immed.

N. 8 & E. 4 were then cased in soft cement. The plates N. 1 & E. 3 were varnished with lac varnish, & the plates N. 2 & E. 2 with a mixture of 6 parts of varnish & 1 of vermilion, & afterwards baked for about 5 hours with a heat part of the time up to 200, and most of the time above 150, & N. 3 & E. 5 were varnished in the same manner, and then cased in a cement composed of 14 of rosin to 12 of brick dust. N.B. E. 5 was heated in drying the varnish to a great degree, so as to make it smoke violently.

Wed. Nov. 4 [1772]. Th. 59. N. 16½. C. – 18.

E. 4 closed in about 4″.

E. 5 in 3″ or 4″.

N. 8 closed and sep. again in 3 or 4″.

N. 3 seemed to do the same rather sooner.

Fr. Nov. 6 [1772], the plates E. 1 and N. 4 were varnished with rosin and the plates E. 2 & N. 2 varnished with a mixture of 4 parts of rosin varnish to 1 of vermilion & afterwards baked. They were a good deal heated both in varnishing and baking, so as to be somewhat blistered.

The plates E. 3 & N. 1 were also varnished before then and dried before fire.

Sat. Nov. 7 [1772]. N. 5 and E. 8 were varnished with rosin, and N. 6 and E. 6 varnished with 8 parts of solut. rosin & 3 of vermilion, and then baked for about 2 hours with heat which part of the time rose to 146, but commonly did not exceed 130.

Sun. Nov. 7 [1773*]. Th. 63. Com. – 2. N. 22½.

N. 3 closed and sep. wide immed.

N. 8 closed & sep. again in 2″ or 3″.

N. 6 rather slower.

N. 5 closed and sep. wide immed.

N. 4 more so.

N. 2 D°.

N. 1 D°.

E. 6 closed in 3 or 4″.

E. 8 closed and sep. again in 2 or 3″ about 1 inch.

E. 3 closed and sep. wide immed.

E. 2 D°.

E. 1 closed and sep. again in about 1′.

* [Probably Nov. 8, 1772. See note to Art. 502.]

E. 5 closed in about 2″.
E. 4 did not close in $\frac{1}{4}$ min.

498] Order in which the elect. spread.

N	E
6 rosin & verm. last done	4 soft cement
8 in soft cem.	6 rosin and verm. last done
5 & 3 {varnished with ros. last done and hard cem.	5 hard cem.
	8 rosin last done
4, 2, 1 {rosin alone, 2 1st; rosin and verm. 1st	1 rosin alone
	2 & 3 ros. and verm. 1st ros. alone 1st

4·21 of the lac varnish contains 12 gra. of lac.

Th. Nov. Th. 58. Com. – 13. N. 19.

N. 8 in soft cem. closed almost immed. sep. again in 3 or 4″.
E. 4 in D° did not quite close in $\frac{1}{2}$ min.
E. 8 in ros. closed and sep. again almost immed.
E. 6 ros. and verm. closed and sep. wide immed.
N. 5 ros. more so.
N. 6 ros. & verm. same as E. 6.

Th. Nov. Th. 58. Com. – 7. N. 21.

E. 4 at first approached a little nearer, afterwards did not.
N. 8 closed and sep. again in about 3″.
E. 2 Lac and verm. closed and sep. wide immed.
E. 3 Lac. not so soon.
N. 2 Lac and verm. rather quicker.
N. 1 Lac. D°.
E. 1 not varn. closed in about 8″.
N. 4 closed and sep. again in about 2″.

499] Sat. Nov. E. 8 & N. 5 were varnished with ros. & E. 6 & N. 6 were varnished with 8 parts of solut. rosin & 3 of vermilion. They were afterwards baked over boiling water for about 2 hours, the heat between 115 & 120.

Sun. Nov. E. 1 & N. 4 were varnished with 6·0 of thick solut. lac, 3·3 of verm. & 19 of spts, and E. 2 & N. 1 were varnished with 6·0 of solut. lac, 4·16 of verm. & 9 of spts. The quantity of this last mixture spread on the glasses was 8·0.

Mon. Nov. 16 [1772]. Th. 52. Com. – 11. N. 19$\frac{1}{2}$.

N. 1 closed in 1 or 2″, sep. again in about 4.
E. 2 did not quite close in $\frac{1}{2}$ min.
E. 1 nearly the same.
N. 4 nearly the same as N. 1.
E. 6 closed and sep. again immed.
N. 6 do.
N. 5 not quite so soon.
E. 8 closed and sep. again in about 3″.

N. 8 in soft cem. same as N. 1.

N. 2 not covered, closed rather sooner and sep. rather more than N. 1.

E. 3 closed in about 5″, did not sep. in ½ min.

500] Trials of quant. el. in Leyden vials, &c.

The following plates were coated as follows*.

	Mean thick.	Diam. coat.	Comp. power	Log. D°
Thick white	·2115	2·252	23·98	1·3799
Thin D°	·104	2·234	48	1·6812
N	·106	2·136	43·04	1·6339
O	·106	2·522	59·99	1·7781
P	·127	2·87	64·87	1·8120
Q	·076	2·082	57·04	1·7562
G	·1848	3·596	69·97	1·8449
White plate	·172	3·444	68·98	1·8387
Crown C	·0659	3·45	180·6	2·2567
D° A	·0682	3·51	180·6	2·2568
Thick rosin	·4845	3·760	29·18	1·4651
2nd do.	·195	3·374	58·37	1·7662 [See Art. 514]
3rd do.	·103	4·247	175·1	2·2434

Two trial plates were made with two plates of glass with rosin between, for comparing thick rosin with the double plates A and B.

Two trial plates were also made on a piece of the white plate glass for comparing N and thin white with D + E.

501] Sat. Oct. 17 [1772]. Th. 65. N. 21. C. 7.

Plates tried	Addit. wire	Trial plate		Inc. el. answering to addit. wire
D of Nairne varnished with lac	7	A	sep. a little pos.	1·58
G of N in cem[ent]	7	—	rather more	
——	0		much the same as D or rather less	·0
E D°	—	—	D°	
E	10	B	did not sep.	
G	—	—	D°	
D	—	—	scarce separated	
2nd ros. plate	18	A	sep. a little	4·07
D	7	A	D°	1·58
Thick ros.	3½	small	D°	·79
Doub. plate B	—	—	D°	
Thick ros.	1¾	large	very little	·40
Doub. pl. B	1¾	D°	less	
D	3½	A	sep.	·79
Thick white	7	A	D°	1·58

* [See Arts. 370, 371.]

Hence it should seem that the plate D contained about 1·6 inc. el. less than the plates E or G, which is nearly conformable to p. 26, 1772 [Art. 489].

The thick rosin plate seems to contain just the same as doub. pl. B, and the 2nd rosin plate to contain 2·49 inc. el. less than D. The thick white seemed to contain ·79 inc. el. less than D.

502] *Q and* P *compared with* M *and* K *of Nairne, also green cylinder* 4 *and white cylinder compared with plates of Nairne by means of sliding trial plates.*

Mon. Oct. 18*. Th. 64½. N. 17½. C. – 15.

[13 observations, Art. 660].

[Result.] Therefore K seems to contain 5 inc. el. less than M, conformable to 1772, p. 26 [Art. 489].

Q contains 16 inc. less, and P 16 less.

The comp. power of white cyl. = 537·5, and [it] appears to contain 756 inc. el. Therefore inc. el. by comp. power = 1·41.

The comp. power of green cyl. is 318·2, and [it] appears to contain $540 \times \frac{21}{22}$ inc. el., therefore inc. el. by comp. power = 1·62†.

503] 1st *and* 2nd *green and white cylinders and white jar compared with* H *of Nairne in usual manner.*

[12 observations.]

[Result.] Hence it should seem that the white cyl. contained as much el. as H; the 2nd green contained 45 inc. el. more than H; the 1st green uncertain, and the white jar seemed to contain 74 inc. el. more.

504] *Trials of the same cylinders and jar in same manner except that in trying the white jar and* 1st *green cylinder the plate* M *of Nairne was placed on the neg. side as an additional trial plate.*

Sat. Dec. 5 [1772]. Th. 56. N. 20.

[13 observations. Art. 660.]

[Result.] Hence	1st green cylinder should cont.	135 inc. el. more than H
	2nd	56
	white cyl.	7
	white jar	88

* [As the records of the actual observations in the following articles are of precisely the same nature as those already given, they will be omitted, and as the author has summed up the results for each day, these statements only will be given, except in cases of more than ordinary importance. According to the day of the week and month the dates for these experiments should belong to 1773, but as the experiments seem continuous with those of dates before and after which are certainly in 1772, I think Cavendish made a mistake in the day of the month which he did not find out till 4th November.]

† [These measures of specific inductive capacity must be multiplied by 5·1. See note to Art. 462.]

By means of this and preceding page, the quant. el., comp. power and quant. el. by comp. power are as follows*:

	Quant. el.	Comp. power	Quant. el. by comp. power†
1st green	1170	600·7	1·84
2nd do.	1023	600	1·70
white cyl.	976	684·1	1·43
white jar	1060	680·7	1·56

505] The quant. el. in the 2 coated globes was tried by putting the white cylinder and the 6th sliding plate on neg. side.

[6 observations.]

[Result.] Therefore $\begin{cases}\text{globe 2}\\ \text{globe 3}\end{cases}$ seems to contain $\begin{cases}1782\\ 1555\end{cases}$ [circ.] inc. el.

Trials of jars used in the 1st sort of experiments: [*Art.* 240] *tried by putting a sliding plate with or without the white cylinder on neg. side.*

Th. Dec. 3 [1772]. Th. 55. N. 22.

[6 observations.]

[Result.] There seems some mistake in the 3rd exper., therefore if we make use only of those exper. in which they sep. pos. the jar for $\begin{cases}\text{neg. side}\\ \text{pos. side}\end{cases}$ contains $\begin{matrix}162\\ 0\end{matrix}$ more than H;

id est $\begin{matrix}1134\\ 972\end{matrix}$ inc. of el., but if we made use only of the other exper. it would be $\begin{cases}\text{neg. side } 1233\\ \text{pos. side } 1043\end{cases}$.

506] *Trials of the 4 large jars, the jars being placed on the neg. side.*

Fr. Dec. 4 [1772]. Th. 59. N. $19\frac{1}{2}$.

[8 observations.]

[Results.] By mean

$$\begin{array}{llll}
\text{jar 1 equals} & \text{w. cyl.} & + \text{ g. c. 2} + \text{B} + \dfrac{\text{g. c. 1} + \text{C}}{2} + 5, 6 & = 3184\\
\text{jar 4} & \text{w. c.} & + \text{ g. c. 2} + \text{B} + \dfrac{\text{C}}{2} + 5 - 10 & = 2675\\
\text{jar 3} & \text{w. c.} & + \text{ g. c. 2} + \text{g. c. 1} + \text{B} + 5 - 7\tfrac{1}{2} & = 3635\\
\text{jar 2} & \text{w. c.} & + \text{ g. c. 2} + \dfrac{\text{g. c. 1} + \text{B}}{2} + 5 - 16 & = 3050
\end{array}$$

507] *Trials of the 5th and 6th trial plates, the trial plate being placed on neg. side.*

Result. Therefore trial plate 6 – 56 = H.

[Trial plate] 6 – 18 rather more than C′.

By mean 6 – 18 = C, and trial plate 5 – 17 = C.

* [See Art. 383.]

† [These measures of specific inductive capacity must be multiplied by 5·1. See note to Art. 462.]

The trial plate 4 is on same plate as 5, and the area of one division on it is to that of 5 on trial plate 5 as $1{\cdot}8^2$ to 9.

508] *Thick white, 2nd rosin,* D *and* F *of Nairne, and the two double plates together, compared together, also thin white with* D *and* E *and* D *and* F.

Sat. Dec. 5 [1772] in evening. Th. 56. N. 28½.

[24 observations.]

N.B. Before these exper. were tried, the plates E and F were freed from cem[ent] and coated afresh with plates of same size. The plate D was also freed from the varnish and coated afresh, and the trial plate B was freed from cement and coated with rather larger plates.

Hence it should seem that thick ros. contained 11 inc. el. less than the doub. plates A or B, *id est* 18·2 inc. el., that the thick white contained same as D, *id est* 36 inc., and that 2nd ros. contained 2·03 inc. less, *id est* 34 inc., which differs very little from p. 12 [Art. 501].

509] *Whitish plate,* P, Q, O, *old* G *and thin rosin compared with* M.

Sun. Dec. 6 [1772]. Th. 54. N. 20.

[19 observations, Art. 655.]

[Result.] By these exper. the two double plates should contain about 1·13 inc. el. more than D, that thick white contained same as D, that the 2nd rosin contained 2·26 less, that the thin white contained ·45 less than D + E, and that N contained 1·81 less.

Sunday evening. Th. 57½. N. 17½.

[18 observations.]

510] *Crown* A *and* C *compared with* A, B *and* C *of Nairne.*

Mon. Dec. 7 [1772]. Th. 55. N. 18½.

[10 observations.

21 observations of spreading of electricity on the different plates.]

By the above exper. Crown A and C should contain 15 inc. el. less than A.

511] *Whether the shock from the plate air was diminished by changing the air between them by moving them horizontally**.

Sat. Dec. 18 [1773†]. Th. 60°. N. 23½.

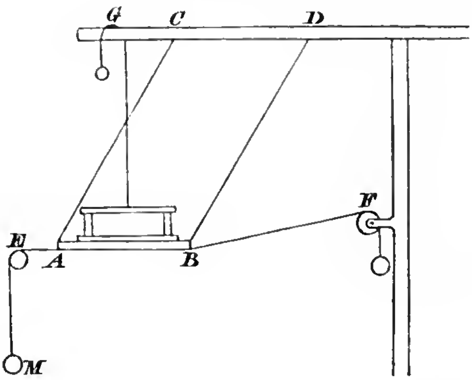
It was tried whether shock in charging plate air was sensibly diminished by moving the 2 plates horizontally, and thereby changing the air between them in the manner represented in figure, where AB represents the two 8-inch brass plates with sealing wax between them suspended by the silk strings AC and BD.

AE and BF are silk strings fastened to the frame on which lower plate rests, and passing over wire hooks E and F, and stretched by weights so that the plates would move from E towards F and rest in any position.

The electrifying wire was suspended by the string G with a counterpoise.

* [See Arts. 345, 516.] † [Probably Dec. 19, 1772. See Art. 502.]

The plates were electrified, holding my finger to bottom plate, they were then moved 24 inches by lifting up the weight M, and then discharged by holding my little finger to lower plate and touching upper plate with brass knob held in the other hand. I could feel a small pulse in little finger, having tried this I electrified and discharged the plates in same manner only without



moving them first and endeavouring to preserve the same distance of time between the electrification and discharge. I was not able to perceive any difference in the feel. I endeavoured to ascertain the time by the vibrations of a pendulum, but without much success. It seemed needless, however, as I could perceive scarce any difference in the sensation whether I discharged it immediately or waited as long as when the plates were moved. The usual time between the electrification and discharge was about $2\frac{1}{2}''$.

The experiment was also made by Richard, who did not perceive any difference.

The heavy paper electrometer was used. The bits of sealing wax between the plates were those made in 1771 [Art. 457].

512] *Whether globe included within hollow globe is overcharged by electrifying outer globe**.

It was tried whether the globe enclosed within hollow paper globe was overcharged when the outer globe was electrified.

This was first tried by making the 2 hemispheres slide on 2 sticks of glass by means of 2 tin hooks and a stick of glass fixed to the back of the hemisphere.

The wire by which it was elect. was suspended about 4 inches above the hemispheres while the vials were charging. It was then let down, and it was so contrived that the same motion of the hand which lifted up again the elect. wire, lifted up the wire which connected the inner globe with the outer, drew back the hemispheres, and drew up the pith balls fastened to a stick of glass till they touched the inner globe.

It was found that if the elect. of the hemispheres was discharged before they were separated, but after the communication between them and the inner

* [See Exp. 1, Art. 218.]

globe was taken away, that the pith balls did not sep., but if they were separated before their elect. was discharged, then the pith balls would at first sep. about an inch or so, but quickly closed, whereas if the inner globe was electrified after the hemispheres were separated, it was found to be a great while before the pith balls closed. It was found that this was owing to the sticks of glass on which the hemispheres slid being electrified thereby, as the same phenomena were produced by electrifying those sticks when the hemispheres were taken off.

N.B. These sticks were not covered with sealing wax, and as appeared by this exper. suffered the electric. to run along them pretty readily. The stick of glass run through the globe had all that part without the globe covered with sealing wax.

513] *The same thing tried by a better machine.*

Wed. Dec. 23 [1772]. Th. 52°. N. 18½.

The exper. was tried in a different manner, the hemispheres being fastened by sticks of glass covered with sealing wax within wooden frames turning on hinges.

If the pith balls were made to sep. pos. about 1 inch before the globe was elect. they separated 1½ or two diameters on touching the globe. If they separated only 1 inch before touching, they did not sep. at all on touching the globe. If they separated negatively 1½ or 2 inches before touching, they did not sep. at all after touching.

The event was just the same whether the wires for discharging the elect. of the globes when separated were placed so as to touch the hemispheres as soon as they were sep. an inch from each other, or whether they were placed so as not to touch them till they were separated almost the whole distance.

N.B. Each hemisphere was drawn back about 11 inches from its first situation.

It appears from hence that the inner globe was a small matter overcharged, but not enough so to make the balls sep. unless they were before positively electrified, so that the redundant fluid in it could hardly be $\frac{1}{24}$ of that which it would have received by the same degree of electrification if the outer hemispheres had been taken away, and probably not more than ½ as much.

[*Exper. rosin**.]

514] These rosin plates were made out of a mixture of 4 parts rosin and 1 of bees wax mixed together with a considerable heat, towards the beginning of the year 1771. Towards end of 1772 some round plates were cast out of this by gentle heat, which were pared to a proper size and shape and then pressed out between brass plates heated in wooden box over furnace (the tin lining being not then made), the bits of tinfoil were at first fastened on by just wetting it in a few places with gum water and sticking it on, but as this was found not to do well, the bits of tinfoil were afterwards rubbed with melted wax and fastened on by keeping them some time pressed with slight weights with flannel between them.

* [See Arts. 337, 373, and 500.]

515] 1st *and* 2nd *sliding plates compared with double plate* B, *also* P, Q *and* O *and thin rosin. Old* G *and whitish plate compared with* D, E, F, *and* M.

The 1st and 2nd sliding coated plates were compared with double plate B [6 observations]. The 25 div[isions] on sl[iding] pl[ate] were measured by using brass plate 3 inc. by 1½ and 9 div. of the tinfoil.

2 trial plates were made for plates M &c. out of a white glass hemisphere [8 observations. Art. 656].

Fr. Dec. 25 [1772]. Th. 49. N. 20.

[19 observations, and 3 on insulation. Art. 656.]

By this exper. with $\begin{cases}\text{large}\\\text{small}\end{cases}$ trial plate Q contains $\begin{cases}0\\ \cdot7\end{cases}$ inc. el. less than D, E & F, P $\begin{matrix}7{\cdot}3\\11{\cdot}7\end{matrix}$ less, O $\begin{matrix}4{\cdot}4\\7{\cdot}3\end{matrix}$ less, old G $\begin{matrix}2{\cdot}9\\7{\cdot}3\end{matrix}$ less, whitish plate $\begin{matrix}4{\cdot}4\\9{\cdot}5\end{matrix}$ less, and thin rosin $\begin{matrix}14{\cdot}7\\17{\cdot}2\end{matrix}$ less.

By mean P contains 9·5 less, O 5·8 less, old G 5·1 less, whitish plate 7 less, thin rosin 16 less, and Q ·3 less.

The wires used in the machine were all cleaned between this experiment and the next.

516] *Whether the charge of plate air is diminished by changing the air between them by lifting up the upper plate*.*

In order to try whether in electrifying plate air the electricity was lodged in the air or in the plates, the two brass 8-inch plates were placed on each other with supports of sealing wax to keep them at about ·4 inc. distance from each other and placed on the machine† with the end *M* of the wire *Mm* resting on it; the uppermost plate being fastened by a stick of waxed glass and 3 pieces of silk to the end of a lever so that it could be lifted up and down. It was also contrived so that in lifting up the plate the wire *Mm* was first lifted up from it about ½ inch, for fear that if *Mm* rested on the plate when lifted from the under one, some electricity might escape from the ends of the wire *Bb*, &c. The third sliding trial plate was put on the negative side.

If the wire *Cc* was let down and up immediately without lifting up the upper plate, the pith balls separated negatively very little with 18 divisions of sliding plate.

If the wire *Cc* was let down and immediately drawn up halfway, but not drawn quite up till the upper plate had been drawn up and let down again, the balls separated very little more.

The event was the same also if the trial plate was drawn out so that the balls should separate a little positively.

The upper plate was lifted up 2 or 3 inches. The sliding plate was let out to 12 divisions that the balls should separate positively.

* [See Arts. 344, 511.]　　† [Art. 295.]

517] *Trials of plate air* 1, 2, 3 *and* 4. [See Arts. 341 and 668.]

Two plates of glass 11½ inches square were coated with tinfoil about 11·4 inches diam. a slip of tinfoil extending from the coating to the other side. These plates were placed upon each other with coated sides to[wards] each other and kept asunder by 3 supports of sealing wax, the supports being placed a little on outside of coated part and tried in the usual manner.

Sun. Dec. 27 [1772]. Th. 50. N. 18.

[4 observations.]

Mon. Dec. 28 [1772]. Th. 53. N. 17½.

The exper. tried in same manner except that only 1 corner of the under plate rested on machine, the rest being supported by 2 wooden pillars, the places where it was supported being nearly under wax supports.

[15 observations. Art. 668.]

By this exper., plate air 1 contains 1 inc. el. more than D, plate air 2, ·1 inc. el. less than D + E, and plate air 3, 10½ inc. less than D + E + F.

Wed. Dec. 30 [1772]. Th. 55. N. 15.

The supports of plate air 3 altered and called plate air 4.

[12 observations.]

One of the pith balls was destroyed by accident, and another put in its room.

The plate air 1 was made to rest intirely on machine.

[3 observations.]

By this exper., plate air 4 contains 1 inc. el. less than D + E + F, plate air 2, 1 inch less than E + F, and plate air 1, 1 inch more than E. It should seem also that the wire *Mm* contained 2 inches less el. when the plate rested intirely on the machine than when it rested on it only by one corner.

The thickness of these plates of air was found by laying these plates on bracket fastened to dividing machine* with or without wax supports between them, and finding the division at which the new machine stood right, the knob of the new machine resting on the middle of upper plate, and the under plate being supported under the wax supports. By this means the thicknesses of these plates of air were as follows:

plate 1 = ·910
2 = ·420
3 = ·288
4 = ·256

Some experiments were made by putting bits of tinfoil between the plates whether the glasses were flat, and consequently whether the measures thus found were true. It seemed as if when the plates lay on each other the middle of the coatings could not want more than ·002 or ·004 of touching, but it did

* [Arts. 341, 459, 591.]

not appear that it wanted so much, and it seemed as if the outside did not want anything of touching, so that the above measures seem pretty just.

The diameter of the coating was 11·4.

The above coatings were taken off from these plates of glass, and coatings 6·254 in diameter put in their room, these with the small wax supports placed between them is called plate air 5.

N.B. 8 folds of the tinfoil used for these coatings was found to be $\frac{1}{100}$ inch thinner than the same number of folds of that used for former coatings, so that this plate air is about ·003 thicker than plate air 4.

The coatings were also taken from thin rosin and coatings 4·525 put in their room.

A plate of pure lac was also pressed out ·125 thick, and the coatings used before for thin rosin put on, which were found at a medium 4·23 in diameter.

Two plates of dephlegmated* bees wax pressed out the year before were also coated.

N.B. The bees wax was heated very hot in dephlegmating, and melted with gentle heat when cast into plates.

The thickness of the $\begin{matrix}\text{thinnest}\\\text{thickest}\end{matrix}$ of these plates was $\begin{matrix}\cdot 064\\\cdot 303\end{matrix}$, and the diameter of their coatings $\begin{matrix}2\cdot 74\\3\cdot 78\end{matrix}$.

518] *Lac plate and* 4th *rosin compared with* D + E + F; *also thin wax with* E + F; *also thick wax and plate air* 5 *with* D.

Mon. Jan. 4 [1773]. Th. 51. N. 17.

[23 observations.]

By these exper. Lac plate contains 1½ inc. el. more than D + E + F; 4th rosin from 3 inc. more to 2 inc. less, by mean much the same as D + E + F; thin wax 4 inc. less than E + F; thick wax 2½ less than D; plate air 5 ¼ more, and 1st made rosin ¼ more.

In the preceding experiments the plates of rosin &c. were exposed to the heat of the fire during trials, which seemed to cause an irregularity. To avoid that, in the following days' experiments the plates were laid on table at same distance from fire as the machine for some time before they were tried, and a screen was placed between all of them (except plate air) and fire while trying.

519] *Lac and* 4th *rosin with* D + E + F; *also thin wax with* D + E; *also thick wax,* 2nd *rosin and* 1st *made rosin and plate air* 5 *with* F.

Tu. Jan. 5 [1773]. Th. 50. N. 17.

[22 observations.]

By these exper. 4th rosin contains from 2 inc. more to 2½ less than D + E + F; by mean, the same as D + E + F.

Lac from 4 more to ½ less, by mean 2¼ more than D + E + F.

Thin wax from 4 less to 1 less, by mean 2½ less than D + E.

* [Art. 375.]

Thick wax 3 less than F.

2nd rosin, $1\frac{3}{4}$ less, plate air 5 $\frac{1}{4}$ more, and 1st made rosin same as F.

N.B. The 1st made rosin was made of the same proportion of rosin and bees wax as the others, but not of the same parcel: it is uncertain how much it was heated in making the mixture.

Result of the exper. on plate air.

	Diam.	Thick.	Comp. pow.	Inc. el.	Inc. el. by comp. pow.	Do.* $\times\frac{8\times 12\cdot 1}{18\cdot 8}$	Inc. el. / diam.	Last col. into excess preceding above unity
1st plate	11·4	·910	143	37	·259	1·34	3·25	1·11
2nd		·42	310	71	·229	1·18	6·23	1·12
3rd		·288	451	$97\frac{1}{2}$	·216	1·11	8·55	·95
4		·256	508	107	·211	1·09	9·39	·84
5	6·254	·259	151	$36\frac{1}{4}$	·240	1·24	5·80	1·39

520] *Breaking of electricity through thin plates of lac, exper. rosin and dephleg. bees wax.*

Thin plates were pressed out of lac, experimental rosin and dephlegmated bees wax, very thin at one end and thicker at the other. The tinfoil was stript from one side of these plates but the other left on, and was fastened to a piece of glass with gum water, and a piece of tinfoil fastened to the under side of glass communicating with the other.

These plates were placed on [the] negative side of the machine with wire δ bearing against bottom and a flat piece of brass at top on which wire β was suffered to rest. The machine was electrified in usual degree, and the bit of brass shifted from thicker to thinner part, till the electricity broke through the plate and discharged the jars.

A piece of the plate with the tinfoil under it was then cut out of the size of the brass plate, as near as possible to the place where the electricity broke through, and the thickness of the plate found by weighing it and also the tinfoil after the plate was separated from it.

The thickness of the plates thus found was as follows †, the specific gravity of $\left\{\begin{array}{l}\text{wax}\\ \text{rosin}\\ \text{lac}\end{array}\right\}$ being supposed $\left\{\begin{array}{l}\cdot 955\\ 1\cdot 06\\ 1\cdot 14,\end{array}\right.$

wax at 1st place		·0130
	2nd	·0123
rosin		·0131
lac		·0143

* [See Art. 343. The inches of electricity are circular inches, and to reduce them to globular inches must be multiplied by 12·1, the diameter of globe, and divided by 18·8, the diameter of a circle which has the same charge. The computed power here is the square of the diameter divided by the thickness, and this must be multiplied by 8 to get the computed power as defined in Art. 311.]

† [With the "usual degree of electrification" Lane's electrometer discharged at ·04 inch. See Art. 329. The electric strength of wax, rosin, and lac is therefore about three times that of air.]

521] *The quantity of electricity in a Florence flask tried with and without a magazine.*

The quant. el. in a Florence flask was tried by putting it on negative side, and some of the jars &c. on the other, the battery of 6 Florence flasks being used instead of the jars.

With the 1st, 2nd, 3rd jar with sliding plate 6 – 40 sep. neg. rather more than 1 diam.

4 jars + white cyl.	sep. a little pos.
1, 2, & 3 jars + 6 – 48	D° neg.

Sat. Jan. . Th. 56. N. 19.

The same thing tried again in same manner

with the 4 jars and white cyl.		sep. about 1 diam.	pos.
with	1, 2, & 3 jar	D°	neg.
	4 jars + white cyl.	D°	pos.

Tried without mag.

With 4 jars and white cyl. sep. at 1st about 1 diam. but soon closed,
with 1, 2, & 3 jar sep. a good deal neg.
with 1, 2, & 3 jar + wh. cyl. + gr. cyl. 2 after a time sep. near 1 diam.
With 4 jars and the 2 cyl., sep. at 1st a good deal, after a time sep. about 1 diam.

Sun. Jan. Th. 56. N. 21.

A coating of tinfoil to a part of the Florence flask out of water.

With 4 jars + wh. cyl. + gr. cyl. 2 sep. rather more than 1 diam.

The case was much the same whether wire was suffered to rest at bottom 2″ or 3″, or less than 1″.

With 4 jars + wh. cyl. + 6 – 16	sep. less than 1 diam.		
With 1, 2, & 3 jars + 6 – 16	D°	neg.	

Without mag.

With 4 jars sep. a little neg., increased after a time to full 1 diam.

By the 1st night's experiments the flask contains	12126 inc. el.
by the 2nd	11694
and by the 3rd	11495
Without magazine by 2nd night it contained	13205 inc. el.
The true quantity is supposed	11700

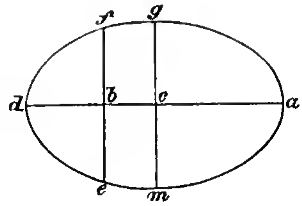
522] *Computed power of above flask.*

The diameter of the flask at the surface of the water in tin pan on Saturday was 1·7; the height of that part above the bottom 5·1; the height of top of tinfoil coating above bottom 6·55; and the diameter of that place, ·68; and the circumference at the widest part 13.

The weight of that part under water was 1 .. 2 .. 7*, and that of the part between that and the top of coating was 2 .. 4.

* [Troy weight.]

If the spheroid *agdm* does not differ much from a sphere, and *ab* does not differ much from *ad*, the surface *afea* is nearly equal to the circumference of $gm \times ab \times \frac{2ab + gm}{3ab}$,



the $\left\{\begin{array}{l}\text{surface}\\ \text{thickness}\\ \text{comp. pow.}\end{array}\right.$ of the part under water was $\left\{\begin{array}{l}62\\ \cdot 0127,\\ 6200\end{array}\right.$ and that of the part above $\left\{\begin{array}{l}5\cdot 3\\ \cdot 0179,\\ 375\end{array}\right.$ and the comp. power of the whole part below top of coating 6575, the specific gravity of the glass being supposed 2·68.

Therefore inc. el. by comp. pow. = 1·78.

523] As it appears from the above experiments that the Florence flask contains more electricity when it continues charged for a good while than when charged and discharged immediately, it was tried whether the white globes would do the same.

This was done by putting the globe 3 on positive side and the white cylinder and trial plate 6 on negative side, and first charging and discharging them in the common manner, and then discharging the magazine and charging it again, while the end *c* of the wire *Cc* rested on *Bb*, while the end *C* was prevented from resting on *Aa* by a silk string. When the magazine was charged, and had continued so for a little time, the end *C* was let down on *Aa* and the wire *Cc* immediately drawn up again so as to discharge the globe &c. The event was as follows,

in common way,	wh. cyl. + 6 − 20	sep. near 1 diam. pos.
globe elect. first,	+ 6 − 24	D°.
in common way,	+ 6 − 47	D° neg.
globe elect. first,	+ 6 − 48	D°.

By these experiments the globe contains 45 inc. el. or about $\frac{1}{34}$ less when electrified in the common way than when charged before the rest, which is as much as is contained in 1 inch in length of the uncoated part of the neck (the whole neck being 1½ inches), so that supposing the experiment exact it seems as if the globe contained rather more electricity when it continued charged a considerable time than when charged and discharged immediately*.

524] *Diminution of shock by passing through different liquors* †.

Tried in November [1772].

The electricity was made to pass through 42 inches of a saturated solution of sea salt in a thermometer tube of a wide bore, and the two jars charged in

* [These phenomena are connected with the "residual charge." A careful investigation of them has been made by Dr Hopkinson, *Phil. Trans.* vol. 167 (1877), p. 599.] {Reprinted in his collected *Scientific Papers.*}

† [This is the first experiment on electric resistance.]

such manner as that a slight shock should be felt in [the] elbows: it was then made to pass through rain water in a tube of a rather greater capacity, and the electricity made rather stronger. The wires were obliged to be placed within ·18 of each other in order to feel the shock in the same degree. Therefore the electricity meets with more than 230 times the resistance in passing through rain water than salt.

The above jars were electrified till light paper cylinders began to separate, and the shock made to pass through a tube filled with rain water. The wires were obliged to be brought within ·48 inches of each other in order that the shock should be just felt in the elbows.

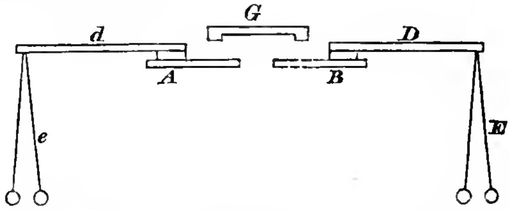
When the same tube was filled with saturated solution of sea salt diluted with 29 its bulk of rain water, a much greater shock was felt when the wires were at 16½ inches from each other.

Therefore electricity meets with much more than 34 times the resistance from rain water than from a saturated solution of sea salt with 29 of rain water.

When the same tube was filled with kitchen salt in 1000 of rain water, the wires must be brought within 4·4 inches; with pump water within 2 inches, and with spirit of wine almost close; therefore the resistance of

$$\left.\begin{array}{r}\text{Pump water}\\ \text{S. salt in 1000 of rain water}\end{array}\right\}\text{ is }\left\{\begin{array}{l}4\tfrac{1}{6}\\ 9\end{array}\right.\text{ less than that of rain water.}$$

Mon. Nov. 16 [1772] with straw electrometer. With sea water a shock was felt when the wires were 19½ inches distant; with rain water when they were at about ·19 inches distant. Therefore resistance of sea water is about 100 times less than that of rain water.



525] Exper. *Whether force with which two bodies repel is as square of redundant fluid in them**.

Tried by pith balls hung by threads.

A and *B* are the coated plates A and B, the bottoms of which communicate with the ground, *D* and *d* are two bits of wood resting on them, supporting the pith balls *E* and *e*. *G* is a bit of wood for making a communication between them. The wire for electrifying the plates rests on *B*, and is so contrived that when that is lifted up the wood *G* is let fall on the plates.

The pith balls *E* had bits of wire made to run into them in order to increase their weight.

A paper with divisions was placed 6 inches behind the pith balls and a guide for the eye 30 inches before them.

* [Arts. 386, 563, 567.]

Tu. Oct. 26 [1773]. Th. 60. Com. – 6½. N. 21½.

One of the balls E with its string weighed ·5 gr. and its wire 1·4, the other ·6 gr. and its wire 1·7.

The two balls and strings together—the weight of one of the strings, weighed 1·05 gr., the weight of the string about ·05, the weight of the two wires together was 3·2.

When the wires were taken out of the balls E, and a communication made between the two plates, while the electrifying wire rested on B, then when balls e sep. $\begin{cases}1{\cdot}2\\1{\cdot}08\end{cases}$, balls E sep. $\begin{cases}1{\cdot}25\\1{\cdot}14\end{cases}$.

The wires were put into balls E and the jars electrified while the electrifying wire rested on B. When the balls E sep. 1·3 inc. the electrifying wire was lifted up and the electricity of the plates taken away, immediately after which the electrifying wire was let down and immediately drawn up again when the balls e separated to 1·44. The electrifying wire being then let fall on B and suffered to remain, the balls E separated to 1·14.

The jars were charged, and the electricity diminished by alternately drawing up and down the electrifying wire and discharging the electricity of the plates till the balls e separated to 1·2; then letting the electrifying wire rest on B, the balls E separated to 1·08.

Wed. morn. The new heavy electrometer made with large wood ball and pith ball separates when the balls E separate to 1·52, and new light electrometer separates when the balls e separate to 1·44.

When balls e separate to 1·44, balls E separate to ·96.

The new heavy electrometer above mentioned separates about ¼ or ½ inch when old light cylinder electrometer just separates.

Result of these experiments.

Balls E without weight separate $\frac{1}{24}$ farther than balls e with the same degree of electrification.

If balls separate $\frac{1{\cdot}22}{1{\cdot}08}$ with 1 part of redundant fluid, balls of ¼ weight separate $\frac{1{\cdot}5}{1{\cdot}25}$ with ½ part of redundant fluid.

If balls of given weight separate 1·5 with given degree of electrification, balls of 4 times weight separate ·96, therefore if balls of given size are electrified in given degree, the distance to which they separate is inversely as $\frac{1}{3{\cdot}1}$ power of their weight.

Therefore, in last paragraph, if balls of given weight separate $\frac{1{\cdot}5}{1{\cdot}25}$, balls of $\frac{1{\cdot}9}{1{\cdot}57}$ their weight will separate to $\frac{1{\cdot}22}{1{\cdot}08}$, therefore if balls of given weight with given quantity of redundant fluid separate to given distance balls of $\frac{\cdot475}{\cdot392}$ that weight separate to same distance with half that quantity of redundant fluid.

526] *Whether the charge of plate* E *bears the same proportion to that of another body whether the electrification is strong or weak: tried by machine for Leyden vials.*

Wed. Oct. 27 [1773]. Th. 61. Com. 8½. N. 20½.

Plate E of Nairne on neg. side against sliding tin plates placed at end of long wire [20 observations].

Result. Therefore with light electrom. the plate E is balanced

$$\text{by a square of } \frac{14\cdot03 + 8\cdot17}{2} + x = 11\cdot1 + x,$$

$$\text{with heavy el. by } \frac{10\cdot09 + 12\cdot33}{2} + x = 11\cdot21 + x,$$

$$\frac{10\cdot59 + 11\cdot23}{2} + x = 10\cdot91 + x.$$

The plate E is balanced by 37 inc. el.

527] *Plain wax and* 3rd *dephlegmated wax with* E + F *and* 5th *rosin with double plate* A *and* B. *Also small ground crown with* D + E + F, *and large do. with* C.

The coatings were taken off from 4th rosin, and coatings 1·79 inc. diam. put in their room. This is called 5th rosin.

A plate of dephlegmated bees wax was also made [·120]* inc. thick and coatings put on 3·525 inc. in diam. This is called 3 dephlegmated bees wax.

A plate of plain bees wax was also pressed out [·119]* inc. thick and coatings put on 3·475 inc. diam.

A piece of thick crown glass was procured from Nairne about ·26 thick and ground down equally on both sides to about ·07 inc. thick. Two circular coatings were put on, one 3·54 inc. in diam. the other 2·035.

Wed. Jan. Th. 56. N. 27½ [16 observations].

528] K, L *and* M *compared with* D + E + F *at distance and close together; also large ground crown with* C *and small one with* D + E + F; *also* 3rd *dephlegmated wax and plain wax with* E + F; *also* 5th *rosin with double* B.

Friday, Jan. 29 [1773]. Th. 3½. N. 16½.

Tried with middle sized cork balls and a new white large trial plate [18 observations. Art. 656].

529] K + L + M *compared with* A, B, *and* C; *also* A + B + C *with* H.

Sat. Jan. 30 [1773]. Th. 50½. N. 16½. [20 observations. Art. 657.]

* [These measures are left blank in the Journal. I have supplied them from Art. 371.]

530] K + L + M *compared with* B *with electrification of different strengths.*

Sun. Jan. 31 [1773]. Trial plate F enlarged.

[14 observations with light and heavy electrometer alternately. Arts. 656, 658.]

531] K + L + M *with* A, B, *and* C; *also* D + E + F *with* K, L, *and* M; *also small crown with* K, L, *and* M; *and* D + E + F *and large ground crown with* A, B *and* C *and* K + L + M.

Mon. Feb. 1 [1773]. Th. 48. N. 16.

[20 observations. Art. 657.]

532] *On light visible round edges of coated plates on charging them**.

Mon. Feb. 1 [1773]. Th. 48. N. 16.

Some coated glass plates were placed on pos. side and electrified in usual manner in dark room in order to see whether any light was visible round their edges. With the plates M and L of Nairne and with the small ground crown glass a light was visible round the edges when the light electrometer was used, and nearly equally so with the large ground crown glass. The light seemed of the 2 rather stronger with the plates F and A of Nairne; no light was visible when light electrometer was used. but it was with the heavy electrometer.

533] *Crown* A *and* C *and large ground crown with* C; *also* 3rd *dephlegmated wax, plain wax and sliding plate* 3 *with* E + F; *also* 2 *double plates with* E, F, *and* D.

Mon. evening. Th. 53. N. 15. [25 observations. Art. 655.]

534] *Charge of the triple plate—the three plates* A, B *and* C *placed over each other, with bits of lead between coatings*†.

The three plates A, B and C were placed over one another with the coatings nearly perpendicularly over each other, with bits of lead between them, so as to keep them at the distance of ‡ inches from each other. This compound plate was tried in the usual manner.

Tu. Feb. 2 [1773]. Th. 50. N. 17½. [9 observations. Art. 677.]

535] *Whether the charge of plate* D *bears the same proportion to that of another body whether the charge is strong or weak: tried with machine for Leyden vials*§. [Art. 664.]

Th. Feb. 4 [1773]. Th. 48½. N. 13½.

Tried with smallest cork balls and the light straw balls as electrometers.

The plate D placed on the neg. side and the sliding tin plates at 23½ inc. dist. from wire.

* [Art. 307.]

† [Art. 380.]

‡ [So in MS.]

§ [Arts. 356, 664.]

Div. on el[ectrometer]*	Div. on sliding plate		Side square equiv. to trial plate	Diff.	Sum.
1 + 3	2 – 2	sep. neg.	10·09	6·61	26·79
	4 17	D° pos.	16·70		
	Tin plates at 17½ inches dist.				
1 + 3	4 18½	D° pos.	17·42	5·88	28·96
	2 5	D° neg.	11·54		
3 + 1	3 1½	D° neg.	12·06	3·62	27·74
	4 15	D° pos.	15·68		
1 + 3	4 18½	D° pos.	17·42	5·88	28·96
	2 5	D° neg.	11·54		
3 + 1	3 1½	D°	12·06	3·88	28·00
	4 15½	D° pos.	15·94		
	The same repeated with neg. elect.				
3 + 1	4 – 15	D°	15·68	3·62	27·74
	3 1½	D° neg.	12·06		
1 + 3	2 4½	D°	11·31	5·63	28·25
	4 17½	D° pos.	16·94		

536] H *with slits and a crown glass with oblong coating compared with white cylinder; also* A *and* C *with slits compared with* B.

The coatings were taken off from the plates A, C and H, and oblong coatings with slits put in their room; an oblong coating without slits was also put to a piece of crown glass, vide Measures [Art. 593].

Tu. Feb. 9 [1773]. Th. 50. N. 12½. 50 observations. [Art. 660.]

These plates were tried with middle cork balls.

Spreading of el. on surf.

A & C. Balls at first sep. wider. Closed in about 10″.

B. D°, but rather sooner. As it was supposed that this proceeded from the wires not conducting ready enough, the machine was moved slower, there was then but little of this and B was a great while before it closed, C about 5″, H a great while.

It was suspected that this increase of separation of the balls before they closed was owing to the wire designed to carry off el. to earth † not conducting fast enough. To try this, the next evening a long wire was insulated, and the cork balls hung to it. It was electrified sufficiently to make them sep. about an inch. They closed instantly on touching the wire with a bit of iron either communicating with wire for carrying off el. to ground, or whether it was only held in the hand. The air was as dry as the night before.

* [Divisions and quarter divisions.]

† [Art. 258.]

537] *Crown with slits and* H *with* D° *compared with white cylinder; and* A *and* C *with oblongs compared with* B*.

The coatings were taken off from the plates A and C and oblong coatings without slits put in their room. The coatings were also taken from the crown glass, and oblong coatings with slits like those put to C put in their room.

Fr. Feb. 12 [1773]. Th. 49. N.

[34 observations, Art. 660.]

538] *Experiment of p.* 61 [Art. 535] *tried with small ball blown to the end of a thermometer tube.*

A ball rather less than $\frac{1}{2}$ inch diam. was blown at end of glass tube and was coated on outside with tinfoil, the inside being filled with ☿. This was used instead of plate D in exper. to see whether charge of Leyden vial bore the same proportion to that of another body whatever force it was electrified with. It was found that 12 inches of this tube when coated contained as much el. as K + $\frac{8}{30}$D, and therefore the spreading of the el. $\frac{2}{10}$ inch on surface of this tube increases its charge by $\frac{1}{18}$D, whereas the spreading of el. $\frac{1}{10}$ on surface of D increases its charge by $\frac{1}{5\frac{1}{2}}$ D.

Th. 49. N. 13½. Tin plates at 17 inches from wire.

With electrometer at 1 + 3.

Div. on sliding plate			Equiv. to trial plate	Diff.	Sum.
4	22	sep. about 1 diam. pos.	19	5·9	32·1
3	3½	D° neg.	13·1		

With electrometer at 3 + 1.

Div. on sliding plate			Equiv. to trial plate	Diff.	Sum.
3	6	D°	14·3	3·6	32·2
4	19½	D°	17·9		

Fringed rings on plate of crown glass &c.†

Sat. Feb. 13 [1773].

It was found on looking at the plate of crown glass that there were narrow fringed rings of dirt all round the edges of the coatings, the space between these rings and the coating being clean. This was supposed to be done by the explosions.

The distance of these rings from the edge of the coating seemed nearly the same both within the slits and without, but of the 2 seemed less within the slits. The mean distance seemed about ·105 inc. which seems to shew that the electricity spreads pretty nearly the same both within the slits and without.

Something of this kind has been frequently observed in the sliding trial plate 1 and sometimes I believe in some of the coated glass plates.

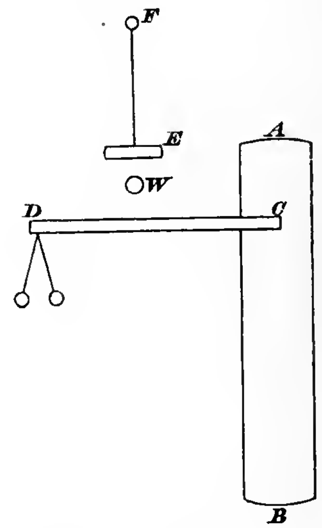
Sun. Feb. 14 [1773]. Th. 49. N. 17. Last exper. repeated.

[At 1 + 3, Sum = 29·6, at 3 + 1, Sum = 28·3. Plate D gave 26 and 27·5 respectively. See Art. 664.]

* [Art. 321.] † [Art. 308.]

539] *Experiment to determine whether the charge of a Leyden vial bears the same proportion to that of another body when elect. is very weak as when it is strong*.*

AB is a tin cylinder 14 feet 8·7 inches long and 17·1 inches in circumference. *DC* is a brass wire 37·1 inches long and ·15 in. diameter; both supported by non-conductors; with the middle sized cork balls hung at *D*.



FE communicates with the prime conductor and is charged till light paper electrometer separates. A brass wire is suspended by silk, so as to be made alternately to touch *E* and *DC*.

Mon. Feb. 15 [1773]. Th. 55. N. 22.

The cylinder *AB* and wire *DC* were electrified negatively till the balls separated about 1 diameter. On touching *DC* twice with the wire, the corks separated about as much positively.

The wire was 27·6 inches long and ·15 in. diameter.

The cylinder *AB* was then taken away and the plates D and E placed under the wire *DC*. The wire was obliged to be changed for one 20·8 inches long to exhibit the same phenomenon. [See Art. 666.]

Tu. Feb. 16 [1773]. Th. 57. N. 20.

Same exper. repeated.

Cylinder touched twice with wire 31 inches long; changed from about 1 diam. neg. to D° pos.

D and E with wire 24 inches D°.
cyl. with wire 31 D°.
cyl. with 27½ did not. [See Art. 666.]

540] *Lane's electrometer compared with straw and paper electrometers.*

In the afternoon. Th. 56½. N. 19. I tried the distance to which the spark would fly by Lane's electrometer.

Divisions on electrometer†	Distance [Lane]	
0 + 5	Knobs touched	
0 + 48	·027	Straw elect. sep. 1 + 3
1 + 5	·038	2 + 1½
1 + 15	·044	2 + 2½
1 + 20	·047	2 + 3
1 + 25	·051	3 + 0½
1 + 27½	·053	3 + 1
1 + 5	·038	light paper elect. just sep.
25 + 28	knobs at ·965 inc. dist.	

* [See Arts. 358, 666, and Note 25.]

† [Revolutions and 60th parts of a revolution. One revolution = ·038 inch.]

541] *Crown and* H *with slits compared with white cylinder; also on the excitation of electricity by separating a brass plate from a glass one.*

Wed. Feb. 17 [1773]. Th. 55. N. 21. [6 observations, Art. 660.]

Fr. Feb. 19 [1773]. Th. 53½. N. 18½.

A plate of glass 11½ inches square, coated with tinfoil 8 inc. in diameter, was supported on waxed glass. A brass plate 8 inc. in diameter was supported over it by silk strings in such manner as to lye on the plate perpendicularly over the tinfoil, and to be drawn up till it touched a piece of wire supported on waxed glass with the middle sized cork balls suspended from it. This was done in order to see how much of the charge of the plate was contained in the coating.

It was found that though the plate was not electrified, yet on lifting up the brass plate the balls separated some inches if the tinfoil communicated with the ground, but if it did not communicate, the balls, as well as I remember, separated considerably less. Some bits of thin silk thread were placed between the glass and brass plate.

In the afternoon. Th. 54. N. 17½.

The experiment repeated with bits of card between the glass plate and brass.

When tinfoil $\left\{\begin{matrix}\text{commun.}\\ \text{did not commun.}\end{matrix}\right\}$ with ground, balls sep. about $\left\{\begin{matrix}1\\ \frac{7}{8}\end{matrix}\right.$ inch.

When there was nothing between the glass and brass plate, they sep. 1·4 inc. whether the tinfoil communicated with the ground or not.

In all these cases the brass plate was negative.

The glass plate was found to be pos. if the tinfoil did not communicate with the ground, but I could not perceive it to be at all electrified if it did communicate.

The next morning the experiment was repeated, but the balls separated much less than before. The temper. of the air was much the same.

542] It was tried whether when three tin plates 1 foot square were placed near to and parallel to each other, the line joining their centers being perpendicular to their planes, the middle plate would receive much electricity on electrifying the plates*.

The experiment was tried with the same apparatus and nearly in the same manner as the experiment with the globe†, except that the two outer plates were suspended by two sticks of waxed glass turning on hinges. The wire too by which the plates were electrified was made so as to touch all three plates at the same time. Four bits of sealing wax were stuck to the middle plate, two on each side, to prevent the outer plates coming too near.

* [Exp. VIII, Art. 288 and Note 23.] † [Art. 218.]

Sun. Feb. 21. Th. supposed about 55. N. $20\frac{1}{2}$.

If the bits of sealing wax were of such size that the distances of the outer plates were about $\begin{cases}1\cdot15\\1\cdot65\end{cases}$, the middle sized cork balls separated about $\begin{cases}\frac{5}{8}\\\frac{3}{4}\end{cases}$.

The light paper electrometer was used in this experiment. If the globe 2 was electrified in the same degree, and its electricity communicated to $\begin{cases}\text{the 4 jars}\\\text{1, 2 \& 4 jars}\end{cases}$ and the middle tin plate electrified by one of these jars (the two outer being drawn aside) and the cork balls then drawn up against the plate, they separated about $\begin{cases}\frac{5}{8}\\\frac{7}{8}\end{cases}$.

In the $\begin{matrix}1^{st}\\2^{nd}\end{matrix}$ case the electricity of the globe was diminished $\begin{cases}8\\6\end{cases}$ [times], and therefore when the outside plates were at $\begin{cases}1\cdot15\\1\cdot65\end{cases}$, the quantity of electricity in the middle plate was about $\begin{cases}\frac{1}{8}\\\frac{1}{7}\end{cases}$ of what it would have received by the same degree of electrification if placed by itself.

543] *Charge of* A, B, *and* C *laid on each other without any coatings between; also charge of* 1st *thermometer tube.*

The coatings were taken from the 3 plates A, B and C of Nairne, and the plates cleaned and placed one on the other without anything between them, and stuck together by dropping some melted wax on the edges. The outside surfaces were then coated with circles 6·6 inc. diam. This is called Triple Plate*.

A thermometer tube was coated with coatings 11 inch long, the inside being filled with ☿, with wire let into one end, and the ends stopped with cement. The tube was 12·7 inc. long; weighed 1 .. 3 .. 0, and the bore held 22.gra. of water, the specific gravity of a piece of the same tube weighed twice over was $\frac{11\cdot7}{3\cdot15\frac{1}{2}} = 3\cdot1$.

N.B. The comp. pow. of this tube is about $90\frac{1}{2}$.

This is called Tube 1†.

Mon. Feb. 22 [1773]. Th. $53\frac{1}{2}$. N. $20\frac{1}{2}$. [8 observations. Art. 675.]

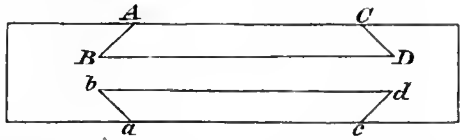
544] *Lane's electrometer compared with straw and paper electrometers; also charge of plate rosin with brass coating made to prevent spreading of electricity.*

Lane's elect.	Rev[olutions]	Div[isions]
Light paper just sep. when [Lane's] el. at	1	13
Straw at 1 + 3	0	54
3 + 1	1	36
Knobs touched at	0	$7\frac{1}{2}$

* [Art. 380.]

† [Art. 382.]

A plate of rosin and bees wax of the same proportions as for exper. rosin



was cast of the shape of figure, *ABDC* and *abcd* being brass plates 2·45 in. diam. their distance before the rosin was poured in being about ·12 inc.

Tu. Feb. 23 [1773] in afternoon, the rosin plate being cast that morning, the hygrom. as well as I remember being about 22.

[4 observations, comparison with E.]

Wed. Feb. 24 [1773]. Th. 54. N. 20. [4 observations.]

Spreading of electricity on surface.

Rosin closed in about 7″, sep. again in 35.

E was irregular.

545] *Second thermometer tube; also comparison of charge of cylinder used in* [Art. 539] *with* D + E.

A thermometer tube whose length was 22·1 inc., weight = 2, 17, 21 and weight water which filled bore 14 gra. was coated with tinfoil 15·5 long, conseq. comp. power = * the spec. gra. of a part of the same tube being 3·243.

Fr. Feb. 26 [1773]. Th. 52. N. 20½.

The cyl. used in [Art. 539] compared with the plates D and E, the wire *Mm* of machine being drawn out to 39½ inches, and resting on the cylinder as in that experiment. A sliding trial plate on neg. side.

[6 observations. See Art. 666.]

546] *Charge of second thermometer tube; also that of rosin plate with brass coating; also that of* A, B, *and* C *laid on each other without coatings between.* [10 observations. Art. 675.]

The same things were tried the day before, Th. 55, N. 17½, but the wire for making communication between machine and ground was forgot to be fixed. [14 observations. See Art. 666.]

547] † *The quantity of electricity in plate* D *compared with that in a tin circle of* 36 *and another of* 30 *inches diameter by means of the machine used for comparing simple plates* ‡, *the trial plate being a tin cylinder inches long, and* § *in circumference, fastened to the end of the usual sliding trial plates, with another cylinder of the same size sliding within it.*

Tried with elect. of usual strength and with the middle sized corks.

Also the double plate A compared with the circle of 18½ inc.

* [So in MS.]

† [Arts. 356, 664.]

‡ [Art. 240.]

§ [So in MS.]

Wed. March 3 [1773]. Th. 56. N. 21. [16 observations.]

548] *Charge of plate of experimental rosin designed for compound plate of glass and rosin; tried both when warm and when cold*.*

A plate of experimental rosin near 8 inches square was pressed out between two glass plates with tinfoil coatings fastened on by oil, the heat being such that it required very little weight to press out the rosin.

The thickness of the plate was much less toward one end than the other, varying in different parts of the coated plate from ·137 to ·108, but the mean thickness was ·122.

It was coated with circles of tinfoil 6·61 in. diameter.

Its charge was compared with that of the plates K, D and E of Nairne by means of a sliding trial plate made of the plate † of Nairne.

Sat. March 6 [1773]. Th. 55½. N. 17.

	Tr. sl. pl.	
K + D + E	24	sep. neg.
	19	D° pos.
ros. plate	19⅓	D°.
	24⅓	D° neg.

In the afternoon. Th. 57½. N. 16.

ros. plate	19½	sep. pos.
	25½	D° neg.
K + D + E	25	D°.
	19	D°.

The rosin plate was then warmed before the fire between two glass plates with flannel between them and the rosin till it would not support its own weight without bending. As soon as it was strong enough to bear its own weight it was compared as before.

rosin plate	20½	sep. pos.
	26	did not sep.

In about 2 or 3 hours after, when it was quite cold, it was tried again:

rosin	25	sep. pos.
	19	D° pos.
	18¾	D°.
	24½	D° neg.

549] *Whether charge of glass plate is the same when warm as when cold.*

The same afternoon the charge of a glass plate when hot and cold was compared together in the same manner.

The glass was 11½ inches square, used for Æpinus experiment ‡, coated on one side with a circle of 8 inc. diameter, and a brass plate of same diameter used for the other coating.

The glass and brass plate were both heated before the fire till almost as hot as I could bear my hand on, and then tried by the help of the 6th sliding plate,

* [Arts. 381, 678.] † [So in MS.] ‡ [Arts. 134, 341, 517.]

when the breadth of the sliding plate was required to be 37 in order that it should sep. pos.

After the plate was cold it was tried again, the breadth of the sliding plate was obliged to be 36.

Hence it would seem as if the charge both of glass and of rosin plate was the same when hot as when cold, the small difference between them being most likely owing to the electricity spreading more on the surface of the warm plate than of the cold one.

550] *Crown with slit coatings and* H *with oblong compared with white cylinder; also second thermometer tube with* D + E + F.

The slit coatings were taken from H and plain coatings, 6·03 square, put in their room.

Sun. March 7 [1773]. Th. 56. N. 15.

Straw elect. at 2 + 3, which is equivalent to light paper electrometer [4 obs.]. Elect. at 3 + 1 [6 obs.]. Elect. at 1 + 3 [7 obs. Art. 660].

Mon. Mar 8 [1773]. Th. 54. N. 14½. [4 obs.].

551] *Quantity of electricity in plate* D *and rosin with brass coatings compared with that of tin circle of* 36″ *and one of* 30″ *by machine for trying simple plates*; with different degrees of electrification*†.

Tu. Mar. 9 [1773]. Th. 51. N. 15.

The exper. of p. 78 [Art. 547] repeated, only using square tin plates of different sizes made to fasten on to sliding cylinder instead of the sliding trial plates.

The tin circles and the square plates both supported on silk.

Straw el. at inner marks N° 2.

	Square plate	Inc. cyl. drawn out	
circ. 36″	5	24	sep. a little neg.
—	3	11	D° pos.
E	1	26	D°.
	5	9	D° neg.
rosin	3	17	D°.
	1	4	D° pos.
circ. 30″	1	20	D° pos.
	3	28	D° neg.

El[ectrometer] at outer marks.

circ. 30	3	20	D°.
	1	29	D° pos.
circ. 36	4	34	D° neg.
	2	28	D° pos.
E	4	21	D° neg.
	2	24	D°.

* [Art. 240.] † [Art. 664.]

The electricity was found to break through the rosin plate when electrified with this strength, but there was no hole made in the plate, as it was found not to break through after that with the weak degree of electrification. It seemed not to pass over the surface, as no light was perceived.

552] *Charge of compound plate of glass and rosin.*

The rosin plate of p. 79 [Art. 548] without its coatings was included between the plates B and H of Nairne and the outside surfaces coated with circles 6·6 inc. diam. and is called Compound Plate.

Th. Mar. 11 [1773]. Th. 53. N. 17. [4 comparisons with K.]

Fr. Mar. 12 [1773]. Th. 53½. N. 13½. [20 observations, Art. 664.]

553] *Circle of 18½″ compared with double plates, also plate* D, *plate air and the two double plates compared with circles of* 36″ *and* 30″.

A sliding trial plate was made of deal, with an additional piece to fit on, the breadth was 31 inches, the length when not drawn out, and without the additional piece, was 15, and the additional piece increased the length 10½ inches.

The number in the 2nd column shews the number of inches by which the sliding piece is drawn out.

March 3					Inc. el.	Diff.	Mean
Circle 30 on silk	12·5	1½ 30	27·3 34·3	− ·8	26·5 33·5	7·0	30·0
D° on glass	14·7	4 36	27·9 35·6	+ 1·0	28·9 36·6	7·7	32·7
D° on silk	11·3	5½ 36	24·6 32·9	+ 1·7	26·3 34·6	8·3	30·4
E	15·7	1½ 38	27·3 36·1	+ 1·9	29·2 38	8·8	33·6
Circle 30″ on silk	18·7	8	33·9	0	33·9		

Tu. Mar. 9. [Electrometer] At inner marks. At outer marks.

	Diff.	Mean	Diff.	Mean
Circle 36″	11·6	35·6	5·8	36·3
E	12·2	32·5	6·9	33·2
Circle 30″	11·4	30·4	7	30·8

March 12. [14 observations.]

Sat. Mar. 13. Th. 55. N. 12. [12 observations. See Art. 649.]

Mon. Mar. 15. Th. 54. N. 14. [15 observations. See Arts. 649, 655.]

554] *The same with addit. four small rosin plates.*

Four plates of rosin and bees wax were cast 4 inches square and about ·22 thick and coated with circles 1·8 in. diam. A tin trial plate was also made 6 inches long and 5 broad. It is called N. The plates of rosin were connected by bits of brass wire like that used for connecting the two double plates.

Fr. Mar. 19. [20 obs. Arts. 649, 651.]

Tu. Mar. 23. [21 obs. Art. 649.]

Wed. Mar. 24. [22 obs. Art. 649.]

555] Sun. Mar. 21st [1773]. Th. about 55. N. about 15.

It was tried whether the 4 rosin plates contained the same quantity of electricity whether they were placed close together or at a distance, and what is to be allowed for the connecting wires, &c.

This was tried with the usual machine*, the rosin plates being placed on the positive side and sliding plate 3 on the negative side, the sliding plate remaining always at the same division, the small variations of the charge being found by the additional wire.

They were tried in 5 different ways.

1st way. The plates placed close together near the end *m*, the usual wires *V* resting on the plates, with the connecting wires put on the plates.

2nd way. D° without the connecting wires.

3rd way. The connecting wires suffered to remain, and also one of the wires *V*, but the 3 others removed towards end *M*, placed at 4 inches distance from each other, and supported in their usual situation by silk strings.

4th way. The same, except that the 3 wires *V* were taken quite away.

5th way. The rosin plates placed at as great a distance from each other as possible *id est* † inches with the usual wires *V*, but without the connecting wires.

	Inc. el. on addit. wire	Sliding plate		Inc. el. on addit. wire	Sliding plate	
3rd way, with connect. wires, usual ones removed to end	0	3...14	sep. pos.	1	3...12¾	D° neg.
4th way, without usual wires	2½		D°	2		D°
1st way, with connecting wires and usual also	1		D°	2		D°
2nd way, without connecting wire	2		D°	2½		D°
5th way, removed to distance	1½		D°	2½		D°

556] *Whether charge of white glass thermometer tube is the same when hot as when cold‡.*

Sun. Mar. 21 [1773] afternoon. Th. about 55. N. about 15.

A ball about 1 inch in diameter was blown at the end of a thermometer tube with a bulb 4·3 inches above. This was filled with ☿ sufficient to rise into

* [Art. 295. See Art. 337.] † [So in MS.] ‡ [See Art. 366 and Note 26.]

the bulb. The tube was coated 3·4 inches from the ball with gummed paper dipped in salt water and bound on with iron wire. This ball was placed in a glass of ☿ surrounded with iron filings and placed on machine near *M*, and heated by a spirit lamp, the ☿ in which the ball was immersed being made to communicate with the ground, and a bit of iron wire bound round the wire *Mm* being dipped into the ☿ in bulb.

The crown glass plate * and the plate A of Nairne, which was coated as a sliding plate, being put on negative side.

The 1st column being the number of square inches which it was necessary to give to the coating of the sliding plate in order that the balls might sep. pos. and the 2nd column that they might sep. neg. The charge of the crown glass plate being equal to that of the sliding plate when its coated surface is 33 square inches.

Sep. pos.	Sep. neg.	Heat of ☿ in which ball was immersed
12·9	29·4	cold
15	33	170
21		210
26·6		270
29·4	——	300 elect. passed through glass pretty fast.
31·8		305 passed through much faster.
29·4		290
19·6		235
17·2		160
14·8	33	145
12·9	30·6	cold

Tu. Mar. 23 and Wed. Mar. 24. [43 obs. Art. 649.]

557] *Allowance for connecting wires in p.* 86. [Art. 554.]

The allowance to be made for the charge of the connecting wires was endeavoured to be found by suspending the two circles of 9·3 inc. horizontally by silk lines at 11 inches distance from each other and finding their charge by means of the forked electrifying wire as in 1772 p. 7 [Art. 472], both when the plates were connected by a wire similar to that used for connecting the rosin plates, and without any connection.

The event was as follows.

Fr. Mar. 26th [1773. 8 obs. See Art. 647.]

Therefore the plates contain about $\frac{2\frac{1}{2}}{2}$ square inc., or 1·41 inc. el. more with the connecting wire than without†.

Sat. Mar. 27 [1773].

It was tried by usual machine whether the 4 rosin plates contained more el. when at a distance than near. The trial plate B *id est* the largest trial plate used for D &a being placed on neg. side.

* [So in MS.] † [See Art. 647.]

With a quantity of additional wire = to $9\frac{1}{2}$ inc. el. the balls sep. pos. when the plates were at as great a distance as possible. When they were placed close together they seemed to require rather more additional wire, and as well as I could judge, a quantity = about $\frac{1}{2}$ inc. el.

558] *Excitation of electricity by separating brass plate from glass one.*

Sat. afternoon. Th. 60. N. 9.

The experiment of p. 71 [Art. 541] was repeated. It was found that the brass plate was electrified on lifting up as before, though the plate was not electrified before. But if the plate was first charged and discharged again before the plate was lifted up, it was found to be stronger electrified.

I then took a piece of tinfoil of the same size as the brass plate, with a silk string fastened to it near the edge, and laid it on the glass and lifted it up gently by the silk string. The tinfoil was found to be electrified thereby.

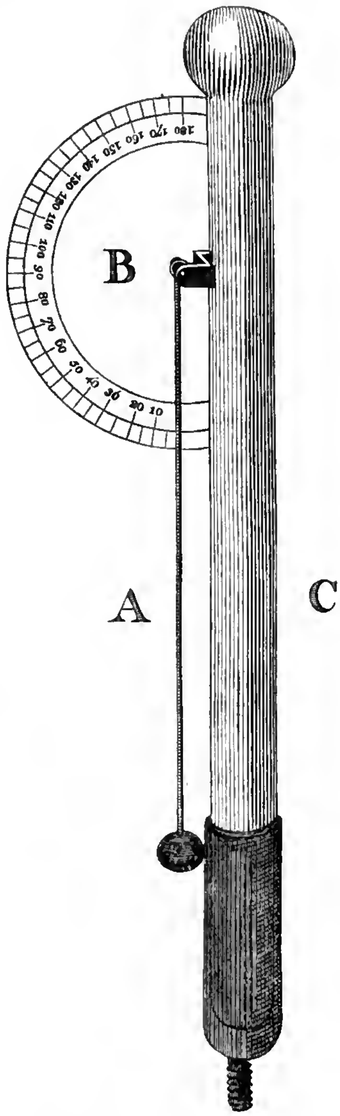
559] *Comparison of Henly's, Lane's, and straw electrometer.*

Sun. Mar. 28 [1773]. Th. about 58. N. about 8.

The two conductors of Nairne were placed end to end, and Henly's electrometer placed on that furthest from globe* parallel to conductor and the cork pointing from globe. The four jars were also joined to the usual wire with the straw electrometer hung to it, the wire and jars being placed at such a distance from the conductors that the electricity was found not to flow sensibly from them to the jars.

The globe 3† was then applied to that conductor nearest the globe and electrified till Henly's electrometer stood at 90°. The globe 3 was then removed from the conductors and its electricity communicated to the jars‡.

The straw electrometer separated to 2 + $\frac{1}{2}$.



[Henly's Electrometer, from the original figure, *Phil. Trans.* 1772, p. 359.]

* [Of Nairne's electrical machine.]

† [Globes 2 and 3 are glass globes coated as Leyden jars. See Art. 505 for their charges.]

‡ [For the charges of these jars see Art. 506.]

The experiment was repeated several times and was found to agree together pretty well.

The jars were then electrified, they and the straw electrometer standing in the same place, and it was found that Lane's electrometer fastened to one of them discharged at 0·53½ with that degree of electrification, the same jar being applied to the conductor and electrified till Henly's electrometer stood at 90°, Lane's discharged at 12·15.

The conductors being then taken away and the jars and straw electrometer placed in usual position, Lane's discharged at 1·17 when straw stood at 2 + 3, and at 1 + 2 when light paper electrometer just separated. The knobs touched at 0·4.

Sun. eve. Th. 58. N. 8.

The globe 3 electrified till Henly stood at 90°, and its electricity communicated to 1, 2, and 3 jars, straw electrometer separated to 2 + 1¾. Lane's with that degree of electrification discharged at 1·7.

When Henly's stood at 90°, Lane's discharged at 12·20.

Jar 2 charged till straw electrometer separated to 4, and electricity communicated to jar 1, straw separated to 2 + ¼.

When straw electrometer separated to	4	Lane's discharged at	2·0
	2 + ¼		·52
	2 + 3		1·19
light paper just separated			1·1

560] *Excess of redundant fluid on positive side above deficient fluid on negative side in glass plate and plate air &c.* *

Mon. Mar. 29th [1773]. Th. 58. N. 7.

The 11½ inch plate coated with circles of 8 inches diameter was supported on waxed glass. I charged this by touching the top with a vial charged till the straw electrometer separated to 2 + 3 while I touched the bottom with a wire. At the same time an assistant stood ready with a bent wire in his hand ready to discharge it as soon as I took the jar away, the wire was fastened to a stick of waxed glass and had the pair of cork balls commonly made use of hanging to it, the cork balls separated about 1 inch.

I then charged the jar 4 to the same degree and communicated its electricity to the jars 1 & 2 and touched the upper side of the plate with one of the jars, but without touching the bottom with the wire. The corks separated very nearly the same as before, but of the 2 rather more. I then charged the jar till the straw electrometer separated to 2 + 2 and diminished its electricity as before, the corks now separated rather less than the first time. The experiment was repeated several times with very nearly the same event.

I could perceive no difference in the separation of the cork balls whether the wire of the jar with which I touched the plate was 17 inches long or only 2¾.

* [See Note 30.]

If the four jars were charged to 2 + 3 and its electricity communicated to globe 3, it was diminished to 2 + 2.

The plate air 4 was charged by jar charged till straw electrometer stood at 2 + 0, and if jar 4 was charged to the same degree and its electricity communicated to jar 2, the corks separated the same if bottom was not touched.

With plate air 1 the charge was obliged to be reduced by communicating jar 2 to jar 4 to make the same separation when bottom was not touched as when it was.

Tu. Mar. 30 [1773]. Th. 56. N. 8.

The same experiment was repeated, only putting a piece of sealing wax with marks on it supported by glass about 2 inches below the corks to serve by way of comparison.

Compound plate of [Art. 552]	2 + 1	Jar 1 commun. to 2 + 4	$2 + 1\frac{1}{2}$
Plate air 1	$2 + \frac{1}{2}$	jar 1 to 2	2 + 1
Plate air 4	2 + 1	1 to 2	2

The second column is the distance to which the straw electrometer separated in charging jar with [which] the plate was electrified when the bottom was touched in order that the cork balls should separate equal to marks on wax. The third column is the ratio in which the electricity of the jar was diminished when the bottom was not touched, and the fourth column shews the degree in which the jar was electrified (as expressed by distance to which the electrometer separated) in order that the balls should separate to the required distance.

N.B. The paper of divisions used for the electrometer was different from that used before, but the divisions nearly of same strength. The marks on sealing wax used for compound plate were nearer than those for plate air.

The jars 1, 3 & 4 being charged till straws separated to 3 + 0 and the electricity communicated to jar 2, they separated to 2 + 1, and the electricity of jar 2 being destroyed and the electricity of the others again communicated to it, they separated to 1 + 3*.

Therefore diminishing the electricity in ratio of 95 to 126 diminishes distance to which the balls separate in ratio of 126 to 165, or diminishing the electricity in ratio 1·33 to 1 diminishes distance in ratio 1·31 to 1.

Result.

On Monday the excess of redundant fluid on the positive side above deficient fluid on negative side in

$$\left.\begin{array}{l} 11\frac{1}{2} \text{ inch plate with 8 inch coating} \\[2ex] \text{Plate air 1} \\[2ex] \text{Plate air 4} \end{array}\right\} \text{is } \begin{array}{c} \frac{1}{3\cdot56} \\[2ex] \frac{1}{1\cdot88} \\[2ex] \frac{1}{2\cdot14} \end{array}$$

* The smaller divisions are equal to $\frac{1}{4}$ of large ones.

of the quantity of electricity which is given to it with the same degree of electrification if the bottom plate is not touched.

	Compound plate	$\frac{1}{2{\cdot}66}$
On Tuesday D° excess in	Plate air 1	$\frac{1}{1{\cdot}86}$
	Plate air 4	$\frac{1}{2{\cdot}16}$

561] Fr. Apr. 2 [1773]. Th. about 55. N. about 10.

It was tried whether a parallelepiped box included within another box of the same shape and communicating with it would receive any electricity on electrifying the outer box*.

The experiment was tried just in the same manner as that with the globe in p. 26 [Art. 513]. The inner box was 12 inches square and 2 thick. The outer box was 14 inches square and 4 thick on the outside, and 13 square and 3·4 thick within.

The boxes were made of wainscot and well salted. I could not perceive that the inner box was at all over or undercharged, for if I previously electrified the cork balls positively sufficiently to make them separate in touching the inner box, they would separate as much if I previously electrified them negatively in the same degree.

Globe within hollow globe tried again†.

562] Sun. Apr. 4 [1773]. Th. 58. N. 11.

The globe included between the 2 hemispheres was tried again in the same manner, except that the hemispheres were coated with tinfoil and were made to shut closer.

I could not perceive the inner globe to be at all electrified either way.

In order to see how small a degree of electricity I could perceive this way, I separated the two hemispheres as far as in the experiment, and electrified the 2nd thermometer tube with the same strength of electricity as was used in the experiment, and communicated its electricity to the jars 1 and 2, then touched the inner globe with one of those jars and drew up the cork balls, previously positively electrified, against the globe. I found them to separate very visibly.

I then repeated the experiment in the same manner except that the balls were negatively electrified in the same degree.

The elect. of the thermometer tube was diminished by communicating to the 2 jars in the ratio of 105 to 6339‡, or of 1 to 60, so that if the redundant fluid in the globe had been so much as $\frac{1}{60}$ of that in the hemispheres, I must have perceived it.

* [Exp. 11, Art. 235.] † [Exp. 1, Art. 218.]

‡ [Charge of 2nd thermometer tube = 80·7 glob. inc. = 124·3 circ. inc., by Art. 675, jar 1 + jar 2 = 6234, by Art. 506.]

As it might be suspected that in the principal experiment the neighbourhood of the hemispheres communicating with [the] ground would enable the globe to hold more than it would otherwise do, and that therefore the cork balls would not separate so much as they would do if the hemispheres were taken away and the quantity of redundant fluid in the globe was the same, and consequently that the above computation of the quantity I could perceive is not just, I took away the hemispheres, made the corks touch the globe, and electrified it till they separated, then holding the hemispheres in my hands as near the globe as in the experiment, I did not perceive any alteration in the separation of the corks.

The outside diameter of the hemispheres was 13·3 inches.

563] *Experiment to see whether the force with which two bodies repel is as the square of the redundant fluid in them*: tried with straw electrometer and glass globes.*

The two electrometers were hung at opposite ends of a horizontal stick of wood 43 inches long, supported on sticks of waxed glass and communicating near the middle with one of the globes†. The same string also which lifted up the electrifying wire let down a piece of wood for making a communication between the two globes. The board with divisions was placed 6 inches behind the electrometers, and the guide for the eye 30 inches before it.

The electricity of the globes wasted very slowly, so that it could not be sensibly diminished in the time between reading off divisions to heavy electrometer and those to light one.

The electrifying wire rested on horizontal wood while globe† was turned, two jars being used as a magazine to prevent the globe Leyden vial from charging too fast. The globe‡ was turned till the heavy electrometer separated to rather more than the intended division, after which I waited till it came right, when by the string I lifted up the electrifying wire and made the communication between the two globes and looked at the division of light electrometer. The electricity of the magazine was discharged as soon as the electrifying wire was lifted up.

564] One of the straws used for the heavy electrometer was black in some places, and is called "blighted," the other is called "fair."

Sun. Jan. 24 [1773].

	Weight	Length	Cent. gr. from needle	Wire length	Added weight	Weight with addit. tried immed. after
Blighted straw	7	11·1	5·1	2·2	10·7	17·8
Fair straw	6	11·1	4·99	1·8	8·8	14·8

* [See Art. 386.]

† [The coated globes 2 and 3. Their charges are given in Art. 505 as 1782 and 1555 circ. inc., or 1159 and 1009 glob. inc., the sum of which, 2168, agrees with Art. 391.]

‡ [Of Nairne's electrical machine.]

The additional wire was run into straw very easily, and was fastened by putting a little wax on the end, which by heat was pressed quite smooth against the end of the straw.

Mon. morn. Jan. 25. Th. 55. N. 20½.

The globe 3 was made to communicate with horizontal wood, then if heavy electrometer separated to $\begin{cases} 8 \\ 9 \\ 10 \end{cases}$ divisions, the light electrometer separated, on communicating electricity to globe 2, to the same number of divisions.

565] *Trials of time in which the electricity of jar 1 was diminished by these straws, from degree in which the heavy electrometer used in former experiments of this kind to that in which the pith balls began to close.*

			Weight
Light electrometer	1	30″	7·8
	2	35	6·8
	fair	27	14·8
Heavy electrometer	blighted	15	17·8

In the afternoon the blighted straw was by mistake for the other covered for an hour with paper soaked in salt water. After standing about an hour to dry, it was found that when heavy electrometer was made to separate 10 divisions, light separated 11½.

The $\begin{matrix}\text{blighted}\\ \text{fair}\end{matrix}$ straw discharged the electricity as before in $\begin{matrix}5''\\ 22\end{matrix}$, weight of blighted straw 17·8.

The fair straw was then kept in salted paper in the same manner for about 3 hours.

Tu. morning. Th. 57. N. 23.

If heavy electrometer separates to 15, 10, 9, 8, light electrometer separates about ¼ less.

Fair straw discharged the electricity almost immediately, blighted in about 5″.

When fair straw rested on cork ball it was about 30″, the blighted was much longer.

Weight of fair straw 14·95.

Ball of fair straw moistened with salt water.

Heavy electrometer at $\begin{matrix}15\\ 10\end{matrix}$, light at $\begin{matrix}14\\ 10\end{matrix}$.

The blighted straw was kept in salted paper for 1½ hours, and then set to dry till the afternoon. In the afternoon its ball was moistened with salt water.

When heavy el. at $\begin{cases}15\\ 10\end{cases}$, light at $\begin{cases}14+\\ 9+\end{cases}$.

$\left.\begin{array}{l}\text{Fair}\\\text{Blighted}\end{array}\right\}$ straw closed in about $\begin{array}{c}2''\\2''\end{array}$ when resting on straw, and about 5″ or 7″ when resting on cork ball.

In about 1½ hours after they were tried again without any alteration having been made.

When heavy at $\left\{\begin{array}{l}15\\10\end{array}\right.$, light at $\left\{\begin{array}{l}14\\9\frac{1}{4}\end{array}\right.$.

$\left.\begin{array}{l}\text{fair}\\\text{blighted}\end{array}\right\}$ closed $\left\{\begin{array}{l}2\frac{1}{2}\\2\frac{1}{2}\end{array}\right.$ on straw $\left\{\begin{array}{c}8\\10\end{array}\right.$ on ball.

The light straw Nº 1 was soaked in salt paper at night for 3½ hours.

Wed. Jan. 27. Th. 57. N. 23.

When heavy sep. to 15, light at 14, but increased after a time to near 15. As it was suspected that this increase might be owing to the air being electrified, I tried and found the air to be much electrified in all parts of the room.

Nº 1 resting on $\left\{\begin{array}{l}\text{straw}\\\text{ball}\end{array}\right.$ closed in $\begin{array}{c}2''\text{ or }3''.\\90\end{array}$

Nº 2 on $\begin{array}{c}\text{straw}\\\text{ball}\end{array}$ closed in $\begin{array}{c}20''\\\text{very slow.}\end{array}$

The ball of Nº 1 was then moistened with salt water.

Heavy sep. to 15, light to 13, but increased to near 14.

In order to avoid in some measure the inconvenience from electrifying the air, Richard turned the globe, by which means the electricity was not made so strong.

heavy to $\frac{15}{10}$, light to $\frac{14}{10}$.

Nº 1 closed in about 4″ whether resting on ball or straw.

Nº 2 was soaked in salt water for 2½ hours till 3 in afternoon, about 5 or 6 it was tried.

heavy to $\frac{15}{10}$, light to $\frac{15}{9\frac{1}{2}}$ 1st time, for several times after to 14.

	On straw	On ball
Nº 2	4″	4
1	3 or 4	3 or 4
fair	2 or 3	8
blighted	2 or 3	9

Wed. Feb. 3. Th. 46½. N. 12.

As it was found the preceding day that the straws conducted ill, they were kept about 3 or 4 hours in the morning in salted paper, at about 3 they were taken out of the paper and hung up to dry. In the afternoon they were tried, a screen being placed to keep them from the fire.

Globe 3 elect. Heavy	Globe 3 elect. Light	Globe 2 elect. Heavy	Globe 2 elect. Light
15	16¼	15	17½
15	16	15	17¼
12	12	15	17½
12	12¼	12	14¼
10	10¼	12	13½
10	10½	12	13
8	8¼	10	11½
8	8	10	11½
8	8½	8	9½
8	8¼	8	9½
10	9¼	10	11½
10	10	10	11½
10	10	12	14
12	13	12	13½
12	12	15	17½
12	12	15	17½
15	16		
15	15½		
15	15½		

	On straw	On ball
The blighted heavy straw		
closed in	25″	1′. 30″
fair	10	not near closed in 2′

	Weight with addition	Without	Distance of pin from cent. grav.
Fair	14·85	6·05	5·07
Blighted	17·85	7·05	5·155

566] After the additional wire had been taken from the heavy electrometer, the two electrometers were electrified and compared together without the process of communicating the electricity from one globe to the other, when they stood as follows.

Heavy	Light
8	9
10	10¾
12	12½
15	15½
12	12⅓
10	10½
8	8½

		Heavy straw electrometer without additions
Heavy paper electrometer	$\frac{2}{10}$	17
	very little	15
Light do.	$\frac{2}{10}$	13
	very little	12

If globe 2 was electrified till D° electrometer separated to 17, on communicating electricity to globe 3 it separated to 9¼.

The light straw electrometer was then placed instead of the paper electrometer, and a paper with divisions placed behind it. It was found that when heavy straw electrometer separated to $\begin{matrix}17\\9\frac{1}{4}\end{matrix}$ divisions, the light straw electrometer separated to

Large divisions	Small divisions
3	1
1	3

567] As the straws seemed not to conduct well enough, they were gilt. The gilding was not perfect in several places, but it was sufficient to conduct the shock of a jar very weakly electrified.

		Weight	Cent. grav. from pin	Wire length	Added weight
Heavy electrometer	N. 1	7·55	5·25	2·45	12
	N. 2	6·55	5·17	2·01	10·1

Tu. Apr. 13.

The globe 2 electrified and communicated to globe 3.

Heavy el.	Light
13	$15\frac{1}{2}$
12	14
10	12
8	$10\frac{1}{2}$

Wed. Apr. 14. Th. 51. N. 13.

Globe 2 communicated to 3		Globe 3 communicated to 2	
8	$9\frac{1}{2}$	8	$8\frac{1}{2}$
10	$11\frac{3}{4}$	10	$10\frac{1}{2}$
12	14	12	13
13	$15\frac{1}{2}$	13	14

It was found that some electricity ran from the electrifying wire to the knob of the globe to which electricity was to be communicated, on which the knob was removed to such a distance that no sensible electricity ran from one to the other.

Globe 3 communicated to 2		Globe 2 communicated to 3	
13	$13\frac{1}{4}$	13	$15\frac{1}{4}$
12	$12\frac{1}{4}$	12	14
10	$10\frac{1}{2}$		
8	$8\frac{1}{2}$		

N.B. The holes where the wires were put in were gilt over.

$\left.\begin{matrix}\text{N. 2}\\\text{N. 1}\end{matrix}\right\}$ of heavy electrometer were found to weigh $\begin{matrix}16{\cdot}65\\19{\cdot}65\end{matrix}$.

The wires were then taken out, the holes stopped up with wax and gilt over. It was then found on electrifying the globe without communicating its electricity to the other, that when the heavy electrometer stood at

$$\begin{matrix}8\\10\\12\\13\end{matrix}\ \text{the light stood at}\ \begin{matrix}9\\11\frac{1}{4}\\13\frac{1}{4}\\14\frac{1}{4}\end{matrix}$$

	Weight	Cent. grav. from pin
N. 1	7·6	5·36
N. 2	6·65	5·285

	N. 1	N. 2
Force requisite to separate straws without wires	40·8	35·1
with wires	159·9	136·1

Therefore force required to separate heavy electrometer in the ratio of 296 to 303·6, or of 1 to 1·027.

568] *Separation of Henly's electrometer by different strengths of electrification.*

Nairne's jar being tried against the two trial plates for plate H, the pith balls separated a little after a short time the same way as the two trial plates. Therefore Nairne's jar is supposed to contain about $\frac{16}{9}$ of plate H, or 16 times as much as plate M.

The two conductors of Nairne were set end to end with [Henly's] electrometer on furthest, and the jar applied to the same, the furthest conductor being without any point, and the plate M was placed near it, set on a conductor communicating with the ground. When the electrometer was raised a little above 90°, the nearest conductor was removed and the electricity of globe taken away. Then as soon as the electrometer was sunk to 90° a communication was made between conductor and plate M and immediately taken away again, and the figure to which the electrometer sunk wrote down and the electricity of plate M discharged, after which a communication was again made between the conductor and plate M.

The results of the experiments are contained in the following Table, where the first column is the number of times that a communication has been made between the conductor and M.

Number of times	Elect. in jar	Numbers on electrometer 1st	2	3	4	5	6	Diff.	Supposed true Elect. in jar	Number on electr.	Diff.	2nd diff. by first
1	·938	70	73	73	79	77	80	063	1·000	90	10	160
2	·879	32	36	44	63	58	63	59	·938	80	17	290
3	·824	20	21	23	31	35	30	55	·879	63	31	564
4	·773	16	16	17	19	18	18½	51	·824	32	13·5	262
5	·725	14	14	15	16	14½	15½	48	·773	18·5	3	62
6	·679	12	12	13	14	13	12½	46	·725	15·5	2·5	55
7	·637	10	11	12	12	11½	11½	42	·679	13	1·5	35
8	·597	9	10	10½	11	10½	10½	40	·637	11·5	1	
9	·560	8	9	10	10	10	9½	37	·597	10·5	1	26
10	·525	7½	8				8½	35	·560	9·5	1	
11	·492	7	7½				8	33	·525	8·5	·5	
12	·461	6	6½				7	31	·492	8	1	
13	·433	5½	6				6½	28	·461	7	·5	25
14	·406	5	5½				5½	27	·433	6·5	1	
15	·380		5						·406	5·5		
16	·357		4½									
17	·334		4									
18	·313		4									
19	·294		3½									
20	·276		3									

The second column shews the quantity of electricity in the jar, which must diminish each time in the ratio of 15 to 16, and the other column is the number which the electrometer stood at in the different experiments.

The above experiment is supposed to have been made in the autumn of 1772.

569] *Separation of Henly's electrometer when fixed in the usual way and on an upright rod.*

Aug. 13, 1773. Th. about 78.

Henly's electrometer was stuck on a thin wooden rod 25 inches long, the end of which was fixed into the hole made in the conductor for receiving the electrometer, being parallel to the conductor as usual. The conductor to which this was fixed was connected to the other conductor which received the electricity from the machine by a brass wire about 10 inches long, and a jar with Lane's electrometer fastened to it was made to communicate with this last conductor, so that the rod to which the electrometer was fastened was about * inches from the globe and * inches from the jar.

Henly's electrometer was then compared with Lane's while in this situation, and when this was done the wooden rod was taken away and Henly's placed on the conductor in the usual manner, everything else being the same as before, and compared with Lane's as before.

N.B. In both trials the cork ball of Henly's was turned from the globe †.

The result was as follows:—

Lane	Henly	
Rev. div.	On rod	Usual way
4·30	21	5
6·30	37	10
8·30	38	18
10·30	40	32

Hence it appears that when Henly's [electrometer] is fixed on the rod it is more sensible towards the beginning of its motion than afterwards, whereas when put in the usual way it is the contrary.

570] Result of P. 70, 75, & 95 [Arts. 540, 544, 559], being a comparison of the different electrometers.

Straw electrometer at	P. 70 Th. $56\frac{1}{2}$ N. 19	P. 75 Th. $53\frac{1}{2}$ N. $20\frac{1}{2}$	P. 94 Th. 58 N. 8	P. 95 Th. 58 N. 8
$1 + 3$	43	$46\frac{1}{2}$		
$2 + \frac{1}{4}$				48
$2 + \frac{1}{2}$			50	
$2 + 1\frac{1}{2}$	60			
$2 + 1\frac{3}{4}$				63
$2 + 2\frac{1}{2}$	70			
$2 + 3$	75		73	75
$3 + \frac{1}{2}$	80			
$3 + 1$	$82\frac{1}{2}$	$88\frac{1}{2}$		
4				116
Light paper elect. just sep.	60	$64\frac{1}{2}$	58	
Henly's at 90°			731	736

* [So in MS.]

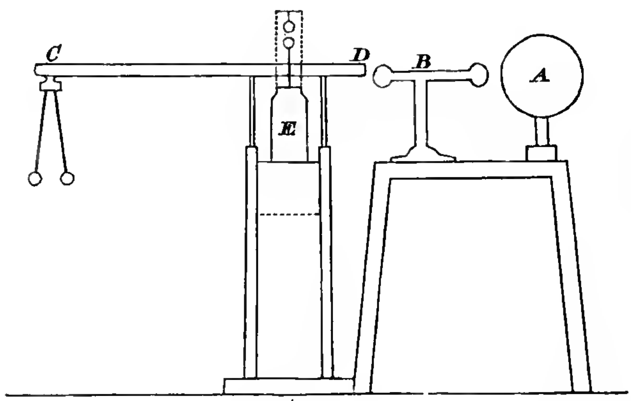
† [Of the electrical machine.]

The three last columns are the distances at which Lane's electrometer discharged, expressed in divisions, or 60th parts of a revolution of the screw.

By $\frac{\text{P. } 94}{\text{P. } 95}$ [Art. 551] the distance at which Lane's discharges is as the $\begin{cases} 1{\cdot}228 \\ 1{\cdot}226 \end{cases}$ power of the quantity of electricity in the jar, and the quantity of electricity when the straw electrometer is at 2 + 3, *id est* the usual charge is to that when Henly's is at 90° as 1 to $\begin{cases} 6{\cdot}53 \\ 6{\cdot}38 \end{cases}$.

571] *Comparison of Lane's electrometer with light straw electrometer in different weather.*

Lane's electrometer was compared with the light straw electrometer by the apparatus represented above. *A* being the globe, *B* a conductor, *CD* a wooden rod supported on two waxed glass pillars, having a pin at *D* almost in contact with the conductor, the straw electrometer being hung to *C*. *E* is a jar with Lane's electrometer fastened to it, supported on a bracket fixed to glass pillars, the wire of which touches *CD*.



The distance of *C* from the globe is 54½ inches and from the nearest glass pillar 32 inches. The height of the pith balls above the floor is 36¾ inches.

A small board with divisions on it, not represented in the figure, supported on an upright wooden rod, is placed behind the straw electrometer 25 inches from it, and a bit of tin with a narrow notch in it for an eye sight is placed at the same distance before the electrometer.

The outward divisions on the board, or those called the 4th, are at 5 inches asunder, the 3rd at 4 inches, the 2nd at 3 inches, and the 1st at 2.

As I found it impracticable looking attentively at both balls of the electrometer, I looked only at one, which, as my eye was guided by a narrow slit, was sufficient, and when I had made the experiment looking at one ball I repeated it looking at the other, so that the mean would be right though the slit was not right placed.

A wire was continued from the coating of the jar to the earth.

Wed. Aug. 18, 1773.

Th. 63°. N. 19. Bar. 29·64.

With two more jars communicating with E by wire.

Knobs of Lane's electrometer touched at 0·29.

$$\begin{matrix}\text{Straw} \\ \text{elect}^{\text{r}}\text{ at}\end{matrix}\left\{\begin{matrix}2^{\text{nd}} \\ 3^{\text{rd}} \\ 4^{\text{th}}\end{matrix}\right. \text{ division; Lane discharged at } \left\{\begin{matrix}1{\cdot}43 \\ 2{\cdot}27 \\ 3{\cdot}1\tfrac{1}{2}\end{matrix}\right. .$$

With only one jar; straw at 3rd division, Lane discharged at 2·27. A slip of tinfoil was then pasted on *CD* the whole length so as to touch the wire of the jar and the frame of the straw electrometer. The result with only one jar was then as follows.

$$\text{Straw at } \begin{matrix}3^{\text{rd}} \\ 4^{\text{th}}\end{matrix} \text{ division. Lane at } \begin{matrix}2{\cdot}26 \\ 3{\cdot}1\end{matrix} .$$

Th. Sept. 2. Th. 65°. N. 19. Bar. 29·865.

$$\text{Straw at } \begin{matrix}3^{\text{rd}} \\ 4^{\text{th}}\end{matrix} \text{ division. Lane at } \begin{matrix}1{\cdot}58\tfrac{1}{2} \\ 2{\cdot}41\end{matrix} .$$

Wed. Sep. 8th. Th. 62°½. N. 19½. C. 18. Bar. 29·235.

$$\text{Straw at } \begin{matrix}3^{\text{rd}} \\ 4^{\text{th}}\end{matrix} \text{ division. Lane at } \begin{matrix}2{\cdot}22 \\ 3{\cdot}1\end{matrix} .$$

In the afternoon. Th. 62°. N. 19. C. 17. Bar. supp. 29·37.

$$\text{Straw at } \begin{matrix}3^{\text{rd}} \\ 4^{\text{th}}\end{matrix} \text{ division. Lane at } \begin{matrix}2{\cdot}33 \\ 3{\cdot}0\end{matrix} .$$

Fr. Sept. 17. Th. 58°½. N. 28½. C. 29. Bar. 29·61.

$$\text{Straw at } \begin{matrix}3 \\ 4\end{matrix} \qquad\qquad \text{Lane at } \begin{matrix}2{\cdot}24 \\ 2{\cdot}59\end{matrix} .$$

572] *Comparison of strength of shocks by points and blunt bodies.*

The wooden rod used in P. 118 [Art. 571] was supported on waxed glass with the straw electrometer at the end, and some tinfoil wound round part of the rod. The white glass cylinder was put in contact with it, electrified in such a degree that I felt a slight shock in discharging it with a piece of brass wire with a round knob at the end. If it was then electrified in [the] same degree, and discharged [with] a like brass wire with a needle fastened to the end, I could perceive no shock, and but a very slight sensation, even though the point was approached pretty quick. The distance to which the straw electrometer separated was about 1·8 inches.

The white cylinder was then changed for one of the large jars, the shock was not very different whether it was discharged by the knob or point unless the point was approached very slow. The distance to which the electrometer separated was about ·9 inch.

The wooden rod was taken away, and the white glass cylinder made to rest on the conductor with Henly's electrometer on it, and electrified till it stood at 90°, and to prevent the shock being too strong it had its choice whether it would pass through my body or some salt water, the wires in the salt water being brought within such a distance that the shock was weak when taken by the blunt body. I then found that if I took it with the point I could scarce perceive any spark.

The experiment was tried in the same manner with a large jar. The shock was very sensibly less though the point was approached almost as fast I could.

573] *Whether shock of one jar is greater or less than that of twice that quantity of fluid spread on four jars**.

It was found that if the jars 3 and 4 were electrified in a given degree, and their electricity communicated to the jars 1 and 2, the shock produced by discharging them was nearly the same, or of the two rather more, than that produced by discharging the jar 1 or 2 by itself. The shock of the jar 3 was found to be very sensibly greater than that of jar 4.

It was tried with the wooden rod, the jars to be electrified being placed in contact wtih the tinfoil thereon, and when they were sufficiently electrified those to which the electricity was to be communicated being approached till they touched the rod, all four standing on the same tin plate. The jars were electrified till the straws separated ·9 inch.

N.B. The jars 1 and 2 contain pretty nearly the same quantity of electricity and their sum is nearly equal to the sum of jars 3 and 4. The quantity of electricity in jar 3 exceeds that in jar 4 in the ratio of 37 to 27, or of 4 to 3 nearly†.

574] *Comparison of the diminution which the shock receives by passing through water in tubes of different bores, and whether it is as much diminished in passing through 9 small tubes as through the same length of one large tube the area of whose bore is equal to that of the 9 small ones‡.*

Nov. 1773. It was tried whether a shock was as much diminished by passing through a glass tube filled with water, 37 inches of which held 250 grains of water, as in passing through 9 tubes, 37 inches of all which together held 258 grains of water, the length of water which it passed through being the same in both cases, namely about 40 inches.

Two jars were used, and charged till the straw electrometer separated to $3 + 0$. The water in the tubes was mixed with a very little salt, and the shock just enough to be perceived.

I could not be certain that there was any difference, but if any, that with the single tube seemed greatest. The shock was then made to pass through 7 of the small tubes, 37 inches of which hold about 200 grains of water. The shock was then sensibly less than with the large tube.

It was afterwards tried through what length of a tube, 37 inches of which held 44 grains, the shock must pass, so as to be as much diminished as in passing through $44\frac{1}{4}$ of the large one.

* [Art. 406 and Note 31.] † [Art. 685.] ‡ [See Art. 506.]

It was found that when it passed through 5·2 inches the shock was sensibly greater, and when it passed through 8·4 sensibly less than with the large one, so that it is supposed it would be equal if it passed through 6·8.

$$\frac{6{\cdot}8}{44\tfrac{1}{4}} = \overline{\frac{44}{250}}\Big|^{1{\cdot}08}$$

so that the resistance should seem as the 1·08 power of the velocity.

N.B. The quantity of water which the tubes held was not measured very exactly.

575] The tubes used in p. 123 [Art. 574] were measured by ☿ and are as follows:

No	Length col. ☿	Weight [Troy] [oz. pwt. gr.]	Length of same column when near: Straight end	Bent end	Weight of 37 inches in grains
1	37·1	16 . 12	20·9	20·2	395
2	37·3	14 . 2	23·65	22·9	335
3	38·4	1 . 0 . 8	24·5	21·8	470
4	38	11 . 6	24·5	24·7	263
5	37·7	17 . 17	21·7	23·3	417
6	36·8	14 . 10	20·4	20·3	348
7	38·8	15 . 22	26·6	22·3	364
8	38·6	17 . 17	24·8	21·6	407
9	39·8	16 . 18	26·3	22·2	374
10	37·8	1 . 10 . 0	16·9	17·4	705
11	37·3	1 . 3 . 20	22·8	22·2	567
large	44·7	8 . 15 . 8	...	...	3480

37 inches of the 9 first tubes, which are what was used in p. 123 [Art. 574], held together 3373 grains, therefore the shock was very nearly the same, but if anything rather greater when it passed through one tube, 37 inches of which held 3480 grains of ☿, than when it passed through 9 tubes, 37 inches of all which together held 3373 grains.

By p. 124 the shock is as much diminished in passing through 6·8 inches of a tube, 37 inches of which hold 567 grains, as through 44¼ of one, 37 inches of which hold 3480, so that resistance should seem as the 1·03 power of the velocity.

576] *Comparison of diminution of shock by passing through iron wire or through salt water*.*

In order to compare the conducting power of iron wire and salt water, the shock of two jars had its choice whether it would pass through 2540 inches of nealed iron wire, 12 feet of which weighed 14·2 grains, or through my body, each end of the iron wire being fastened to a pretty thick piece of brass wire which I grasped tight, one in one hand and the other in the other, and with them discharged the jars.

* [Art. 398 and Note 32.]

It was found that when the straw electrometer separated to 1 + 0, I just felt a shock in my wrists, and when it separated to 2 + 0, I felt a pretty brisk one in them but not higher up.

I then gave the shock its choice whether it would pass through my body, or 5·1 inches of a column of a saturated solution of sea salt contained in a glass tube, 1 inch of which holds 9·12 grains of fresh water, the wires running into the salt water being fastened to brass wires as before.

I found the shock to be just the same as before, and found too that increasing the length of the column of salt water not more than $\frac{1}{4}$ of an inch made a sensible difference in the strength of the shock.

Therefore the electricity meets with the same resistance in passing through 2540 inches of wire whose base is $\frac{142}{78 \times 144} = \frac{1}{79}$ as through 5·1 inches of salt water whose base is 9·12.

Therefore, if the resistance is as the 1·08 power of the velocity, the resistance of iron wire is 607,000 times less than that of a column of salt water of the same diameter*.

577] *Comparison of conducting powers of saturated solution of sea salt and distilled water.*

The shock of 1 jar charged till the straw electrometer separated to 1 + 0$\frac{1}{2}$, discharged through a column of $\begin{cases} \cdot 8 \\ 1 \cdot 0 \end{cases}$ inches of a mixture of saturated solution of sea salt with 99 of distilled water in tube 6, was $\begin{cases} \text{greater} \\ \text{less} \end{cases}$ than when it was discharged through 35$\frac{1}{2}$ inches of saturated solution of sea salt in tube 2.

By a former experiment, the shock passed through $\left.\begin{matrix} \cdot 87 \\ 1 \cdot 35 \end{matrix}\right\}$ of the mixed water was $\begin{cases} \text{greater} \\ \text{less} \end{cases}$ than through 40$\frac{1}{2}$ of saturated solution.

By a mean, the resistance of one inch of the mixed water is equal to that of 38 of the saturated solution, therefore allowing for the different bases of the tubes, the resistance of the mixed water is 39 times greater than that of the saturated solution.

The shock of two jars, charged to 4 + 0, and discharged through $\begin{matrix} \cdot 55 \\ 1 \cdot 8 \end{matrix}$ of distilled water in tube 5, was $\begin{cases} \text{greater} \\ \text{less} \end{cases}$ than when it was discharged through 23$\frac{1}{2}$ of the above-mentioned mixed water in tube 8.

By a former experiment, the shock passed through $\begin{cases} \cdot 8 \\ 2 \cdot 0 \end{cases}$ of distilled water was $\begin{cases} \text{greater} \\ \text{less} \end{cases}$ than through 23$\frac{1}{2}$ of the mixed.

* [If the resistance is as the velocity, resistance of saturated solution of salt is 355,400 times that of iron wire. By Matthiessen and Kohlrausch it should be about 502,500. See Note 32.]

By the mean, the resistance of 1·3 of distilled water = that of $23\frac{1}{2}$ of mixed.

10·9 inches of tube 5 in the place where used holds 120 grains of ☿, or 37 inches holds 408 grains, which is the same as tube 8: therefore the resistance of distilled water is 18 times greater than that of mixed, or 702 times greater than that of a saturated solution of sea salt.

578] *Whether the electricity is resisted in passing out of one medium into another in perfect contact with it.*

The 9th tube of P. 126 [Art. 575] was filled with 8* columns of saturated solution of sea salt inclosed between columns of ☿, the end columns being ☿. The tube 7 was filled with one short column of ☿ at the bent end, and a long column of saturated solution of sea salt.

It was found that the shock of one jar, charged till the straw electrometer separated to $1{\cdot}0\frac{1}{2}$, passed through a column of the salt water in tube 7, $\left.\begin{matrix}27{\cdot}7\\21{\cdot}2\end{matrix}\right\}$ inches long, was rather $\left\{\begin{matrix}\text{more}\\\text{less}\end{matrix}\right.$ diminished than in passing through the mixed column in tube 9, the wires used in tube 9 being immersed in the end columns of ☿, and those used in tube 7 being immersed one in the short column of ☿ at the end and the other in the column of salt water.

The length of the mixed column in tube 9 was 43·5 inches, its weight was 10·5, the weight of a column of ☿ of the same length was 18·10, therefore the sum of the lengths of all the columns of salt water was 21·8 inches, and by the experiment the shock was as much diminished by passing through 24·4 inches of salt water in tube 7 as through this. But as the bore of tube 7 in that part which was used was greater than tube 9 in the ratio $\frac{24{\cdot}4}{22{\cdot}3} \times \frac{36}{37{\cdot}4} = 1{\cdot}06$ to 1 nearly, the shock should be as much diminished in passing through a column 22·94 long in tube 9 as through one of 24·4 in this. Therefore the shock is as much diminished in passing through a mixed column, in which the length of salt water is 21·8 inches, as through a single column of the same size whose length is 22·94 inches.

The difference is much less than what might proceed from the error of the experiment.

579] A slip of tin was made consisting of 40 bits soldered together, all $\frac{1}{10}$ inch broad and all about $\frac{1}{5}$ inch long. They were made to lap about $\frac{1}{20}$ inch over each other in soldering. I could not perceive that the shock of a jar was sensibly less when received through this than through a slip of tin of same length and breadth of one single piece.

If the jar were charged pretty high and a double circuit made for it, namely through this piece of tin and my body, I could not perceive the least sensation.

580] *Made at Nairne's with his large machine.*

A long conductor was applied to the electrical machine and a smaller conductor to its end, a Henly's electrometer was placed on the middle of the long

* [8° in MS. Perhaps 80.]

conductor, and a small jar with a Lane's electrometer fastened to it was made to touch the short one. When Henly's stood at

30	Lane's dis-	17 + 35 = ·668
55	charged at	17 + 50 = ·678 inch.
70		19 + 30 = ·741

The jar was then changed for one of rather more coated surface and a much smaller knob. When Henly's stood at 30 or 35, Lane's discharged at 17·7 = ·650, so that Lane's discharged at nearly the same distance with the same charge, whichever jar was used.

Henly's electrometer was then placed on an upright rod, touching the long conductor near the furthest end, Lane's electrometer with the first jar being placed as before.

Henly then rose to 55 or 60 before Lane discharged at 17·55 = ·681 inch. Henly being then lifted higher it rose to 65, Lane remaining as before. It was then lifted still higher, when it rose to

65	before Lane's	17·55 = ·631
50	discharged at	9·55 = ·377
35 or 40		6·55 = ·263

Lane's being then separated to 27·55 = 1·060, the jar once discharged over surface of glass and once to the electrometer, but there seemed reason to think that Henly's rose no higher than before, namely 65.

My Henly's electrometer usually rose to 90 when Lane's discharged at 12·20 = ·467 inches.

Therefore the distance at which Lane's discharges, answering to different numbers on Henly's, is as follows:

		[Lane]
Henly on highest rod	65	1·060
	65	·681
	50	·377
	35 or 40	·263
Henly on conductor	.70	·741
	55	·678
	30	·668
My Henly on conductor	90	·469

The distance at which Lane's discharges with a given jar is nearly proportional to the quantity of electricity in the jar, for if a jar is charged to a degree at which Lane is found to discharge at a given distance, and its electricity is communicated to another jar of the same size, so as to contain only ½ as much electricity as before, Lane will then discharge at nearly ½ the former distance.

M[EASURES]

{From MS. N°. 20. See Table of Contents at the beginning of this volume.}

[These "Measures" are on a set of loose sheets of different sizes marked M. 1 to M. 21, and another set marked M. 1 to M. 12.]

581] *Comparative charges of jars and battery*.*

M. 1. If jar 1 is electrified till straw electrometer separates to $1\frac{1}{2}$, and its electricity is communicated to jars 2 + 4, pith electrometer separates $5\frac{3}{4}$. Therefore charge required to make pith balls separate $5\frac{3}{4}$ is to that required to make straw electrometer separate $1\frac{1}{2}$ as 3184 to 8909, and that to make pith separate $5\frac{1}{4}$ to that to make straw separate $1\frac{1}{2}$ as 2920 to 8909.

Jars 1 and 2 being electrified by wire and jar $\begin{cases}5\\6\\7\end{cases}$ by coating till pith electrometer separated $1\frac{1}{2}$ and a communication being then made between them in the manner used for trying Leyden vials, pith balls separate $\begin{matrix}5\frac{3}{4}\\5\frac{1}{2}\\5\frac{1}{4}\end{matrix}$ negative, therefore charge of jar $\begin{matrix}5\\6\\7\end{matrix}$ should be $\begin{cases}1316\dagger\\1273\\1231\end{cases}$.

Charge of 1 + 2 + 3 + 4 = 12544.

Jars 1 + 2 + 3 + 4 being compared in the same manner with jar $\begin{cases}5\\6\\7\end{cases}$ the pith balls did not separate at all.

M. 2. If the charge of jars 1 + 2 + 3 + 4 is called 4

jar 1 or 2 is nearly	=	1
5, 6, or 7	=	4
1 row of battery	=	22
whole battery	=	154

Jar 8 being electrified it was found that it must be touched $7\frac{1}{2}$ times by white cyl. to reduce the quantity of electricity to $\frac{1}{2}$. The 4 jars must be touched $8\frac{1}{2}$ times by do. Therefore charge of jar 8 = $3\frac{1}{2}$.

A piece of crown glass 1 foot square of which weighed 10·12 was coated with tinfoil about 10 inches square.

* [Art. 411.]

† [These numbers are given as in the MS. They should be each multiplied by 10. See also Art. 585, where the numbers seem to be deduced from some other experiment.]

M. 3. The charge of each row of the battery was found by charging to a given degree by electrometer and touching it repeatedly with jar 4 till the separation of electrometer was reduced to that answering to $\frac{1}{2}$ the charge.

*The 1st, 2nd, 3rd, 4th, 5th, 6th, 7th row required to be touched 18, 19, 17, $18\frac{1}{2}$, 17, 17, 18 times, therefore charge

1st row	= 26	charge of jar 4
2nd	= 27·4	
3rd	= 24·6	
4th	= 26·7	
5th	= 24·6	
6th	= 24·6	
7th	= 26	

and charge of whole battery = 180 times that of jar 4

and real charge = 321000

and if real charge by computed of white glass = 7·5,

computed charge = 42800

which answers to 187 square feet of glass whose thickness = $\frac{1}{10}$.

Therefore charge of jar 4 answers to 1·04 square feet of D° thickness. The coating is about $\frac{7}{12}$ of a square foot, and therefore the mean thickness = ·058.

582] M. 4. Let jar be touched n times† by jar which is to first as x to 1, it will be reduced in ratio of 1 to $(1+x)^n$, therefore if it is reduced to $\frac{1}{2}$ thereby

$$(1+x)^n = 2.$$

Therefore let $\quad$ N. L $2 = a$ and N. L $(1+x) = px$,

$$pxn = a,$$

and
$$\frac{1}{x} = \frac{pn}{a};$$

but
$$\text{N. L }(1+x) = x - \frac{x^2}{2}\text{ nearly}, = x\left(1-\frac{x}{2}\right),$$

therefore
$$p = 1 - \frac{x}{2}\text{ nearly}, = 1 - \frac{a}{2pn}\text{ nearly},$$

therefore
$$\frac{1}{x} = \frac{n}{a}\left(1-\frac{a}{2n}\right)\text{ nearly},$$

$$= \frac{n}{a} - \tfrac{1}{2}\text{ nearly},$$

whence we have the following

Rule for finding ratio of charge of 2 jars, supposing the charge of first to be reduced to $\frac{1}{2}$ by touching n times by 2nd.

Charge of 1st is to that of 2nd :: $1·444n - \frac{1}{2}$ to 1.

* N.B. The left-hand row is supposed to be called the 1st row. [If Jar 4 = 2675 circ. inc. (see Art. 506) whole battery = 481,500 circ. inc. or 321,000 glob. inc., counting 1 glob. inc. = 1·5 circ. inc., as Cavendish seems to do here.]

† [Art. 413.]

583] M. 5.

$$\begin{aligned} \text{jar } 1 &= 3184 \text{ [circ. inc.]} \\ 2 &= 3050 \\ 3 &= 3635 \\ 4 &= 2675 \\ 5 &= 11816 \\ 6 &= 12544 \\ 7 &= 11816 \\ 8 &= 10761 \\ 1^{\text{st}} \text{ row} &= 64538^* \end{aligned}$$

Quantity of electricity communicated to whole battery† by

$$\begin{aligned} B + 2A &= 3{\cdot}61 + 2A \\ 2B + 2A &= 7{\cdot}07 + 2A \\ 3B + 2A &= 10{\cdot}36 + 2A \\ 3B + C + 2A &= 13{\cdot}16 + 2A \\ R \quad &= 20{\cdot}58 \\ R + B &= 23{\cdot}66 \qquad D = \tfrac{1}{2} \\ R + 2B &= 26{\cdot}74 \\ R + 3B &= 29{\cdot}83 \end{aligned}$$

Quantity of electricity communicated to 1st row by

$$\begin{array}{ll} A = {\cdot}95 & \left.\begin{aligned} B + 2A &= 2{\cdot}56 \\ 2B + \quad &= 4{\cdot}58 \\ 3B + \quad &= 6{\cdot}17 \\ \{?4\}\ 3B + \quad &= 7{\cdot}54 \end{aligned}\right\} + 2A \\ 2A = 1{\cdot}8 & \\ 3A = 2{\cdot}6 & \\ 4A = 3{\cdot}3 & \end{array}$$

Charge of 1st battery of Nairne.

584] M. 6. Electricity of 1st row of old battery was reduced to $\frac{1}{2}$ by touching $11\frac{1}{4}$ times by crown glass of 10 inches square. Therefore charge of 1st row to that of crown glass as $15\frac{3}{4}$ to 1. The first row of new battery appeared by that means to contain 10·7, the 2nd row 11, and the 3rd 11·4 times the charge of the same plate.

The mean area of the convex coating of each jar seemed to be $14 \times 12\frac{1}{2} = 175$ inches, to which adding 5, *id est* $\frac{5}{12}$ of area of bottom, whole coating may be estimated at 180 square inches of same thickness as sides.

Elect. $\begin{cases} 1^{\text{st}} \\ 2 \\ 3 \end{cases}$ row of new battery was reduced to $\frac{1}{2}$ by touching $\begin{cases} 10 \\ 10\frac{1}{2} \\ 10 \end{cases}$ times by jar 1, therefore charge = jar $1 \times \begin{cases} 13{\cdot}94 \\ 14{\cdot}66, \\ 13{\cdot}94 \end{cases}$ and charge mean row = jar $1 \times 14{\cdot}18 = 45{,}149$ inc. el.

* [See Art. 506.]

† [Here A seems to be the charge of one of the first 4 jars taken as unit, B that of one of the others taken as 4, and R that of the row taken as 22, the battery being 154, as in M. 2.]

By top leaf its charge should be $\frac{64538 \times 11{\cdot}33}{15{\cdot}75} = 46390$ inc. el., therefore its computed charge $= \frac{45150}{1{\cdot}5} = 30100$, and thickness of glass should seem $= \frac{180 \times 6 \times \frac{4}{3} \times \frac{21}{20}}{30100} = \frac{1}{20}$ inc.

585] M. 7. *Whether shock of battery is sensibly diminished by imperfect conduction of the salt water in the jars.*

An uncoated glass jar like the coated ones was filled with fresh water and put into a glass jar of fresh water, a brass wire with knob being put into it, and a slip of tinfoil into the outer jar, it was charged till straw electrometer separated to 8 and tried by shock melter* filled [with] sea water, wires about 3 inc. dist.

The water in inner jar was then changed for sat. sol. s. s.† and that in outer for about equal parts of D° and fresh water, and tried in the same manner. The shock seemed rather greater, but was plainly less when electrometer was at 7.

When shock was taken without shock melter* it was as strong with el. at 5 as with D° at 8. Jar 2 being charged to 8 and its electricity communicated to jar, the electrometer separated to $4\frac{1}{2}$.

586] M. 8. Feb. 28, 1775.

Specific gravity bottle filled with salt water from torpedo trough weighed 8.4.18 by ingraved weights. Th. at 49. Specific gravity = 1·0254.

Being mixed with $\frac{707}{2000} = {\cdot}3525$ its weight of rain water, specific gravity bottle weighed 8.4.1, Th. at $49\frac{1}{2}$, specific gravity 1·0190.

Excess of specific gravity above unity of stronger is to that of weaker as 1·335 to 1. The quantity of salt in them is as 1·3524 [to 1].

Therefore the excess of specific gravity above 1 differs pretty nearly, but not quite, in as great a ratio as the quantity of salt in them.

M. 9. April 1. D° Specific gravity bottle with water from torpedo trough weighed 8.4.22 by D° weights.

April 29. Torpedo trough filled with water to within 1 inch of top, and 58 oz. salt added.

Specific gravity bottle filled therewith, Th. at 70°, weighed 8.4.12. At $54\frac{1}{2}$ same water weighed $8.4.16\frac{1}{2}$.

One bottle of sea water weighed 8.4.11, Th. at 67. Another bottle weighed $8.4.19\frac{1}{2}$, Th. D°.

Specific gravity bottle with rain water weighs $8.1.22\frac{1}{2}$.

[M. 10 blank.]

* [This word occurs also in Arts. 622 and 637. See facsimile at Art. 622.]

† [Saturated solution of sea salt.]

M. 11. *Rule for finding the quantity of salt in water by its specific gravity.*

Let the specific gravity of the solution at $46\frac{1}{2}$ = S, and $\frac{\text{quantity of salt}}{\text{solution}} = x$. If S is above

$$\left.\begin{array}{l} 1{\cdot}0675 \\ 1{\cdot}0261 \\ 1 \end{array}\right\} \frac{1}{x} = \left\{\begin{array}{l} \dfrac{\cdot779}{S - 1 + \cdot0033} \\ \dfrac{\cdot719}{S - 1 + \cdot0022} \\ \dfrac{\cdot789}{S - 1} \end{array}\right.$$

$$\begin{aligned} \cdot779 &= 9{\cdot}8917 \\ L\ \cdot719 &= 9{\cdot}8568 \\ \cdot784 &= 9{\cdot}8942 \end{aligned}$$

vide Heat p. 98*.

587] In 2nd Lane's electrometer or 1st detached do.

40 threads screw = $1\frac{1}{2}$ inches, or 1 division of plate = $\frac{1}{1600}$ inch.

For 3rd Lane D°.

588] M. 12. June 11.

	For salting†	Not salting
Mahogany	2 . 19 . 5	3 . 15 . 15
Wainscot	2 . 16 . 10	3 . 8 . 21
Beech	3 . 11 . 1	4 . 5 . 8
Ash	3 . 14 . 10	4 . 7 . 9
Alder	2 . 4 . 10	2 . 13 . 12
Lime	2 . 11 . 8	2 . 18 . 13
Deal	2 . 12 . 14	3 . 4 . 22

Weight of the unsalted ones on June 18, and number of vibrations of a pendulum ‡ inches long, in which the electricity of 1 row of the battery was reduced from 2 to 1 by pith balls by touching with them, the ends being wrapt round with tinfoil fastened on with gum.

	Weight	Number of vibrations	Loss of weight
Mahog.	3 . 14 . 12	34	1 . 3
Wainscot	3 . 7 . 20	19	1 . 1
Beech	3 . 15 . 5	36	10 . 3
Ash	4 . 0 . 16	6	6 . 17
Alder	2 . 11 . 18	200	1 . 18
Lime	2 . 15 . 14	22	2 . 23
Deal	3 . 3 . 12	60	1 . 10

* [Mr Vernon Harcourt, in his Address to the British Association (*B.A. Report*, 1839, p. 48), has given extracts from Cavendish's MS. on Heat, p. 1 to p. 50, but he does not mention any page 98.] {See Vol. II of this Edition.}

† [Art. 609.] ‡ [So in MS.]

M. 13. The salted ones taken out of water two or three hours weighed

Mahogany	3 . 15 . 14
Wainscot	3 . 9 . 0
Beech	4 . 8 . 0
Ash	4 . 14 . 12
Alder	3 . 9 . 22
Lime	3 . 17 . 11
Deal	3 . 1 . 4

June 19th. Bits of tinfoil were fastened round the ends of these pieces of wood with gum.

2B being electrified to $1\frac{1}{2}$ and its electricity communicated to the whole battery gave a slight shock when received through the Lime, Alder, Ash and Beech, but most through the Lime.

1R + 3B through wainscot and 2R + 3B through deal gave much the same shock, and 3R was just sensible through Mahogany.

589] M. 14. Dimensions of coatings made to pieces of glass D, E, F, G; A, B, C, I, K, L, M, H. [See Art. 324.]

M. 15. Bluish-green ground glass from Nairne called R and S,

M. 16. Logarithms for calculations of these plates.

M. 17. Do.

M. 18. Straight piece of elect[rifying] wire, thickness ·15, length 30.

Increase of quantity of electricity in wire of that thickness by increasing its length from 33 to 53 inches = 4·53 inches, therefore increase of quantity of electricity in wire = increase length × ·226.

The two trial plates of white plate glass, which contain together 66 inches of computed power, or 66 × 1·6 inches of electricity, were balanced by twice the sum of the double plates A and B + 48 of additional wire = 73·4 + 10·85 = 84·2 inc. el., therefore 10·7 inc. el. is to be allowed for the usual length of the wire.

[*Rules for making trial plates.*]

M. 19. By p. 21 [Art. 459] it should seem that the difference between two trial plates ought to be to their sum as L + S + 20 inc. of wire to L + S, or as $3{\cdot}6 + \dfrac{20 \times {\cdot}226}{1{\cdot}6}$ to 66, or as 32 : 330.

The plates $\begin{cases}\text{D}\\\text{E}\\\text{F}\end{cases}$ of Nairne are to be coated with circles $\begin{cases}2{\cdot}16\\2{\cdot}16\\2{\cdot}19\end{cases}$ in diameter.

The mean quantity of electricity in the trial plates should be 47·4 inc. if nothing is allowed for additional wire, therefore if this is increased by $\frac{1}{20}$ to allow for uncertainty, and the plate $\frac{\text{A}}{\text{B}}$ is used for $\frac{\text{small}}{\text{large}}$ trial plate, the computed power should be $\frac{27{\cdot}84}{33{\cdot}68}$, and [M. 20] the coating should be a square whose side $= \frac{2{\cdot}123}{2{\cdot}27}$.

Quantity of electricity in small thin plates to be 110·1, computed power = 67·8, diameter of circles to be

$$\begin{aligned} I &= 2{\cdot}299 \\ K &= 2{\cdot}286 \\ L &= 2{\cdot}358 \\ M &= 2{\cdot}207 \end{aligned}$$

The trial plates to them to be made of plates I and L $\frac{\text{quant. el.}}{\text{comp. power}}$ of that glass supposed = 1·95.

Computed power of mean between the two plates to be $\frac{120{\cdot}8}{1{\cdot}95} \times \frac{11}{10} = 68$ = a square of 1·933, the thickness of the glass being ·07, therefore

$$\left.\begin{matrix}\text{Small} = L \\ \text{Large} = I\end{matrix}\right\} = 1{\cdot}933 \text{ long and } \begin{cases}2{\cdot}126 \\ 1{\cdot}\ 74\end{cases} \text{broad.}$$

$$\text{Oblong coatings } 1{\cdot}82 \text{ long and } \left\{\begin{matrix}1{\cdot}62 \\ 1{\cdot}74 \\ 2{\cdot}02 \\ 2{\cdot}14\end{matrix}\right. \begin{matrix} \\ \text{were made to} \\ \text{old ground plates} \\ \ \end{matrix} \left\{\begin{matrix}L \\ I \\ H \\ K\end{matrix}\right.$$

M. 21. $\left.\begin{matrix}\text{Small} \\ \text{Large}\end{matrix}\right\}$ trial plate for large thick plates $\begin{cases}F \\ E\end{cases}$ to be coated with oblong square $\begin{cases}5{\cdot}7 \\ 6{\cdot}6\end{cases}$ by 6.

Dimensions of trial plates.

	Length	Breadth
A	16·4	11·8
B	13	9·7
C	10·4	7·8
D	8·5	6·4

Sliding Plates.

Value of 1 division in

Number of Plate	Inc. el.	Parts of D
1st	·75	·0208
2nd	1·50	·0416
3rd	3·37	·0940
4th	8·57	·238
5th	19·0	·530
6th	18·0	·500
1 inch additional wire	·226	·0063

[From this table it appears that D is supposed to contain 36 "inc. el." Now, by Art. 655, D contains 26·3 "globular inches," which is equal to 41 "circular inches," or 36·7 "square inches."]

[Specimen of Measurements of thickness by dividing engine.]

590] 2nd rosin plate, [Arts. 371, 500] mean diameter 5·6.

M. 23. At 2·110 knobs coincided,

$$\left.\begin{array}{r} 2{\cdot}306 \\ 05 \\ 06 \end{array}\right\}\text{ center}\quad 2{\cdot}3057$$

$$\left.\begin{array}{r} 2{\cdot}306 \\ 05 \\ 04 \\ 04 \\ 04 \\ 04 \end{array}\right\}\text{ one inch [from center] } 2{\cdot}3045$$

2·110 coinc.

2·110 coinc.

$$\left.\begin{array}{r} 2{\cdot}306 \\ 04 \\ 06 \\ 05 \\ 00 \\ 02 \\ 02 \\ 00 \\ 02 \\ 02 \\ 02 \\ 01 \end{array}\right\}\begin{array}{l}1\tfrac{1}{2}\text{ inc. from center} \\ \qquad 2{\cdot}303\end{array}$$

2·110 coinc.

Mean thickness = ·195.

591] M. 32. Measures of thickness of crown glass [Arts. 370, 500] measured in the middle of each 16th square part, the numbers being placed in the same situation as the squares.

C. At 1·881 the knobs coincided.

1·944	48	51	48
48	52	52	44
53	53	46	38
53	46	40	34

mean 1·947, thickness ·066.

{From MS. N°. 13.}

592] M. 1*. *List of Plate glasses.*

First got.

	[Thickness]	[Side]	
A	·205	4·02	Made into trial plates and cased in cem[ent] of a greenish colour inclining to blue with a great want of transparency.
B	·193	3·99	
C	·162	4·01	not used.
D	·16	3·98	coated, comp. pow. = 46.
E	·19	8·06	Same sort of glass as A and B, but from their greater length and breadth I could not so well judge of their colour. Made up into trial plates.
F.	·204	8·03	
G	·184	8·04	Same kind as C and D. Remains not approp.
H	·062	4·35	H is marked on side with single scratchof file, I with 2, and so on to L.
I	·066	4·36	
K	·063	4·34	Made into trial plates and broke except L.
L	·068	4·37	More transparent than the thick plates.
M	·055	8·68	not used.

The thickness was measured in the middle of each side, beginning with that side to right of letter, the letter being held towards eye.

	Thickness	Diam. coating	Comp. power
Double plate ground glass A	·3	1·82	11·04
B	·31	1·855	11·1

1st *got from Nairne.*

2 thickish plates of 8 inc. made into trial plates for 2nd sort, called S and L.

Another cut into 4 pieces for trying effect of different varnishes.

A 4th not used.

2 thin ones of bluish glass coated in order to serve for trial plates to largest plate, and called L and S, but not used.

2 thick plates and 1 thin one rough.

2 white glass plates from Nairne.

4 pieces out of same piece with different sorts of surface.

A large piece of whitish plate glass divided into 3 pieces, one used for sliding trial plate.

4 irregular shaped pieces called N, O, P, Q. [Art. 459.]

2 thinnish pieces 5 inches square with very thin plate rosin between.

M. 2. 1 piece of much the same kind fastened to piece of crown glass with cement between, used for sliding plate.

* [Here follows another series of measures on loose leaves of different sizes.]

593] M. 3. *Nairne's plates of same piece**.

	Out of water	Loss in water			
4 thin pieces	7. 1.14	2.12.21	7·079	2·644	2·6774
4 thick do.	19. 3.11	7. 3. 4	19·173	7·158	2·6785
large thin piece	7. 0. 2	2.12. 7	7·004	2·615	2·6785
A	19. 6.17	7. 4.10	19·335	7·221	2·6776
B	19.12.11	7. 6.13	19·623	7·327	2·6782
	19. 1.16	7. 2.13	19·083	7·127	2·6775
					Mean 2·6779

1 cub. inc. water = ·5278 oz.
1 oz. of glass = 9·84973 cub. inc.

[The weights in 1st & 2nd column are in ounces, pennyweights and grains (Troy), in the 3rd and 4th in decimals of an ounce, and the 5th is the specific gravity. The number 9·84973 is the logarithm of the number of cubic inches in an ounce of glass.]

M. 4. [Gives 1st the length of each side of each piece of glass, and the distance between the middles of opposite sides to hundredths of an inch.

2nd the thickness at each corner and middle of each side to thousandths of an inch.

3rd specific gravity and mean thickness deduced from it for plates A to M of Nairne.

The results are given in M. 5. The thicknesses are as follows:

	Thickness calculated	Measured	Diff.
A	·2112	·2095	·0017
B	·2132	·2109	23
C	·2065	·2057	8
D	·2057	·2047	10
E	·2065	·2055	10
F	·2115	·2101	14
G	·2022	·2103	9
H	·07556	·0735	21
I	·07797	·0759	21
K	·07712	·0755	16
L	·08205	·0804	16
M	·07187	·0707	12

[The thicknesses given in Art. 324 are those calculated from the weight in and out of water and the measurement of the sides. They are greater than the measured thicknesses in every case.]

594] M. 6. *Measures of thickness &a of green glass cylinder.*

Longest cylinder: a mark made with file near middle.

The 1st column is the distance in inches of the point to which cylinder is immersed in water from the scratch.

* [Art. 314.]

The 2nd column is weight required to balance it in that position.

The 3rd is the same thing in the 2nd trial.

The 4th is the difference of these numbers, or bulk of intermediate portion of glass.

The 5th is the same thing in 2nd trial, and

The 6th is the mean between them.

The 7th is the circumference in the middle of that space.

Towards wide end	1st tri[al]	2nd tri.	Bulk int. space by 1st	2nd	Mean	Circum.
13	11 . 12 . 15	12 . 22				
			1 . 13	1 . 12	1 . 12 . 5	3·595
12	11 . 11 . 2	11 . 10				
			2 . 22	2 . 22	2 . 22	3·435
10	11 . 8 . 4	8 . 12				
			2 . 18	2 . 17	2 . 17 . 5	3·265
8	11 . 5 . 10	5 . 19				
			2 . 20	2 . 21	2 . 20 . 5	3·140
6	11 . 2 . 14	2 . 22				
			2 . 16	2 . 18	2 . 17	3·020
4	10 . 19 . 22	17 . 10				
			2 . 18	2 . 18	2 . 18	2·940
2	10 . 17 . 4	17 . 10				
			2 . 23	2 . 21	2 . 22	2·905
0	10 . 14 . 5	14 . 13				

[after this a table for the narrower half of 1st cylinder, and in M. 7 for 2nd, 3rd and 4th cylinders. M. 8 and M. 9 is a table of 11 columns.

1st column, distance from mark.

2nd Mean loss [of weight] for 2 inches.

3rd Supp[osed] mean circumference.

4th Log. loss.

5th Log. supp. circ[umference].

6th Log. thick[ness] × p.

7 Thick. × p [p = ratio of circumference to diameter].

8 True mean circ.*

9 Log. do.

10 Log. comp. power of 1 inch.

11 Comp. power of 1 inch.]

M. 10. Measures of the circumference and substance of glass in jars and cylinder.

Marks with file are made at the extremities of the whole space, and the numbers begin with the space marked with double mark.

* By mean circumference is meant the mean between the inside and outside circumference.

The circumference was measured by a slip of tinfoil put round, and the intersection marked with knife.

The substance of glass was found by hanging it to end of sliding ruler fastened to one end of balance, and weighing it in water; and by sliding the ruler I made more or less of it to be immersed, and knew the difference of the space immersed.

M. 11. *Specific gravity of different pieces of white glass.*

Large jar	3·253	3·253
small D°	3·256	3·257
long cylinder	3·281	3·279
thick flat glass	3·280	3·279
thin do.	3·280	3·284

The small jar being broke, a 2nd was measured.

Thickness measured by calipers in 4 different rows parallel to axis and in 5 different places in each row, beginning at a scratch with a file near bottom.

[Here follow the measures.]

The thickness was then tried in 4 different parts of circumference at 4·4 inc. distance from scratch.

It was then weighed in water in the same manner as the others.

The jar was dried before each trial, and before the 3rd was rubbed with solut. p. ash*, which made the water stick less to side, for which reason it is supposed most exact.

The circumference was measured in two parts of the middle space, and they came out both the same.

595] M. 12. *Measures of coatings to jars and cylind.*†

A coating made to 2nd small jar extending to 4·4 inches from scratch. Comp. power = 680·7.

Coating to white cylinder extends 9·86 inches from double mark. Comp. power = 684·1.

A coating made to 4th green cyl. extending 7 inches from mark. Comp. power = 318·2.

A mark was made on wide part, extending 7·16 inc. from new mark. Comp. power 600·7.

M. 13. A mark made on 2nd green cylinder 11 inches from first towards thick end, and the tube cut off about 1 inch from 1st mark.

A coating made to the thick part extending 8·55 inches from 2nd mark. Comp. power = 600.

* [Pearl ash.]

† [See Art. 383. The computed power here is 8 times the true value, and there is no correction for spreading of electricity.]

EXPERIMENTS WITH THE ARTIFICIAL TORPEDO [1775]

{From MS. N°. 20. See Table of Contents at the beginning of this volume.}

596] Torp. 1 in water touching sides*.

3 rows $1\frac{1}{2}$†	felt plain shock	in	hands.
4 —	more brisk	in	D°.
7 —	more violent	in	D°.
2	plain	in	D°.
1	sensible.		
+ 2 + 3‡			
4 + 1 + 5 + 6 + 7	—but just sensible.		

Out of water.

4 + 1	uncertain.
4 + 1 + 2	sensible.
4 + 1 + 2 + 3	D°.
5 + 4 + 1	sensible in elbows.
5 + 6	gentle in elbows.
5 + 6 + 7 + 1 + 2 + 3 + 4	strong in elbows.
1 row	more violent.

Uncoated, out of water.

4	scarce percept.
4 + 1	sensible.
4 + 1 + 2	gentle.

In water.

5 + 6 + 7	perceptible.

Without any torped. jar 4 was perceptible.

I could not perceive any sensible difference in the conducting power of the water I used & of sea water, but the difference caused by mixing $\frac{1}{11}$ part of rain water with the sea water was scarcely perceptible.

$$\text{§N.B. resistance of} \left\{\begin{array}{l}\text{distilled water}\\ \text{sat. sol. in } \frac{99}{29}\\ \text{sat. sol.}\end{array}\right\} \begin{array}{c}\text{are to each}\\ \text{other nearly}\\ \text{as}\end{array} \left\{\begin{array}{r}1\\ 18\\ 100\\ 702\end{array}\right.$$

* [Art. 415.]

† [3 rows of battery electrified till the electrometer separated to $1\frac{1}{2}$.]

‡ [These numbers are those of the jars of the first row of the battery. See Art. 583.]

§ [This should be conducting power, instead of resistance. The numbers then agree with those in Art. 684.]

so that there seems no reason to think that the resistance of water about the saltness of sea water varies in a quicker ratio than that of the quantity of salt in it.

Without torpedo jar 1 + 2 + 4 was very sensible in elbows, but 1 + 2 was felt only in wrists.

597] Let a given charge be passed by double circuit through your body and another circuit; let the quantity of electricity which passes along the second circuit be to that which passes through your body as x to 1; the rapidity with which the fluid passes through your body is the same whatever is the value of x, and the quantity which passes through your body is* as $1 + x$.

If the resistance which the electricity meets with before it comes to the double circuit is to that which it would meet with in passing through your body alone as a to 1, the force required to drive electricity through the whole circuit in given time is as $a + \frac{1}{1 + x}$, and therefore the time in which it is discharged $= \frac{1}{a + \frac{1}{1+x}} = \frac{1 + x}{1 + a + ax}$, and the velocity with which it passes through your body is as $\frac{1}{1 + a + ax}$, and the strength of shock is as

$$\frac{1}{(1 + x)(1 + a + ax)}.$$

In trying resistance of liquors by double circuit, if the quantity of electricity which passes through the liquor is to that which passes through your body as x to [1], the quantity of electricity which passes through your body is as $\frac{1}{1 + x}$, and the rapidity with which it passes through your body is given.

In trying it with single circuit, if resistance el. in passing through liquor is to that in passing through your body as x to 1, velocity of electricity is as $\frac{1}{1 + x}$, and the quantity is given, therefore in both ways of trying it, the greater x is, the more exact will be the method, and both methods will be equally exact if x is given or very great, supposing the strength of the shock to be as the quantity of electricity into its velocity†.

598] Shock produced by charge $\begin{cases} 16 \\ 22 \\ 44 \end{cases}$ in water bears the proper proportion to that caused by charge $\begin{cases} 6 \\ 8 \\ 16 \end{cases}$ out of water.

* [Should be *inversely* as $1 + x$. The rest is a correct statement of the strength of derived currents according to the law afterwards published by Ohm. See Art. 417.]

† [The "velocity" is what is now called the strength of the current. The strength of the shock is assumed to be proportional to the energy of the discharge. See Arts. 406, 573, 610, and Note 31.]

It is supposed that it required about $2\frac{3}{4}$ the charge to give a proper shock in water as [it does] out, or it is supposed to require 5 times quant. el. It is supposed too that it requires 2ce charge of 3 times quant. el. to give same shock with torp. out of water as without torp.

Let quant. el. which passes through $\begin{cases}\text{body}\\ \text{torp.}\\ \text{water}\end{cases}$ be as $\begin{cases}1\\ 2\\ a\end{cases}$, if quant. el. which passes through torp. is increased to $2n$, quant. el. which passes through your body $\begin{cases}\text{in wat.}\\ \text{out wat.}\end{cases}$ will be $\begin{cases}\dfrac{1}{2n+1}\\ \dfrac{1}{a+2n+1}\end{cases}$, therefore

$$\frac{a+2n+1}{2n+1} \text{ must } = \frac{a+2+1}{3\times 5}, \text{ or}$$

$$15a + 30n + 15 = 2na + 6n + a + 3,$$

$$n\,(2a + 6 - 30) = 14a + 12,$$

$$n = \frac{14a+12}{2a-24}, \text{ which if } a = 60, \text{ is}$$

$$= \frac{14a}{2a \times \frac{4}{5}} = \frac{7\times 5}{4} = 9,$$

and therefore it should require about 9 times quant. el., or about $5\frac{1}{4}$ times the charge, to give the same shock out of water as at present.

599] Tu. Mar. First leather Torpedo*.

Out of water.

1 row jars el. to $1\frac{1}{2}$ by straw el. and commun. to rest, a shock just sensible in elbows.

$1+2+3+4+5+6+7$: just sensible in hands.

Do + 1 row: stronger than No 1.

In water.

2 rows: plain in hands,

1 row: just sensible,

3 rows: rather stronger than 2 Do out of water.

600] Tu. Apr. 4 [1775]. 2nd leather Torpedo.

Out of water,

$5+6+7$: very slight in fingers.

2 rows: only in hands, there seemed to be something wrong.

4 rows: brisk in elbows.

2 rows: briskish in elbows.

* [Art. 416.]

In water.

2 rows just sensible in hands.
3 rows stronger.
4 rows pretty strong D°.

1st leather Torpedo in water.

4 rows nearly same, but I believe not so strong as last.
2 rows very slight.

Out of water.

2 rows slight in elbows.
4 rows strong in elbows.

601] Sat. May 27 [1775] with 2nd leather Torpedo under water, 3 rows charged to 1½ on electrom.*

Shock with one hand to one person seemed stronger, to another weaker than with both.

Communication being made with metals instead of the hands, no shock was felt, but when all the rows were charged to 3, Mr Ronayne felt a small shock.

With wooden Torpedo, 1 row to 1½; shock passed across 27 links of heavy chain with light. It also passed across 4 links of small chain with light, but not across 6.

Without Torpedo, 5 + 1 + 2 to 1½; shock passed with light through electrom., no candle in room; also with torp. charged as in trial.

On a former night in trying wooden Torp., charged I believe much the same as this time, no light was perceived, though Mr Hunter felt a shock, but very weak.

One candle in room, hid as well as possible behind screen.

With Gymnotus, all rows charged to 3½.

Doubtful.

Dr Priest[ley] and Mr Lane touching with 1 hand at same time, Dr Priestly felt shock extend to elbow.

A former night, 3 rows charged to 1½, Mr N. thought the shock extended to elbow; no one else thought so.

Sat. May 27 [1775].

602] Old Torp. out of water 2B + A (8·1)†, tried with metals, weak shock.

New torp. B + A (4·6) as strong as former.

The old Torp. tried with one hand holding metal against bottom side, in other hand holding bright link.

* [Art. 419. The (artificial) Gymnotus is not elsewhere mentioned.]

† [The numbers in brackets are the charge communicated to the battery or the row. See Art. 583.]

3A (3) no shock, with B (3·6) a very slight shock when torp. was just wetted, none else.

With long link, not bright, 3A, (3), sometimes left it, not always; with 2A never.

With wire of same size bright without link, seemed not to feel it so well.

With small link not bright no shock with B + A (4·6), but there was with B + 2A (5·6); with bright wire without link felt shock with B + A (4·6), but not with B (3·6).

With dirty link 2B + A (8·1), sometimes a small shock, not always; with 2B + 2A (9·1), certain.

603] Tried with Lane's electrom.; dirt unaltered.

Rows of batt. to which el. is comm.	Jars el.	Equiv.*	
7	R + 2B	26·7	shock.
	R + B	23·7	none.
1	B + 2A	4·56	small shock.
	B + A	3·56	none.
7	R + B	23·7	shock.
	R	20·6	none.
7	R + 2B	26·7	shock.
	R + B	23·7	none.
1	B + 3A	5·6	shock.
	B + 2A	4·6	none.
7	R + 2B	26·7	shock.
	R + B	23·7	none.
1	B + 3A	5·6	shock.
	B + 2A	4·6	none.

Tu. May 30 [1775].

604] It was tried whether distance on Lane's electrom. at which jars discharged was the same at the same separation of straw & pith ball electrom. whether number of jars was great or small †.

This was tried first by laying small knob'd Lane on wire while jars were charging, and afterwards by charging jars, without Lane lying on wire, to a little greater and little less degree by electrom. than what it was before found that they discharged at; then touching them with Lane, I could not perceive that the number of jars made any difference.

It was tried by comparing 1 & 4 jars with straw el. at 2 and by comparing 1 and 7 rows of battery with pith balls at 1.

It was also tried whether the number of jars electrified affected the separation of straw el.: by connecting 4 jars to the wire & then withdrawing 2 of them. It was not found to be at all affected.

* [See Art. 583.] † [Art. 402.]

Tu. May 30 [1775].

605] *Charge required to force el. through 4 links of small chain, and also through 2 loops of machine*, 5 links of chain in each loop.*

Rows of Batt.	Jars el.	Equiv.		
7	R	20·6	passed through 4 links	old torp.
	3B + C + 4A	17·1	did not	
	R	20·6	passed through 2 loops	
	3B + C + 4A	17·1	did not	
	3B + C + 4A	17·1	passed through 4 links	new torp.
	2B + 4A	11·1	did not	
	R	20·6	passed through 2 loops	
	3B + C + 4A	17·1	did once, failed once	
	R	20·6	passed through 4 links	2nd leather torp.
	3B + C + 4A	17·1	did not	
	R + 3B + C	33	passed through 2 loops	
	R	20·6	did once, failed once	
	3B + C + 4A	17·1	did not	

leather torp.	R	= 20·6	gave same shock.
old	2B + A	= 8·1	
new	B + 3A	= 6·6	
without torp.	A + D	= 1·5	

Tried with new Torpedo.

Rows Batt.	Jars el.	Equiv.	
7	B + A	4·6	gave same shock.
1	3A + D	3	
7	2B + 2A	9·1	gave same shock.
	B + 3A	5·4	

Trial of charge required to pass through 4 links of chain.

1	3A	2·6	sometimes passed, sometimes not.
7	3B + C + 4A	17·2	D°.
1	4A	3·3	passed.
7	R	20·6	passed.
7	3B + C + 4A	17·2	did not.
1	3A	2·6	

Tried with 2 loops of machine†.

[Rows	Jars el.	Equiv.]	
7	3B + C + 4A	17·2	did not.
1	4A	3·3	
1	B + A	3·6	did.
7	R	20·6	
1	B + 2A	4·6	did not.
1	B + 3A	5·6	did.
7	R	20·6	did.
1	B + A	3·5	did not.
1	B + 2A	4·5	did.
1	B + A	3·5	did not.
1	B + 2A	4·5	did.

* [Art. 433.]   † [Arts. 433, 605.]

$3B + C + 2A = (9{\cdot}2)$ commun. to 1 row, 2ce passed through 2 loops, once missed, once did not pass through 3, never through 5.

R communicated to 7 rows = 20·6 was tried 3 times without ever passing through 3 rows.

Wed. May 31 [1775].

606] 1 jar was elect. and commun. to 1 row of battery, and shock taken without torpedo. There seemed a little difference in the strength of the shock according to which row it was communicated to, but hardly more than was observed at different times from the same row.

Result of exp. May 30.

607] By mean, quant. el. req. to give same shock with 7 rows is to that with 1 :: 18·3 : 11·5 :: 1·6 : 1*.

Charge req. to force through $\begin{cases}\text{4 links}\\\text{2 loops}\end{cases}$ with 7 rows is to that with 1 in rat. between $\begin{cases}6{\cdot}6 \text{ to } 1\\5{\cdot}7 \text{ to } 1\end{cases}$ & $\begin{cases}6{\cdot}2 \text{ to } 1\\3{\cdot}7 \text{ to } 1\end{cases}$, by means as $\begin{cases}6{\cdot}4 \text{ to } 1\\4{\cdot}8 \text{ to } 1\end{cases}$.

Tu. June 6 [1775].

608] The 2nd leather Torp. was tried in sand† wetted with salt water. The Torp. lay flat on sand and was covered by it all but pos. elect. parts & middle of back. With 3 rows charged to $1\frac{1}{2}$, felt a shock whether I laid bare hands on torp. & on sand 16 inc. dist. from nearest part of D°, or whether I touched torp. with metals. In latter case shock seemed much the same as shock 10 inch plate crown glass‡ received through Lane's el. at $\frac{9\frac{1}{2}}{1600}$ inc.

If I laid pieces of sole leather§ which had been soaked in salt water for a week and then pressed between paper with $\frac{1}{2}$ hundred weight for $\frac{1}{2}$ day to drain out moisture on torp. and on sand, and received shock with metal that way, shock was about equal to that of 10 inc. plate with Lane at $\frac{6\frac{1}{2}}{1600}$.

The torp. taken out of sand and tried with metals in usual way gave shock about equal to D° plate, Lane at $18\frac{1}{2}$.

Being tried in same manner with 1 row, shock was weaker than in sand through leathers, & with 2 rows stronger than without leathers.

The spe. gra. bottle with water which came from sand weighed 8.4.11. Th. at 69, so that the water with which it was moistened appears to be of right strength.

609] Bits of beech, wainscot & deal|| about $\frac{3}{4}$ inch square were soaked in salt water for 3 or 4 days, then taken out and wiped and exposed to the air in dry room for about 6 hours.

* [Arts. 406, 573, 610, and Note 31.] † [Art. 422.]
‡ [Arts. 411, 430.] § [Art. 423.] || [Art. 588.]

The shock of the Torp. was received touching pos. el. part with metal and neg. with one of these bits, the end which touched the torp. and that part which I held in hand being bound round with tinfoil.

With 6 rows elect. to $1\frac{1}{2}$, I felt slight shock through wainscot: dist. tinfoils 2 inc.

With D° charge through deal, tinfoils at 1 inc., none.

With 3 rows to $1\frac{1}{2}$; received shock through beech, tinfoils at $4\frac{1}{2}$ inc. dist., about as strong as with $1\frac{1}{2}$ rows when touched with metals on both sides.

With D° charge through $4\frac{1}{2}$ inches of dry deal dipt in salt water and tried the instant it was taken out, none.

Taking hold of tail in one hand & touching pos. side with metal, brisk shock. When touching neg. side with metal much slighter, the exper. tried with each pos. and each neg. part.

Mon. June 12 [1775].

610] Jar 1 elect. to $2\frac{1}{2}$ by pith el. seemed to give shock of same strength as B + 2A comm. to whole battery; it was weaker than 2B and stronger than B commun. to D°, but as there is a good deal of difference between the sensations of the 2, it is not easy comparing them.

According to this exp. the numb. jars which el. should be divided amongst in order to produce given shock should be as the $2\frac{7}{8}$ power of quant. el., and therefore el. 2 jars should be comm. to $5\frac{1}{3}$ more in order to produce same shock as 1 jar*.

Mon. June 18 [1776†].

1R + 3B + C + 2A comm. to 7 rows = ($34\frac{1}{2}$), & el. to a given mark on pith el. gives shock equal or rather greater than 1 row el. to same degree and not commun. to rest.

1R + 3B + C + 2A elect. to $1\frac{1}{4}$ on straw el. and comm. to rest always passed through 1 loop of machine. The same elect. to 1 sometimes passed, sometimes failed.

1 row charged to 1 and not commun. to any more passed 3 times through 5 loops without once failing.

1R + 3B + C + 2A el. to 1 and comm. to rest would never pass through 2 loops.

611] 2nd Leather torpedo tried under water with metals with glass tubes on them, all rows charged to 4 gave briskish shock, which was much greater than shock out of water with 1 row to $1\frac{1}{2}$, but rather less than with 2 rows to D°.

The shock received in same manner with 1 row not communicated to rest was less when el. to $1\frac{1}{2}$, and about equal when el. to $2\frac{1}{2}$.

* [Arts. 406, 573, and Note 31.]

† [Probably June 19, 1775.]

With 7 rows el. to $1\frac{1}{2}$ shock of D° Torp. when received through the salted lime tree wood gave slight shock about equal to 3A passed through same wood without torpedo.

Charge of 7 rows el. to 4 is to that of $1\frac{2}{3}$ row el. to $1\frac{1}{2}$ as $\frac{7 \times 2{\cdot}8}{1\frac{2}{3}}$: 1 :: 12 to 1.

612] Tu. July 4 [1775]. 2nd leather torp., the wire belonging to convex side fastened to outside of battery and inside of battery touched by wire of flat side.

3 rows of battery charged to $1\frac{1}{2}$ and comm. to remainder. Under water no sensible diff. whether I touched convex or flat side with one hand.

Out of water, touching tail with one hand and one side of one elect. organ with metal, a much greater shock if I touched convex side than flat side. The event was the same if it was elect. by neg. elect.

Touching convex side of both organs with one hand, only standing on electrical stool, a shock in that hand, but I think scarcely so strong as under water.

Touching flat side in same way, much the same.

Laying 1 finger on convex surface of one organ & another finger of same hand on the middle of the convex surface, a very slight shock.

Laying one finger on convex surface of one organ & the other on the nearest edge of the torpedo, a considerably greater shock, but not strong.

Laying one finger on convex and another on flat side of same organ, a considerably greater shock, but do not know how to compare it in point of strength with that taken the usual way.

Tried without any torpedo.*

613] 3B being comm. to 7 rows and passed through 1 loop of 26 links of small chain. If the chain was not stretched by any additional weight, the shock did not pass. If the middle link was stretched by a weight of 7 pwt. it passed, & the light was visible in a few links. If it was stretched by a weight of $13\frac{1}{2}$ pwt. no light was seen. There was no remarkable difference in the strength of the shock, whether it was received through chain tended by $13\frac{1}{2}$ pw. or without chain.

The chain was fastened to the same machine that was used in a former experiment, it was 7·9 inc. long and the distance of the supports 5·1.

The room was quite dark, it being tried at night without any candle in the room.

3 rows of battery were elect. till pith el. sep. to 1, its el. was then comm. to the rest of the battery, & I received the shock of 1 row, the elect. having its choice whether it would pass through my body or through some salt water.

* [Art. 437.]

I then elect. 1 row of battery till pith el. sep. to same degree, and commun. its elect. to rest of battery and received the shock of 5 rows of it in same manner. The shock seemed to be nearly of same strength, perhaps rather less.

Therefore shock of 5 rows elect. to a given degree seems about equal or perhaps rather less than that of 1 row el. to 3 times that degree.

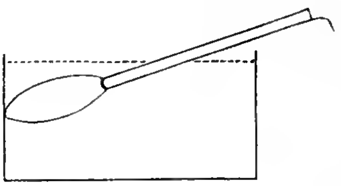
614] The mean thickness of the section of the elect. organ in the section given in Mr H.* paper, in which the breadth is 10·3 inches, that is, the same as my torpedo's, is 1·3 inc.; the area of one organ is $2{\cdot}5 \times 5\frac{1}{4} \times \frac{12\frac{1}{4}}{15\frac{1}{2}} = 9\frac{1}{2}$ sq. inc., as found by cutting out a piece of paper of that size and weighing it.

And according to Mr H. there are about 150 partitions in 1 inch, therefore comp. charge both organs reckoned in old way is

$$19 \times 1{\cdot}3 \times 150 \times \tfrac{4}{3} \times 150 = 748{,}000,$$

and the real charge is 1122,000 inches of el. supposing the partitions to consist of plates of white glass $\frac{1}{150}$ inc. thick, which is about $2\frac{1}{2}$ times as much as my battery, that being = 451,000 inc. el.

615] Tried with the 2nd leather torpedo, new covered, in large trough full of water, the torpedo laid flat as in figure, the electrical organ being (as supposed) 3 inches under water.



If torpedo was tried out of water with 1 row to $1\frac{1}{2}$ comm. to 7 and touched with hands in usual manner, the shock was just felt in hands, and if touched with metals, was just sensible in elbow.

Tried under water in above-mentioned manner with 7 rows el. to 4, the upper surface being touched with the pestle of a mortar held in one hand, the other hand dipt into water as far as wrist, a shock in the wrist of the hand in the water I believe full as strong as the former.

The place where the hand was dipt into water was about 11 inc. from the front of the fish, and conseq. about 14 from elect. organ.

Tried in the same manner as before, except that the fish lay in an open wicker basket†, just big enough to receive it, and which had been soaked for some days in salt water. The shock seemed much the same.

Holding hand in water in same manner as before without touching torpedo —no sensation.

With three rows to $1\frac{1}{2}$ out of water, the shock was stronger if I touched convex side with one hand laid flat on elect. organs than if I touched flat side in same manner, but the difference was not great.

Charge of 7 rows el. to 4 is to that of 1 row el. to $1\frac{1}{2}$ as 19·6 to 1.

The water appeared by its spe. gra. to contain $\frac{1}{31}$ of salt.

* ["Anatomical observations on the torpedo." By John Hunter, F.R.S., [*Phil. Trans.* 1773]. Art. 436.]

† [Art. 421.]

[EXPERIMENTS ON] RESISTANCE TO ELECTRICITY [1776]

{From MS. N°. 19. See Table of Contents at the beginning of this volume. See also near the end of the Introduction.}

616] *Comparison of conducting power of salt and fresh water in the latter end of March and beginning of April*, 1776.

Tried with Nairne's last battery, 6 jars being chose, each of which held very nearly the same quantity of electricity; the wires run into the bent ends of the tubes being made to communicate with the outside of the battery, and the wires run into the straight ends being fastened to separate pieces of tinfoil.

The six jars were all charged by the same conductor: the communication with that and each other was then taken away, and the jars discharged through the tubes, one after the other, by touching the above-mentioned bit of tinfoil by metal held in one hand, and the wire of the jar by metal held in the other hand, the shock being received alternately through each tube.

617] Exp. 1.

Distance of wires In tube 15	14	Sat. sol. S.S.* in tube 14, salt in 69 of water in tube 15	
6·5 inc.	40·7	very sensibly less	in short tube than in long one
5·8		sensibly less	
3·5		sensibly greater	
4·2		scarce sensibly	
5·3		just sensibly less	

Straw electrom. = 4. Th. = 57. [Resistance = 390000 Ohms. †]

Resistance of 4·7 inches in tube 15 supposed equal to 40·7 in 14. Therefore sat. sol. conducts 8·6 times better than salt in 69 of water.

Exp. 2. *The same solution tried in tubes 22 and 23.*

Tube 23 22 electrom. at $1\frac{1}{4}$. Th. = 58. [R. = 118000] †.

Tube 23	22	
3·3	41	sensibly greater.
5·5		less in same proportion.

4·4 inches in tube 23 = 41 in tube 22.

Therefore sat. sol. conducts 8·94 times better than salt in 69 of water.

* [Saturated solution of sea salt.]

† [The resistance of the saturated solution in Ohms, calculated from the measurements in Art. 635 by Kohlrausch's data, is given for each tube within brackets to indicate the absolute value of the resistances compared.]

Exp. 3. *A new saturated solution and solution in* 69 *of water made and tried in tubes* 15 *and* 14. [R. = 390000.]

3·5	40·7	just sensibly greater.	Electrom. = 3.
3·1		very plain.	Th. = 57.
5·5		sensibly less.	
5·0		just sensibly less.	

Therefore new saturated solution conducts 9·61 times better than new solution in 69.

Exp. 4. *Salt in* 69 *of water compared with salt in* 999 *of water in tube* 22 *and* 23.

[R. = 1230000.]
Electrometer = $1\frac{3}{4}$.
Th. = 57.

23	22	
3·1	41·1	sensibly greater.
3·5		scarce sens.
4·3		scarce sensibly less.
4·9		just sensib.
5·3		very sensib.

Resist. 4·1 in tube 23 supposed equal to 41·1 in 22.

Therefore salt in 69 conducts 9·57 times better than salt in 999.

Exp. 5. *Salt in* 999 *compared with distilled water in tubes* 12 *and* 20.

[R. = 462000.]
Electrom. = 3.
Th. = 58.

20	12	
·78	43·5	sensib. greater.
1·2		scarce sensib. less.
1·4		diff. more sensib. than in 1st trial.
1·05		supposed right.

Therefore salt in 999 conducts 36·3 times better than distilled water.

The distilled water changed for rain water.

1·9		sensib. greater. Electrom. = 3. Th. = 58°.
3·3		less, rather more sensib. than former.
2·55		supposed right.

Therefore rain water conducts 2·4 times better than distilled water, or 15·2 times worse than salt in 999.

The rain water changed for distilled water with $\frac{1}{20000}$ of salt in it.

5·3		sensib. less.
2·7		about as much greater.

Therefore salt in 20,000 of distilled water conducts [3·67] times better than distilled water, or 9·92 worse than salt in 999.

[6] *Saturated solution and salt in 69 of water (the new solutions) compared in the same manner, only using the jars 1 and 2 instead of the battery; with the tubes 5 and 17.*

17	5	[R. = 25100.]
2·6	41·1	sensib. greater.
5·5		sensib. less.

Therefore saturated solution conducts 10·05 times better than salt in 69 of water.

618] The electricity of the 6 jars was found to be as much diminished by being communicated to 3 rows of the battery as that of 1 row is by being communicated to 4 rows, therefore quantity of electricity in the 6 jars is to that in one row as 3 to 4.

Exp. 7. *Saturated solution and salt in 69 (the new solutions) tried in the same manner with battery; 1 row being electrified to 2, and its electricity communicated to remaining rows, and one row used at a time.*

Tried in tube 5 and 17. [R. = 25300.]

17	5	
5·4	41·4	plainly less.
2·6		about as much greater.
3·0		scarce sensibly greater.
5·0		just sensib. less.

3·95 supposed right. Therefore sat. sol. conducts 10·31 times better than salt in 69.

Exp. 8. *Saturated solution compared with salt in 29 in tubes 22 & 23 with Nairne's jars.*

23	22	Electrometer 1. Th. 63. [R. = 65000.]
8·7	24·9	just sensibly less.
4·8		about as much greater.

The bore of that part of tube 23 which was used is supposed $\frac{3}{80}$ greater than that of whole tube together. Therefore sat. sol. conducts 3·51 times better than salt in 29 of water.

Exp. 9. *The solution in 29 diluted with* $1\frac{1}{3}$ *of water,* id est, *solution of salt in 69, compared with sat. solution in same tubes.*

23	22	Electrom. = 1. Th. = 63. [R. = 65000.]
2·0	24·9	greater.
3·8		about as much less.

Therefore sat. sol. conducts 7·79 times better than the diluted solution, and the diluted solution conducts 2·2 times worse than solution in 29.

Exp. 10. *Saturated solution compared with salt in 69 in same tubes.*

23	22	[R. = 65000.]
3·1	24·9	sensib. less. Electrom. = 1. Th. = 63.
1·9		as much greater.

Therefore sat. solut. conducts 9·02 better than the solution in 69.

619] *Examination whether salt in 69 conducts better when warm than when cold.*

Salt in 69 in tube 17 placed in water; solution in 29 in tube 23 out of water, the distance of wires in tube 17 being not measured, but remaining always the same.

Electrometer = $\frac{3}{4}$.

23		
8·1	sensib. less	heat of water = $58\frac{1}{2}$.
5	about as much greater	
4	plainly less	heat of water = 105.
2·6	as much greater	

Therefore salt in 69 conducts 1·97 times better in heat 105 than in that of $58\frac{1}{2}$*.

620] *Examination whether the proportion which conducting power of sat. sol. and salt in 999 bear to each other is altered by heat.*

Sat. sol. in tube 15, salt in 999 in tube 19, both in water; distance of wires in tube 15 not altered.

19	Electrom. = $1\frac{1}{4}$.	
3·25	sensib. less	heat of water 50.
2·15	sensib. greater	
2·25	just sensib. greater	electrom. = 1.
3·5	rather more sensib. less	heat of water 95.

Therefore the proportion seems very little altered by heat†.

621] Jan. 1, 1777. *Salt in 2999 of water compared with water distilled in preceding summer in tubes 12 and 20.*

20	12	Electrom. = $4\frac{1}{2}$.
1·5	43·5	rather greater. Column of 1·6 in tube 20.
1·95		plainly less. Supposed equal to 43·5 in tube 12.
1·4		greater.

Therefore allowing for different bores of tubes, salt in 2999 conducts 24 times better than distilled water.

* [By the experiments of Kohlrausch, this ratio would be 1·59. See Art. 691 and Note 33.]

† [This agrees with the results of Kohlrausch.]

Jan. 2 M. Same experiment repeated with the same water, which had been left in the tubes all night.

20	12	
1·4		plainly greater.
1·9		seemingly less.
2·1		plainly less.

Therefore salt in 2999 conducts 22 times better than distilled water.

The same experiment repeated, only the water in the tubes was changed for fresh by pouring out the old and putting in fresh by small funnel, without taking out the wires.

1·1		plainly less.
·35		plainly greater.
·7		plainly less.

Therefore salt in 2999 conducts 72 times better than distilled water.

The same experiment repeated, only the distilled water changed for that used in the preceding year.

·4	considerably greater.
1·1	plainly less. ·8 supposed equal.

Therefore salt in 2999 conducts 47 times better than distilled water.

Jan. 3. Experiment repeated with the same water left in.

·8	plainly greater.
1·2	plainly less.

Therefore salt in 2999 conducts 38 times better than the distilled water.

The distilled water changed for the new distilled water.

·28	plainly greater.
·6	plainly less.

Therefore salt in 2999 conducts 86 times better than distilled water*.

Salt in 2999 *compared with salt in* 150,000 *in same tubes.*

1·2	43·5	sensib. greater. Electrom. = 4½.
1·7		sensib. less.

Therefore salt in 2999 conducts 26 times better than salt in 150,000.

The experiment repeated with the same waters, only the wires in tube 12 brought nearer.

·3	12·5	sensib. greater. Electrom. = 1½.
·45	—	sensib. less.

* [See Art. 690.]

622] *Examination whether comparative resistance of salt in* 2999 *and salt in* 150,000 *was the same when tried in the above-mentioned manner, or when passed through* 2 *wires in glass of water, as in fig.**

Jan. 6. The tubes 12 and 20 filled with salt in about 105 of water: salt in 150,000 of water in glass. 2 jars electrified to $1\frac{3}{4}$ and communicated to the rest.

If the distance of wires in tube 12 was $\begin{matrix}33\cdot5\\18\cdot5\end{matrix}$ the shock was sensibly $\begin{cases}\text{less}\\\text{greater}\end{cases}$ than that through the wires in glass.

The same tried as before, only with the jars electrified to 2 and the shock received with shock melter*.

If the distance of wires in tube was $\begin{cases}31\cdot5\\20\cdot3\end{cases}$ shock was plainly $\begin{cases}\text{less}\\\text{greater}\end{cases}$ than through wires in glass.

The glass filled with salt in 2999 and the shock compared with that through tube 20 with same solution of salt in 105.

The jar electrified to 2 and received with shock melter†.

If dist. wires in tube 20 was $\begin{cases}\cdot5\\1\cdot\end{cases}$ shock was $\begin{cases}\text{greater}\\\text{less}\end{cases}$ than through wires in glass.

N.B. Great irregularity was found in trying this last experiment, the cause of which I am unacquainted with.

Therefore salt in 2999 conducts 31·5 times better than salt in 150,000.

The same salt in 150,000 which was used in this experiment was saved and compared with salt in 2999 in the usual manner with tubes 12 and 20, electrometer at $4\frac{1}{2}$.

If distance of wires in tube 20 was $\begin{cases}1\cdot2\\1\cdot85\end{cases}$ shock was plainly $\begin{cases}\text{greater}\\\text{less}\end{cases}$ than through tube 12 with wires at 42·4 inches distant.

Therefore salt in 2999 conducts 24·6 times better than salt in 150,000.

The thermometer in all the foregoing experiments of this year supposed to be about 45°.

623] Exp. 11. *Saturated solution in tube* 14 *compared with salt in* 149 *of water in tube* 15.

Tube 15	14	Electrometer at $3\frac{1}{2}$. Th. = 45. [R. = 474000.]
1·6	41·8	sensib. greater.
2·6		sensib. less.

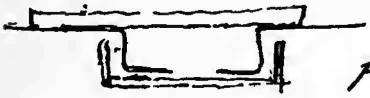
Sat. sol. conducts 20·5 times better than salt in 149.

* [See figure in facsimile of MS. on opposite page.]

† [The reading here is doubtful; see facsimile of MS. on opposite page. Cavendish says, Arts. 601, 602, 616, that he took the shock with *metals* in each hand, but the word here cannot be read "metal." The word occurs also in Arts. 585 and 637.]

P. 9

Examination whether comparative resistance of salt in 2999 & salt in 150000 was the same when tried in above mentioned manner or when passed through 2 wires in glass of water as in fig.



Jan. 6 The tubes 12 & 20 filled with salt in about 105 of water salt in 150000 of water in glass 2 jars el. to $1\frac{3}{4}$ & commun. to rest

If dist. wires in tube 12 was $\frac{33,5}{18,5}$ the shock was sensib. { less / greater than that through the wires in glass

The same tried as before only with the jars el. to 2 & the shock received with shock melter

If dist wires in tube was { 31,5 / 20,3 shock was plainly { less / greater than through wires in glass

The glass filled with salt in 2999 & the shock compared with that through tube 20 with same sol. salt in 105

The jars el. to 2 & received with shock melter

If dist. wires in tube 20 was { 1,5 / 1,1 shock was { greater / less than through wires in glass

N.B. Great irregularity was found in trying the last experiment the cause of which I am unacquainted with

Therefore salt in 2999 conducts 30/1,5 times better than salt in 150000

Exp. 12. Jan. 8. *Sat. sol. in tube 22 compared with salt in 149 in tube 23.*

Tube 23	Tube 22	Electrom. $1\frac{1}{4}$. Th. = 42. [R. = 146000.]
1·6	41	plainly greater.
1·8		seeming. greater, but doubtful.
2·4		plainly less.
2·2		rather less.
2·0		not sensib. different.
1·8		seemed greater.

$\frac{1·8 + 2·1}{2}$ in tube 23 supposed = 41 in tube 22.

Therefore sat. sol. conducts 19·6 times better than salt in 149.

Exp. 13. *Salt in 149 in tube 5 compared with salt in 2999 in tube 17.*

Tube 17	5	Electrometer = 3. Th. = 44. [R. = 652000.]
1·8	40·5	sensib. greater.
2·8		sensib. less.

Salt in 149 conducts 17·3 times better than salt in 2999.

By new measure of tubes.

Exp. 14. *Same solutions in tubes* 18 *and* 19.

Tube 19	18	Electrometer = $1\frac{3}{4}$. [R. = 308000.]
2·2	42·8	sensib. greater.
2·4		not sensib.
3·0		sensib. less.
2·8		seemed less, but doubtful.

$\frac{2·2 + 2·9}{2}$ in tube 19 supposed equal to 42·8 in tube 18.

Salt in 149 conducts 16·7 times better than salt in 2999.

Exp. 15. Jan. 9. *Sat. sol. in tube 22 compared with salt in 29 in tube 23.*

Tube 23	22	Electrometer = $1\frac{1}{4}$. Th. = 42. [R. = 128000.]
9·7	35·8	sensib. less.
9·4		seemed less, but doubtful.
6·6		seemed greater.
6·9		not sensib. gr.
6·3		sensib. greater.

6·5 + 9·7 supp. = 35·8 in tube 23.

Sat. sol. conducts 4·38 times better than salt in 29.

624] *Comparison of water purged of air by boiling and plain water.*

Jan. 12. Salt in 2999 in tube 12. Salt in 150,000 in tube 20.

Tube 20	12	Electrometer = $4\frac{1}{2}$. Th. = 50.
1·2		plainly greater.
1·4		seemed greater, but doubtful.
1·8		seemed less, but doubtful.
2·0		plainly less.

The water was then boiled over lamp in the same vial in which it had been kept some time, and then cooled in water and compared in the same manner.

2·0	sensib. less.
1·8	seemed less, but doubtful.
1·2	seemed greater, but doubtful.
1·0	plainly greater.

Therefore if anything water conducted better before boiling than after, but the difference might very likely proceed from the error of the experiment.

In order to see whether the water had absorbed much air by being exposed to the air in the trial, some of the boiled water was exposed to the air as much as that which was tried in the tube was supposed to have been, and boiled over again in a vial. It did not begin to discharge air till it was heated to 190°, and then discharged but little. Some more of the boiled water which had not been poured out of the vial seemed to discharge as much air. But some distilled water which had not been boiled began to discharge air almost as soon as heated, and discharged a great deal before it began to boil*.

625] *Comparison of water impregnated with fixed air and plain water.*

Some distilled water was impregnated with fixed air produced by oil of vitriol and marble, and compared with salt in 2999 in same tubes and manner as in former experiments.

		Electrometer = $4\frac{1}{2}$. Th. = 55.
1·6		seemed greater, but doubtful.
1·4		plainly greater.
2·4		not sensibly less.
2·6		sensibly less, scarce doubtful.
2·8		plainly less.

The same water deprived of its fixed air by boiling, and tried as before.

Tube 20	12	
1·2		plainly less.
1·0		sensib. less.
·9		seemed less, but doubtful.
·6		plainly greater.
·7		not sensib. greater†.

* [See Art. 692.] † [See Art. 693.]

P. 14

~~Some sal Sylvii, Sal Amm., quadr. nitre & Glaub salt & Calc. f. f were dissolved in in mixed with such a proportion of water that the~~.

2.3 of sal Amm. 3.2 of sal Sylvii 3.17 of quadr nitre 2.21. of glas calcined glaub. salt & 14.10 of calc. S.S A were dissolved in water the solut. of each being 3.10.12 that is such that the quant. acid in each should be equiv. to that in a solut. salt in 29 of water

Jan. 13
Sal Sylvii in tube 15 comp. with salt in 29 in tube 22 el. = 2 th. 55

tube 15	22	
5,	18,2	seemed gr. but doubt
4,5		plainly greater
7-		seemd less but doubt
7,7		seemed less
8,2		plainly less

$\frac{7,4+5}{2} = 6,2$ supposed right

7 :

Jan. 14 sal. amm. tried same way

7,2	.2	seemd less but doubt
7,9		plainly less
5,3		sensibly greater
5,8		seemed gr. but doubt

626] 2·3 of Sal. Amm., 3·2 of Sal. Sylvii, 3·17 of Quadr. nitre, 2·21 of calcined Glauber's salt and 14·10 of calc. S. S. A.* were dissolved in water, the solution of each being 3.10.12, that is, such that the quant. acid in each should be equiv. to that in a solut. salt in 29 of water.

Jan. 13. *Sal. Sylvii in tube* 15 *compared with salt in* 29 *in tube* 22.

Tube 15	22	Electrometer = 2. Th. = 55. [R. = 253000.]
5	18·2	seemed greater, but doubtful.
4·5		plainly greater.
7		seemed less, but doubtful.
7·7		seemed less.
8·2		plainly less.

$\frac{7·4 + 5}{2} = 6·2$ supposed right.

Jan. 14. *Sal. amm. tried same way.*

7·2		seemed less, but doubtful.
7·9		plainly less.
5·3		sensibly greater.
5·8		seemed greater, but doubtful.

Calc. S. S. tried same way.

4·0		plainly greater.
4·5		scarce sensib. gr.
6·0		plainly less.
5·5		sensib. less.

Salt in 29.

4·5		plainly greater.
5·0		scarce sensib. greater.
7·0		plainly less.
6·5		scarce sensib. less.

Glauber's salt.

Tube 15	22	
4·2		plainly less.
3·8		no sens. diff.
4·2		scarce sens. less.
4·4		do.
4·6		just sens. less.
3·4		sensibly greater.
3·6		scarce sensib. less.

* [See facsimile on the opposite page. The results are given in Art. 694. See Note 34, p. 430, and Introduction.]

Quadrang. nitre.

4·0		plainly greater.
4·3		seemed greater, but doubtful.
6·0		seemed less, rather doubtful.
6·2		plainly less.

627] 2·0 of oil of vitriol, 2, 5·10 of spirit of salt 2, and 5·19 of f. alk. D, were diluted with water, the solution being 3.10.12. Consequently the quantity of acid in 2 first were equivalent to that in salt in 59 of water, and the alk. in last was equivalent to that in salt in 29; compared in the same manner as the former.

Jan. 15. *F. alk.* Th. = 55°.

4·5		scarce sensib. greater.
4·0		seemed greater, but doubtful.
3·7		plainly greater.
5·7		plainly less.
5·4		seemed less, but doubtful.

Diluted oil of vitriol.

4·0		sensib. greater.
4·3		not sensib.
5·0		sensib. less.
4·7		not sensib.

Diluted spirit of salt.

11·8		seemed less, rather doubtful.
8·0		seemed greater, rather doubtful.

Another diluted spirit of salt was made of same strength as the former. Being tried with wires at 9·9 inch. distance no sensible difference was perceived, which agrees with former.

Another diluted oil of vitriol was made and tried, Jan. 16.

5·2		sensib. greater.
7·7		sensib. less.

Experiments in January, 1781*.

628] Some basket salt† was dried before fire, and a saturated solution made with it which contained $\frac{1}{3{\cdot}78}$ of salt‡, and also other solutions of different strengths, all being made with distilled water.

* [The results of these experiments are collected in Art. 695. See Note 33.]

† ["Salt made up in form of sugar loaves, in small wicker baskets, which is thence called *loaf salt* or *basket salt.*" *Rees' Cyclopædia.*]

‡ [26·45 per cent. Saturated solution at 18° C. is 26·4 per cent. by Kohlrausch.]

Sat. sol. in tube 14, *salt in* 69 *in tube* 15. [R. = 399000.]

15	14	Electrometer $3\frac{1}{4}$. Th. = 53.	
3·6	39·1	sensib. greater.	
5·8		plainly less.	4·5 supposed right.
5·4		sensib. less	

Sat. sol. conducts 8·63 times better than salt in 69.

Same solutions in same tubes.

5·4		plainly less.	
5·1		seemed rather less.	4·3 supposed right.
3·6		scarce sensib. greater.	
3·3		sensib. greater.	

Sat. sol. conducts 9·03 times better than salt in 69.

Sat. sol. in tube 14, *salt in* 29 *in tube* 15. El. = $3\frac{1}{4}$.

7·3		sensib. greater.	9·1 supposed right.
11·0		sensib. less.	

Sat. sol. conducts 4·1 times better than salt in 29.

The same solutions in same tubes.

10·7		seemed rather less.
7·5		sensib. greater.
7·8		seemed rather greater.
9·7		supposed right.

Sat. sol. conducts 3·85 times better than salt in 29.

The same solutions in same tubes. El. = $1\frac{1}{2}$. [R. = 136000.]

2·6	13·3	seemed rather greater.	
2·6		D° scarce sensib.	
3·7		plainly less.	2·95 supposed right.
3·5		sensib. less.	
3·3		seemed rather less.	

Sat. sol. conducts 3·95 times better than salt in 29.

Sat. sol. in tube 14. *Salt in* 11 *in tube* 15. El. $1\frac{3}{8}$.

15	14	[R. = 136000.]	
4·7	13·3	sensib. greater.	
8		plainly less.	5·95 supposed true.
7·6		do.	
7·2		sensib. less.	

Sat. sol. conducts 1·92 times better than salt in 11.

The same again.

7	seemed rather less.
4·9	sensib. greater. 6·05 supposed right.
5·1	seemed rather greater.

Sat. sol. conducts 1·88 times better than salt in 11.

The same again.

5·1	seemed rather greater, but doubtful.
4·9	sensib. greater.
7	sensib. less. 5·95 supposed true.

Sat. sol. conducts 1·92 times better than salt in 11.

629] *Salt in 142 put in tubes 5 and 15 in order to find what power of velocity the resistance is proportional to.*

15	5	Electrometer = 3. [R. = 579000.]
3·3	41·9	plainly less.
3·1		do.
2·9		scarce sensib. less. 2·75 supposed right.
2		plainly greater.
2·2		D°.
2·4		sensib. greater.
2·6		seemed rather greater.
3·1		sensib. less.

Therefore log. vel. in 15 by do. in 5 = 1·2122,

log. length in 5 by do. in 15 = 1·1829.

Therefore resistance is as $\frac{1·1829}{1·2122}$ = ·976 power of velocity.

The same repeated.

15	5	
3·2		sensib. less.
3		hardly sensib.
2·6		seemed sensib. greater.
2·5		sensib. greater. 2·85 supposed true.
2·7		hardly sensib.

Log. vel. in 15 by D° in 5 = 1·2122,

Log. length in 5 by D° in 15 = 1·2122.

Therefore resistance is directly as velocity*.

* [This is the first experimental proof of what is now known as Ohm's Law.]

630] *Salt in* 69 *in tube* 22. *Salt in* 999 *in* 23.

23	22	Electrometer = 3½. [R. = 1335000.]	
4·9	41·5	plainly less.	
4·6		D°.	
4·3		scarce sensib. less.	
3·6		sensib. greater.	
3·7		scarce sensib. greater.	4 supposed true.
3·4		seemed greater.	
3·2		plainly greater.	
3·4		sensib. greater.	

Salt in 69 conducts 9·91 times better than salt in 999.

The same repeated.

3·4		seemed greater.	
3·6		scarce sensib. greater.	
3·2		sensib. greater.	3·85 supposed true.
4·3		scarce sensib. less.	
4·5		sensib. less.	

Salt in 69 conducts 10·3 times better than salt in 999.

The same liquors in tubes 5 *and* 17.

17	5	Electrometer = 1½. [R. = 288000.]	
4·2	42·2	plainly less.	
4		sensib. less.	
3·8		hardly sensib.	3·55 supposed right.
3·1		sensib. greater.	
3·3		hardly sensib.	

Salt in 69 conducts 11·31 times better than salt in 999.

The same repeated.

17	5	
3·3		hardly sensib. greater.
3·1		sensib. greater.
4		not sensib. less.
4·2		seemed rather less.
4·3		D°.
4·4		sensib. less.

Salt in 69 conducts 10·75 times better than salt in 999.

Salt in 999 *in tube* 12; *distilled water in tube* 20. El. = 2½.
[R. = 494000.]

20	12	
·3	43·3	seemed rather less.

2 or 3 hours after it seemed rather greater at ·5.

Next morning was plainly greater at ·7.

The water being changed for fresh, seemed rather less at ·3.

The distilled water changed for salt in 20,000.

2·1		sensib. less.	
2		not sensib. less.	
1·7		sensib. greater.	
1·8		seemed rather greater.	1·95 supposed right.
1·9		not sensib. greater.	
2·1		sensib. less.	

Salt in 999 conducts 20 times better than salt in 20,000.

Salt in 20,000 conducts about 7 times better than distilled water; therefore if distilled water contains $\frac{1}{120000}$ of salt their conducting powers will be as the quantity of salt in them.

The same repeated. Electrometer = 2.

2·1		not sensib. less.	
2·2		seemed rather less.	
2·3		D°.	
2·4		D°.	
2·5		plainly less.	2 supposed right.
2·3		seemed rather less.	
1·7		seemed rather greater.	
1·6		plainly greater.	

Therefore salt in 999 conducts 19·5 times better than salt in 20,000.

The waters changed for fresh. El. = 2½.

20	12		
2		scarce sensib. different.	
1·9		D°.	
1·8		sensib. greater.	
2·1		not sensib. less.	2·05 supposed right.
2·3		sensib. less.	

Salt in 999 conducts 19 times better than salt in 20,000.

The same repeated. El. = 2.

2·3	42·9	sensib. less.	
2·1		scarce sensib. less.	
1·8		seemed rather greater.	1·95 supposed right.
1·7		D°.	
1·6		sensib. greater.	

Salt in 999 conducts 19·8 times better than salt in 20,000.

Salt in 69 *in tube* 22. *Salt in* 142 *in tube* 23.

23	22	El. = 2. [R. = 407000.]	
7·3	12·65	seemed rather less.	
7·5		sensibly less.	
6·3		not sensib. greater.	6·75 supposed right.
6		sensib. greater.	

Salt in 69 conducts 1·74 times better than salt in 142.

The same repeated.

6		seemed rather greater.	
5·8		doubtful.	
5·6		plainly greater.	
5·8		not sensib. greater.	6·45 supposed right.
7·3		plainly less.	
7		seemed rather less.	
6·8		scarce sensibly less.	

Salt in 69 conducts 1·84 times better than salt in 142.

Salt in 999 *in tube* 12, *distilled water in tube* 20. El. = $2\frac{1}{2}$.

[R. = 494000.]

·83		sensib. less. N.B. The tubes had been measured
·7		sensib. greater. between the last trial and this.

The distilled water was then changed for fresh.

·3		sensib. less.

Another bottle was filled with distilled water and tube 20 filled up again with that.

·3		seemed rather less.

631] *The tube* 20 *filled with the same distilled water mixed with* $\frac{1}{200}$ *of spirits of wine.*

·3		seemed not sensibly less.

Same mixture mixed with $\frac{1}{20}$ *more of spirits of wine,* id est, *sp^t wine in* 18 *of distilled water.*

·3		scarce sensib. less.

Equal bulks of sp^t wine and distilled water.

·3		seemed scarce sensib. less.

Pure spirits of wine.

·3		seemed of 2 rather greater.

Therefore there is not much difference between the resisting power of the above distilled water and spirits of wine and mixtures of the 2: but of the 2, spirits of wine resists least.

[CALIBRATION OF TUBES.]

632] Tube 14.

Dist. mid. col. from str. end	Length col.	
3·6	4·08	col. = 2·45.
9·2	4·03	40·6 inc. = 28·5 gr.
13·6	3·9	4·26 = 3 gr.
18·1	3·7	12·2 inches of tube next to bend contain $\frac{1}{3}$ part of ☿ of that in col. 42·5 long.
25·5	3·54	
31·1	3·46	
36·8	3·09	
42	2·8	

Tube 15.

− 3·6	5·32	col. = 3·25 gr.
+ 1·7	5·16	
+ 7·9	4·95	
+ 9·9	4·47	
+ 11·6	4·67	

After it was measured 7 inches were cut off from straight end, and the numbers in the first col. are the distances from the shortened end. A new bend was also made 12·8 from new end, and the part where the tube is equal to tube 14 is at 10·3 from D°.

633] Jan. 1781. The following tubes were measured over again by introducing a col. ☿ and measuring its length in 3 different places, the beginning of the 1st col. being at $\frac{1}{2}$ inch from bend, and the beginning of the second at the end of the first. Another column was afterwards introduced whose length was pretty nearly equal to the sum of the 3 former, and weighed.

N° of tube	1st col.	2nd col.	3rd col.		
14	12·2	14·3	16	41·8 inc. =	29·3 gr.
15	3·47	3·71	3·86	11·55 =	7·7
22	13·03	13·65	14·6	41·8 =	100
23	3·6	3·3	3·09	10 =	24·5
5	12·65	14	13·5	42·1 =	489
17	3·25	3·18	3·08	9·9 =	116

634] The two following tubes were measured by stopping up the end near bend and weighing them with different quantities of ☿ in them, and measuring the distance of the top of the column from straight end, whence it was found that in

	14·8	long beginning		1448
N° 12 a col.	28·6	at $\frac{1}{2}$ inch	=	2777
	42·8	from bend		4033
	3·17	long beginning		270
N° 20 a col.	6·27	at $\frac{1}{2}$ inch from		543
	9·97	bend weighed		881

635] In the following result the column whose length is given in the 2nd column is supposed to begin at ½ inch from the bend.

By the resistance of each is meant $\frac{1}{\text{grains} ☿ \text{ in each inch}}$.

[The resistance of a column of mercury one inch long weighing one grain is ·13 Ohms, and the resistance of saturated solution of salt at t° Centigrade is to that of mercury as 10^8 is to 2015 + 45·1 (t − 18). Hence the resistance as given by Cavendish must be multiplied by 6907 + 82·2 (59 − T) to convert it into Ohms when the tube contains saturated solution at T° Fahrenheit.]

RESULT.

No.	Length column	Resist.	Log. do.	Resist. for each inch	Log. do.
14	12·2 26·5 42·5	14·99 35·58 61·36	1·1758 1·5512 1·7879	1·229 1·343 1·444	·0894 ·1280 ·1595
15	3·47 7·18 11·04	4·907 10·518 16·591	·6908 1·0219 1·2199	1·414 1·465 1·503	·1505 ·1658 ·1769
22	13·03 26·68 41·28	5·157 10·817 17·294	·7124 1·0341 1·2379	·3958 ·4054 ·4190	9·5975 9·6079 9·6222
23	3·6 6·9 9·99	1·588 2·923 4·093	·2009 ·4658 ·6120	·4412 ·4237 ·4096	9·6446 9·6270 9·6124
5	12·65 26·65 40·15	1·030 2·291 3·463	·0126 ·3600 ·5395	·08138 ·08596 ·08626	8·9105 8·9343 8·9358
17	3·25 6·43 9·51	·2844 ·5566 ·8121	9·4539 9·7455 9·9096	·08750 ·08656 ·08539	8·9420 8·9373 8·9314
12	14·8 28·6 42·8	·1513 ·2946 ·4552	9·1798 9·4692 9·6582	·01022 ·01030 ·01064	8·0095 8·0128 8·0268
20	3·17 6·27 9·97	·03722 ·07243 ·11293	8·5708 8·8599 9·0528	·01174 ·01155 ·01133	8·0697 8·0626 8·0541

COMPARISON OF RESISTANCE OF COPPER WIRE WITH THAT OF SAT. SOL.

636] The wire was wound on reel on bars of glass about $\frac{3}{4}$ inch broad, the distance of one round of wire from the next on same bar being ·6.

The mean circumference of reel $= 46{\cdot}7 \times 2\sqrt{2}$*.

There were 8 rows of glass bars, and 28 rounds of wire on each row, and on one row there was $\frac{1}{4}$ round over. Therefore whole length of wire

$$= 93{\cdot}4 \times \sqrt{2} \times 8 \times 28 + \tfrac{1}{4} = 29{,}623 \text{ inches.}$$

This weighed 2967 grains, consequently there are 9·984 inches to 1 grain.

N.B. There were many knots in the wire.

637] The resistance of this wire was attempted to be compared with that of sat. sol. in tube 17 by shock melter† as in former experiments, but without success. It was therefore compared by the sound of the explosion by discharging the jars by a wire without its passing through my body; but in this there was considerable difficulty, as the light of the spark passed through the wire was very different from that passed through the water, the first being reddish and the latter white. The sound also was of a different kind, the latter being sharper.

Distance of wires in tube 17.

Distance	El. = 4.	
·68	not sensib. diff.	
·6	scarce sensib. stronger.	
·55	doubtful.	
·5	seemed rather greater.	
·45	sensib. greater.	
·9	seemed sensib. less.	
1	do.	
1·2	sensib. less.	
·9	scarce sensib. less.	El. = $5\frac{1}{2}$.
1	not sensib. less.	
1·2	seemed rather less.	
·6	plainly greater.	
·7	not sensib. greater	
·4	not sensib. gr.	El. = 3.
·3	seemed rather gr.	
·2	plainly gr.	
·8	seemed rather less.	
1	D°.	
1·2	plainly less.	

* [The reel was probably square, with glass bars at the corners, the length of the diagonal being 46·7 inches.]

† [See Arts. 585 and 622.]

1·2	plainly less.	El. = 3.
1	D°.	
·8	scarce sensib. less.	
·4	sensib. gr.	
·5	scarce sensib.	
·5	sensib. gr.	El. = 4.
·6	seemed rather gr.	
·7	not sensib. diff.	
1	seemed rather less.	

638] Two Leyden vials were made of barometer tubes filled with ☿ and coated on outside with tinfoil. The quantity of electricity in them was found to be very nearly the same, but that in N° 1 rather the greatest.

The charge of each of these tubes is about 714 inches, and that of the large jars about 6100, and that of the three jars 1, 2 and 4 together is also 6100*.

The shock of these tubes was received through my body in the same manner as in trying the large jars, either by making the shock pass through the copper wire or through the sat. sol. or receiving it in the simple manner without passing through either: the experiment being tried as usual by charging both tubes from the same conductor and receiving the charges of one one way and the other the other.

639] It was found by repeated trials that the shock received through the copper wire was plainly greater than the simple shock. When received through the sat. sol. with wires†

in contact		not sensib. less than simple.	
at ·1	dist.	seemed rather less, but doubtful.	
·5	D°.		
1	D°.	scarce doubtful.	
2		not doubtful.	
4	D°.		
6·6		considered less.	

The tubes charged to $1\frac{1}{2}$ by old electrometer.

640] It was also found by the small jars 1, 2 or 4 that the shock received through the wire was stronger than the simple shock.

The shock through the wire was also much greater than the simple shock when the covering which was put over the wire to defend it from accidents was taken away.

It was also plainly greater when the shock passed through only 3 rows of the wire instead of the 8.

* [Probably globular inches. The numbers do not agree with those in Art. 583.]

† {See end of Note 31.}

If the shock was received through 166 inches of the same wire not stretched upon glass, without any knots in it, it seemed not at all greater, but if anything less than the simple shock. It was the same if received through a piece of wire of about the same length with 37 knots in it.

641] Some more of the same wire was stretched by silk into 32 × 12 rows, each 78·7 inches long; consequently the whole length was 30,220 inches. It weighed 3272 grains, *id est*, 9·24 inches to a grain.

The shock of the above-mentioned tubes was sensibly greater when received through this last wire than when received simply, but was considerably less than when received through the first wire.

They were then compared by sound with the same tube charged to $5\frac{1}{2}$, when the sound of the shock passed through the new wire was sharper, and the other fuller.

The sound of the shock passed through the new wire seemed full as brisk, and the light as white as of that passed through ·55 of sat. sol., but not near so strong as when the wires in sat. sol. were in contact, the sound and light, however, seemed nearly of the same kind. When distance in tube was 1·1 the sound was evidently less loud than that with the wire.

When the shock was allowed to pass through both wires, the sound, I thought, seemed much of the same kind as when passed through new wire singly.

The shock passed through both wires felt plainly greater than the simple shock, and the difference seemed greater than that between the new wire simply and the simple shock.

In the foregoing the shock passed at the same time through both wires, but it was then tried so that it should first pass through old and from thence through new wire.

The shock felt then evidently stronger than the simple shock or that through new wire alone, but I could not tell whether it was greater or less than that through the old wire alone.

642] A piece of the same wire was wound about 150 times round one of the slips of glass, and was laid flat on another of these slips which lay flat on a table.

The shock of these tubes seemed rather greater when received through this wire than when received simply, but the difference was not considerable, but it seemed evidently less than the shock received through the new wire.

643] The wire was taken from off the reel with the slips of glass, and all except a small part of it stretched round the garden in 14 rounds. The shock of the above-mentioned tubes received through this wire felt plainly greater than that passed through the wire stretched by the silk threads, and much greater than the plain shock.

The shock passed through the sat. sol., wires in contact, seemed about equal to the plain shock.

The spark passed through garden wire seemed rather redder than that through the silk wire, but the difference was not remarkable.

The spark passed through garden wire seemed about as strong as that through about ·8 of an inch of saturated solution, but sensibly redder.

644] The reel was altered, and some copper wire silvered stretched upon it.

The mean circumference of reel = $44{\cdot}05 \times 2\sqrt{2}$.

There were 12 rows of glass bars and 42 rounds of wire on each row, therefore whole length of wire $= 88{\cdot}1 \times \sqrt{2} \times 12 \times 42 = 62{,}790$ inches. This weighed 5747 grains. Consequently there are 10·93 inches to 1 grain.

The shock received through this wire felt vastly stronger than the simple shock; the shock of tube 2 received through the wire with electrometer at $1\frac{1}{4}$ seeming little less strong than the simple shock with the same tube and the electrometer at $1\frac{3}{4}$, but considerably stronger than with electrometer at $1\frac{1}{2}$.

645] The above-mentioned wire compared with sat. sol. by sound.

1·46.	Seemed more brisk. The light of salt water white, the other very red.
1·7	D°
2·5	D°
3·5	D°
5·5	D°
	El. = $5\frac{1}{2}$.
8·7	I believe nearly the same.
8·3	seemed much weaker. El. = 3.
6	seemed rather greater.
7·5	doubtful.
8·5	seemed rather weaker.
6	seemed rather stronger.
7·2	doubtful. El. = 3.
8·5	seemed rather weaker.
6	doubtful.
5	D°.
4	seemed stronger.
8·5	seemed rather weaker.
8·5	doubtful.
9·5	I believe rather less, certainly a sharper sound, but I believe rather less loud.
7	seemed greater.

$$\frac{1{\cdot}65}{2} = {\cdot}82$$
$$\frac{1{\cdot}8}{2} = {\cdot}9$$
$$\frac{1{\cdot}1}{2} = {\cdot}55 \qquad \frac{3{\cdot}72}{5} = {\cdot}74$$
$$\frac{1{\cdot}3}{2} = {\cdot}65$$
$$\frac{1{\cdot}6}{2} = {\cdot}8$$

$$\frac{14{\cdot}5}{2} = 7{\cdot}25$$
$$\frac{12{\cdot}5}{2} = 6{\cdot}25 \qquad \frac{21{\cdot}75}{3} = 7{\cdot}25$$
$$\frac{16{\cdot}5}{2} = 8{\cdot}25$$

646] ·74 of sat. sol. in tube 17 is equivalent to 29,623 inches of copper wire, 9·984 inches of which = 1 grain.

7·25 of sat. sol. in do. = 62,790 of copper wire, 10·93 of which = 1 grain*.

* [Length of wire	Resistance of pure copper calculated from Matthiessen annealed	hard drawn	Resistance of saturated solution in tube 17 calculated from Kohlrausch
29,623	424	433	413
62,790	984	1004	4046]

RESULTS {OF EXPERIMENTS ON COMPARISON OF CHARGES. ARTS. 438—595}

{From MS. N°. 16. See Table of Contents at the beginning of this volume.}

647] 1773, p. 92 [Art. 557]. The connecting wire to the two plates of 9·3 inches contains 1·4 inc. el. The connecting wire to the rosin plates of p. 86 [Art. 554], should contain rather more in proportion to its length than this, *id est*, rather more than ·28.

By p. 93 [Art. 557], the 4 rosin plates seemed to contain about $\frac{1}{2}$ inc. el. less when placed close together than at dist. Let us therefore suppose that the charge of 2 rosin plates placed close together with connecting wire between them exceeds twice the charge of 1 plate by ·28 inc. el., and that the charge of 4 plates exceeds 4 times the charge of 1 by $2\frac{1}{2}$ times that quantity, or ·7 inc. el. Let us suppose, too, that the charge of the 2 double plates A & B with connecting wire exceeds twice the charge of 1 by ·28.

648] 8 square inches of elect. = 9 circular inches.

	L.*	
$\frac{\text{glob. inc. el.}}{\text{circ. inc. el.}} = 1·54$	$= ·1880.$	Res. p. 5 [Art. 654].
$\frac{\text{circ. inc. el.}}{\text{glob. inc. el.}} = ·649$	$= 9·8120.$	

N.B. By inc. el. is meant circular inches of electricity.

649] Mar. 13. P. 85 [Art. 553].

	[Side of square equivalent to trial plate when the balls separate negatively,	positively.]	Difference	Mean
Circle $18\frac{1}{2}$	17·66	13·34	4·32	15·50
Double B	17·89	13·34	4·55	15·61
Double A	17·89	13·34	4·55	15·61
Circle 36	33·65	26·56	7·09	30·10
2 doub.	31·38	24·08	7·30	27·73
D	29·61	22·53	7·08	26·07

* [These logarithms are correct only to three places of decimals, they should be 0·1875 and 9·8122. See Note 35.]

Mon. Mar. 15. P. 85.

	[Side of square equivalent to trial plate when the balls separate negatively,	positively.]	Difference	Mean
Circ. 36	33·65	27·18	6·47	30·41
Circ. 30	28·42 28·10			
Plate air	30·87	24·39	6·48	27·63
2 doub.	31·13	24·39	6·74	27·76
D	29·86	22·84	7·02	26·35
Doub. B	18·22	13·82	4·40	16·02
Doub. A	18·22	13·82	4·40	16·02
Circle 18½	18·11	13·58	4·53	15·84

Mar. 19. P. 86.

Circle 9	9·28	6·48	2·80	7·88
Rosin 1	10·59	7·13	3·46	8·86
2	10·47	6·91	3·56	8·69
3	10·59	7·24	3·35	8·91
4	10·35	7·02	3·33	8·68
1 + 2 Rosin	18·56	14·51	4·05	16·53
3 + 4	18·67	14·62	4·05	16·64
Circle 18½	18·00	13·82	4·18	15·91
Circle 36	33·90	27·49	6·51	30·74
4 rosin	32·14	25·94	6·20	29·04

Mar. 23. P. 90 [Art. 554].

Circle 9·3	9·28	6·48	2·80	7·88
Rosin 1	10·22	7·13	3·09	8·67
2	10·09	7·02	3·07	8·55
3	10·22	7·13	3·09	8·67
4	10·22	7·13	3·09	8·67
Rosin 1 + 2	18·56	14·06	4·50	16·31
3 + 4	18·56	14·06	4·50	16·31
Circle 18½	17·66	13·34	4·32	15·50
Circle 36	34·40	26·56	7·84	30·48
4 rosin	33·40	26·25	7·15	29·82

Mar. 24. P. 91.

Circle 36	33·65	27·49	6·16	30·57
Plate air 1	30·75	25·01	5·74	27·88
4 rosin	31·76	25·32	6·44	28·54
rosin 1 + 2	18·34	14·51	3·83	16·42
3 + 4	18·78	14·51	4·27	16·64
Circle 18	18·11	13·82	4·29	15·96
Circle 9·3	9·56	6·48	3·08	8·02
Rosin 1	10·83	7·35	3·48	9·09
2	10·83	7·46	3·37	9·14
3	10·83	7·46	3·37	9·14
4	10·83	7·46	3·37	9·14

650] [Results of Art 649.] By Mar. 13. [Art 553.]

Double plate = circ. $18\frac{1}{2}$ + ·11 sq. inc., or ·12 inc. el.

Circ. 36 = 2 doub. + 2·67 inc. el. without allowance for communicating wire, &c., or 2·95 with.

D = 2 doub. − 1·60 with allowance.

$$\left.\begin{matrix}\text{Circ. } 36\\ \text{D}\end{matrix}\right\} = \text{Circ. } 18\tfrac{1}{2} \times 2 \begin{cases} +\ 3{\cdot}19 \\ -\ 1{\cdot}36 \end{cases}.$$

By Mar. 15 [Art. 553].

Doub. pl. = $18\frac{1}{2}$ + ·20.

$$\left.\begin{matrix}\text{D}\\ \text{pl. air}\\ \text{circ. } 36\end{matrix}\right\} = \text{circ. } 18\tfrac{1}{2} \times 2 \begin{cases} -\ \ {\cdot}91 \\ +\ \ {\cdot}53 \\ +\ 3{\cdot}66 \end{cases} = 2 \text{ doub.} \begin{cases} -\ 1{\cdot}51 \\ +\ \ {\cdot}13 \\ +\ 3{\cdot}26 \end{cases} \text{with allowance.}$$

651] All the following are with allowance.

Mar. 19 [Art. 554]. 1 ros. = circ. 9·3 + 1·01.
circ. $18\frac{1}{2}$ = 2 ros. − ·47 = circ. 9·3 × 2 + 1·55.
circ. 36 = 4 ros. + 2·61 = circ. $18\frac{1}{2}$ × 2 + 3·55.

Mar. 23 [Art. 554]. 1 ros.= circ. 9·3 + ·85.
circ. $18\frac{1}{2}$ = 2 ros. − ·63 = circ. 9·3 × 2 + 1·07.
circ. 36 = 4 ros. + 1·44 = circ. $18\frac{1}{2}$ × 2 + 2·76.
1·32

Mar. 24 [Art. 554]. 1 ros. = circ. 9·3 + 1·25.
circ. $18\frac{1}{2}$ = 2 ros. − ·36 = circ. 9·3 × 2 + 2·14.
circ. 36 = 4 ros. + 2·98 = circ. $18\frac{1}{2}$ × 2 + 3·70.
Plate air 1 = circ. 36 − 3·03 = circ. $18\frac{1}{2}$ × 2 + ·67.

By mean of all

circ. 36 = circ. $18\frac{1}{2}$ × 2 + 3·37.
circ. $18\frac{1}{2}$ = circ. 9·3 × 2 + 1·59.

Therefore charge of circle of

$$\begin{matrix}37 \text{ inc.}\\ 18\tfrac{1}{2} \text{ inc.}\end{matrix} \text{ exceeds } 2^{ce} \text{ charge of circ. of } \begin{matrix}18\tfrac{1}{2} \text{ by } 4{\cdot}47\\ 9\tfrac{1}{4} \text{ by } 1{\cdot}69\end{matrix},$$

37 inc. exceeds 4 times charge of $9\frac{1}{2}$ by 7·85.

652] *If charge circle is greater than it would be if placed at a great distance from any other body in ratio of $a : a - 36$,

charge circ. of $18\frac{1}{2}$ should exceed in ratio of $a : a - 18\frac{1}{2}$ and so on.

Therefore, if we suppose $a = 167$,

$$\text{charge circ.} \begin{cases} 36 \\ 18\tfrac{1}{2} \\ 9{\cdot}3 \end{cases} \text{should exceed its true charge by} \begin{cases} 9{\cdot}89 \\ 2{\cdot}31 \\ \ {\cdot}55 \end{cases},$$

and charge circ. 36 should exceed

$$\begin{matrix}2^{ce} \text{ charge of } 18\tfrac{1}{2} \text{ by } 4{\cdot}27\\ 4 \text{ times charge of } 9{\cdot}3 \text{ by } 6{\cdot}49\end{matrix}, \text{ which is } \begin{cases} {\cdot}90 \text{ greater} \\ {\cdot}06 \text{ less} \end{cases}$$

* [See Art. 338, and Note 24.]

than by experiment, and charge circ. 18½ should exceed 2ce charge of 9·3 by 1·11, which is ·58 less than by experiment.

We will therefore suppose that the charge of circ. 18½ or of globe 12·1, as found by experiment, exceeds the true charge in the ratio of 9 to 8, as it should do if $a = 167$.

653] 1771. P. 15 [Art. 456].

doub. B contains $\frac{3}{16}$ sq. inc. or ·21 circ. inc. less than circ. 18½.
doub. A contains $\frac{1}{8}$ sq. inc. or ·14 circ. inc. less than circ. 18½.

1772. P. 12 [Art. 478].

doub. B contains ·11 sq. inc. or ·12 circ. inc. more than circ. 18½.
doub. A contains ·23 sq. inc. or ·26 circ. inc. more than circ. 18½.

1773. P. 85 [Arts. 553 & 650]. Each doub. plate contains ·16 circ. inc. more than circ. 18½.

654] P. 15. 1771 [Art. 456]. Globe cont. $\frac{1\cdot25}{4}$ sq. inc. or ·35 circ. inc. more than circ. 18·5.

P. 12. 1772 [Art. 478]. Globe contains same as circ.

By mean, globe of 12·1 = circ. of 18·67, or
globe of 12 inc. = circ. of 18·5.

Therefore 1 circ. inc. = ·65 glob. inc.
or 1 sq. inc. = ·73 glob. inc.

Def. *The charge of globe 1 inc. diam. placed at great dist. from any other body is called 1 glob. inc.*

The circ. 18·5 = 13·5 glob. inc.*

The doub. plate A or B is supp. = 13·6 glob. inc.

655] P. 18, 1772 [Art. 483],
D, E, F & G cont. ·68 inc. el. less than 2 doub.

P. 19, 1773 [Art. 509],
D & F cont. 1 inc. less than do.

P. 59, 1773 [Art. 533],
D, E & F cont. 1¼ inc. less than do.

P. 85, 1773 [Art. 553],
——— D cont. 1·6 less than 2 doub.
——— D cont. 1·31 less than do.
——— D cont. 1·36 less than 2ce circ. 18½.
D cont. ·91 less than do.

D is supposed to cont. 1·3 circ. inc. or ·85 glob. inc. less than 2 doub., *id est*, 26·3 glob. inc.

* [See Note 35, p. 433.]

656] 1773, P. 28 [Art. 515],

M cont. 1 inc. el. more than D + E + F.

P. 29 [Art. 515],

M cont. same as ....

P. 54 [Art. 528],

$\left.\begin{array}{ll}\text{M} & \text{cont. } 2\frac{1}{2} \\ \text{K \& L} & 1\end{array}\right\}$ more than D + E + F. N. $16\frac{1}{2}$.

1773, P. 57 [Art. 530],

$\begin{array}{ll}\text{M} & \text{cont. } 2 \\ \text{K \& L} & 1\end{array}$ more than D + E + F. N. $14\frac{1}{2}$.

P. 57 [Art. 530],

$\begin{array}{ll}\text{M} & \text{cont. } 3\frac{1}{2} \\ \text{K \& L} & 2\frac{1}{2}\end{array}$ more than D + E + F. N. 16.

It is supp. that $\begin{array}{ll}\text{M} & \text{cont. } 2{\cdot}7 \\ \text{K \& L} & \text{— } 1{\cdot}5\end{array}$ more than D + E + F, *id est*, $\begin{array}{l}80{\cdot}7 \\ 79{\cdot}9\end{array}$ glob. inc. el.

657] 1773, P. 55 [Art. 529],

$\begin{array}{l}\text{A} \\ \text{B or C}\end{array}$ cont. $\begin{array}{l}30{\cdot}3 \\ 32\end{array}$ inc. el. less than K + L + M. N. 15.

P. 57 [Art. 530],

each cont. 33·7 less than do. N. $14\frac{1}{2}$.

P. 58 [Art. 531],

each cont. 38·6 less. N. 16.

It is supp. that A, B, and C each cont. 34·8 inc. less than K + L + M, *id est*, 29·1 less than 9D, *id est*, 217·8 glob. inc.

658] By exper. of 1772,

F or G	cont.	2	inc. more than D.
E	—	1·6	—
M	cont.	3·86	less than E + F + G.
K & L		12·02	less.
F		10·72	less.

Therefore E + F + G cont.	5·6	more than 3D.
M	1·7	more.
K or L	6·4	less.
F	5·1	less.
A, B & C each contain	15·2	less than F + K + L.
or	33·1	less than 9D.

1773, P. 56 [Art. 530],

H cont. 10 inc. more than A + B + C.

1772, P. 29 [Art. 493],

H cont. the same as A + B + C.

H is supposed to contain 654 glob. inc.*

659] *Instantaneous spreading of el.*† Measures P. 19 [Art. 593].

The area of the old coatings of	A = 33·9	and circumf. =	20·6
	C = 33·2		20·4
	H = 36·3		21·4
Area of slit coatings of	A = 31·8	and circumf.	73·5
	C = 30·4		76·5
	H = 33·3		80·1
	crown = 24·7		69·6
Area of oblong coatings of	A = 34·1	& circumf.	23·4
	C = 33·3		23·2
	H = 36·4		24·1
	crown = 29·0		21·6

660] P. 15, 1773 [Art. 504],

White Cyl. cont.	7	inc. el. less than H.	
P. 13 [Art. 502],	5		
By mean it cont.	6	less than H.	
P. 62 [Art. 536],			
H with slit coat. cont.	77·5	more than white cyl.	
crown with oblong coat.	33·7	....	N. 12.
P. 63 [Art. 536],			
H with D° cont.	99·1	more than wh. cyl.	N. 11.
crown	43·8	....	
P. 65 [Art. 537],			
H with D°	70·8	more than wh. cyl.	N. 14.
crown with slits	34		
P. 66 [Art. 537],			
crown D° cont.	20	more than wh. cyl.	N. 12½.
P. 71 [Art. 541],			
H D° cont.	74·1	more than wh. cyl.	N. 21.
crown	67·3		

* [See Art. 318.] † [See Art. 319.]

P. 81 [Art. 550],

H with obl. cont.	20·2 more than wh. cyl., st. el.* at	2 + 3	N. 15.
H	34	3 + 1	
crown	57·3		
H	9 less than wh. cyl.	1 + 3	
crown	14·6 more than		

P. 82 [Art. 550],

H	18·5 more than wh. cyl.	N. $14\frac{1}{2}$.

A and C with circ. coatings are supposed to contain same as B.

P. 62 [Art. 536],

A with slit coat. cont.	13·5	more than B	us. el.†	
C	13·5			
A	15·2		1 + 3	N. $12\frac{1}{2}$.
C	11·8			
A	33·7		3 + 1 very irreg.	
C	33·7			

P. 63 [Art. 536],

A	18·5	us. el.	N. 11.
C	15·2		
A	13·5	1 + 3	
C	10·1		
A	18·5	3 + 1 very irreg.	
C	18·5		

P. 65 [Art. 537],

A with obl.	3·4	more than B	us. el.	N. 14.
C	5·1			
A	5·1	——	1 + 3	
C	1·7			
A	0		3 + 1	
C	1·7	less than B		

P. 66 [Art. 537],

A	3·4	more than B	us. el.	N. $12\frac{1}{2}$.
C	5·1			

661] By mean H with slits cont. 78 inc. el. } more than wh. cyl.
with oblong 19 —— }

Crown with slits contains 27 inc. el. more than wh. cyl.
oblong 39 ——————

N.B. This is meant in dry weather & with usual deg. el.

The crown with slits exceeded wh. cyl. by 42·7 more with electrom. at 3 + 1 than at 1 + 3, and H with oblong exceeded wh. cyl. by 43 more with

* [Straw electrometer. See Art. 560, note.]

† [Usual degree of electrification. See Art. 329 and Note 10.]

electrom. at 3 + 1 than at 1 + 3, but it must be observed that this was only one day's observ.

With usual deg. el. $\genfrac{}{}{0pt}{}{A}{C}$ exceeded B by $\genfrac{}{}{0pt}{}{16}{14\cdot3}$

with electrom. at 1 + 3 by $\genfrac{}{}{0pt}{}{14\cdot3}{11}$

& at 3 + 1 by $\genfrac{}{}{0pt}{}{26\cdot1}{26\cdot1}$

$\genfrac{}{}{0pt}{}{A}{C}$ with oblong exceeded B with us. el. by $\genfrac{}{}{0pt}{}{3\cdot4}{5\cdot1}$

with electrom. at 1 + 3 by $\genfrac{}{}{0pt}{}{5\cdot1}{1\cdot7}$

and at 3 + 1 by $\genfrac{}{}{0pt}{}{0}{-1\cdot7}$

662] Hence we have the following results:—

	L. inc. el. in each sq. inc. circ. or obl. coa.	Inc. el. in slit coat. more than in oblong	Sq. inc. coating answering to D°	Sq. inc. of slit coat. equiv. to obl.	Sq. inc. in obl.	Diff.	Excess circum. slit coat. above oblong	Spreading of elect.
A with us. el.	9·959	12·6	1·27	30·53	34·1	3·57	50·1	·072
el. at 1 + 3		9·2	·93	30·87		3·23		·065
el. at 3 + 1		26·1	2·63	29·17		4·93		·098
C with us. el.	0·050	9·2	·91	29·49	33·3	3·81	53·3	·071
el. at 1 + 3		9·3	·92	29·48		3·82		·072
3 + 1		27·8	2·75	27·65		5·65		·106
H with us. el.	4·437	59	2·12	31·18	36·4	5·22	45·0	·094
Crown do.	5·552	− 12	− ·33	25·03	29·0	3·97	48·0	·083

663]

	Inc. el. in oblong coating − D° in circular	Sq. inc. of coating equiv. to D°	Sq. inc. of circular coating equiv. to oblong	Sq. inc. in oblong	Diff.	Excess circumf. oblong above circular	Sq. inc. equiv. to excess of spreading of elect. in oblong above that in circular
A	3·4	·34	34·24	34·1	·14	2·8	·20
C	5·1	·51	33·71	33·3	·41	2·8	·20
H	1·3	·46	36·76	36·4	·36	2·7	·25

It is plain that the numbers in the 8th or last col. ought to be equal to those in the 6th, as is nearly the case.

664] *Whether charge of coated glass bears the same proportion to that of another body whether el. is strong or weak**.

P. 61 [Art. 535], E on neg. side tried against sliding tin plates on pos.

Charge of E was $\frac{1}{80}$ part less with straw el. at 3 + 1 than at 1 + 3, the diff. between neg. and pos. el. was much too small to be certain of.

* [Arts. 356, 451, 463, 535, 539, 551.]

P. 66 [Art. 538], a ball blown at end of therm. tube tried in same manner. Charge just the same whether electrom. at 1 + 3 or 3 + 1.

P. 68 [Art. 538], charge D° $\frac{1}{50}$ less with el. at 3 + 1 than at 1 + 3.
——— D° $\frac{1}{37}$ ———

P. 82 & 84 [Arts. 551 & 555], tried with machine for finding quant. el. in common plates. No perceptible diff. between charge of E whether tried with el. at 1 + 3 or 3 + 1.

665] By P. 9 [Art. 661], it should seem that el. spread. ·034 inc. more on surface with greater degree of el. than with smaller, and therefore, as the diam. coating of E or D is 2·16.

So that it should seem as if the charge of a coated plate in which the spreading of the el. was prevented would be at least $\frac{1}{16}$ less with the stronger degree el. than with the weaker.

666] By exper. of P. 69 [Art. 539], it appeared that the charge of tin cyl. was to that of D + E when electrified very weakly as 1·28 to 1, and by P. 70 [Art. 539] as 1·24 to 1. By mean as 1·26 to 1*.

By mean of P. 76 [Art. 545], the charge of the same cyl. was to that of D + E when electrified in the usual degree as 1·33 to 1.

By mean of P. 77 [Art. 546], it came out as 1·37 to 1, but this last can not be depended on, as wire for making communication with ground was forgot to be fixed †.

667] It should seem that the charge of D and E is increased $\frac{10}{78}$ by spreading of el. when elect. in usual degree, therefore if we suppose that the spreading is insensible when electrified in very small degree, the charge of a glass plate is less in proportion to that of another body when electrified with usual degree el. than when elect. with a very small one in ratio of 1·26 to 1·51, or of 5 to 6.

668] *On plate air* ‡.

[By Art. 517],

$$\left.\begin{array}{l}\text{P. 32 pl. air 1 cont. 1 inc. el. more than D}\\ \quad 33 \qquad\qquad\qquad \tfrac{3}{4}\end{array}\right\}\text{ by mean } \tfrac{7}{8} \text{ more than D.}$$

The same plate air contained 2 inc. el. less when resting intirely on machine than when resting by 1 corner.

* [See Arts. 358, 539, 545.]

† The comp. charge of the cyl. is 48·4 glob. inc. The real charge, supposing that the wire contains 3·6 glob. inc. less when joined to cyl. than to D + E = 73·6, and therefore its real charge exceeds the computed in the ratio of 1·52 to 1. [See Note 25.]

‡ [See Art. 340.]

[By Art. 517], Pl. air 2 cont. 1 inc. el. less than D + E.
P. 32, pl. air 3 10·5 inc. el. less than D + E + F.
P. 33, pl. air 4 1 inc. el. less than D + E + F.
P. 36, pl. air 5 ¼ more than D.
P. 37, Do.

By res. P. 5 [Art. 653], D, E, and F cont. 26·3 glob. inc.

Therefore pl. air 1 contains 27 glob. inc.

2	52
3	72·1
4	78·3
5	26·5

669] [Table of plates of air given in Art. 343.]

670]

Plate air	Log. diam. by thickness				
1	1·1017	·7919	7452	6928	6332
2	1·4375	·9426	8689	7799	6679
3	1·6013	1·0163	9235	8054	6427
4	1·6525	·9747	8566	6939	4307
5	1·3895	·9458	8809	8045	7117

The 4 last columns are the log. of $\frac{\text{diam.}}{\text{thickness}} \times$ excess $\frac{\text{real charge}}{\text{computed charge}}$ above N, the value of N in 3rd {?} col. being 1, in 2nd 1·05, in 3rd 1·1, and in 4th 1·15.

The numbers in the 3rd column seem most uniform, and therefore it seems likely that [if] the $\frac{\text{diam.}}{\text{thickness}}$ was very great, $\frac{\text{real charge}}{\text{comp. charge}}$ would equal 1·1*.

671] If we suppose the el. to spread ·07 inc. on surf. thick plates and ·09 on surf. thin ones, the result of Nairne's plates is as follows†.

	Diam.	Do. corrected	Thick-ness	Computed charge	Real charge	Real charge by com-puted	Real charge by diam.
D	2·155	2·295	·2057	3·20	26·3	8·22	
E	2·16	2·3	·2065	3·20	26·3	8·22	11·4
F	2·175	2·315	·2115	3·17	26·3	8·30	
K	2·265	2·445	·07712	9·69	79·9	8·29	
L	2·335	2·515	·08205	9·63	79·9	8·29	
M	2·195	2;375	·07187	9·81	80·7	8·23	32·6
A	6·57	6·71	·2112	26·6	217·8	8·18	
B	6·6	6·74	·2132	26·6	217·8	8·18	
C	6·5	6·64	·2065	26·7	217·8	8·16	
H	6·8	6·98	·07556	80·6	654	8·11	93·7

* [See Art. 347.]

† [In Art. 324 the "Real charges" of this table are multiplied by ·122 for easy comparison with the computed charges.]

672] *Computations of other flat plates of glass, &c.*

						Mean charge
[Art. 507]	P. 18	thick white	= D			
[Art. 508]	P. 19		D°.		26·3	
	P.	thin white	= D + E − ·5	·33	52·3	
	P. 19	N	= D + E − 1·8	1·2	51·9	
[Art. 509]	P. 20	P	= M − 15	9·7	71	71·9
[Art. 515]	P. 28		= D + E + F − 9·5	6·1	72·8	
[Art. 509]	P. 20	Q	= M − 9	5·7	74·8	76·5
[Art. 515]	P. 28		= D + E + F − 1·1	·7	78·2	
[Art. 509]	P. 20	O	= M − 9	5·7	74·8	75
[Art. 515]	P. 28		= D + E + F − 5·8	3·8	75·1	
[Art. 509]	P. 20	white plate	= M − 7·7	5	75·7	
[Art. 515]	P. 28		= D + E + F − 7·7	5	73·9	
[Art. 509]	P. 20	old G	= M − 7·3	4·8	75·9	
[Art. 515]	P. 28		= D + E + F − 5·8	3·8	75·1	
[Art. 510]	P. 21	crown A	= A − 13	8·5	211·3	
[Art. 533]	P. 59		− 6·7	4·4		
[Art. 510]	P. 21	crown C	= A − 13	8·5	208·7	
[Art. 533]	P. 59		− 15	9·8		
[Art. 527]	P. 53	small ground crown	= D + E + F − 4½			
[Art. 528]	P. 54		− 3½	2·4	76·5	
[Art. 531]	P. 57		− 3			
[Art. 527]	P. 53	large ground crown	= C − 13			
[Art. 528]	P. 54		− 2	2·7	215·1	
[Art. 531]	P. 57		0			
[Art. 533]	P. 59		− 1·7			
[Art. 507]	P. 18	exper. rosin 1	= doub. B − 1	·06	13·5	
		rosin 2	E − 2			
[Art. 519]	P. 36		− 1·7	1·1	25·2	
[Art. 509]	P. 20	rosin 3	= M − 17	11·1	69	
[Art. 515]	P. 28		= D + E + F − 16	10·4		
[Art. 518]	P. 35	rosin 4	= D + E + F		78·9	
[Art. 519]	P. 36		= D°.			
[Art. 527]	P. 53	rosin 5	= doub. B − 1	·65	13	
[Art. 528]	P. 54		− 1			
[Art. 518]	P. 35	1st made ros.	= D + 2		·6	26·9
[Art. 519]	P. 36		0			
[Art. 518]	P. 35	deph. bees wax 1	= D − 2½		1·8	24·5
[Art. 519]	P. 36		− 3			
[Art. 518]	P. 35	deph. bees wax 2	= E + F − 4		2·1	50·5
			− 2·5			
[Art. 527]	P. 53	deph. bees wax 3	= E + F − 7·5			
[Art. 528]	P. 54		− 11	$\frac{30}{3}$	6·5	46·1
[Art. 533]	P. 59		− 11			
[Art. 527]	P. 53	plain bees wax	= E + F − 3·5			
[Art. 528]	P. 54		0	$\frac{6}{3}$	1·3	51·3
[Art. 533]	P. 59		− 2·5			
[Art. 518]	P. 35	lac	= D + E + F + 1·5	$\frac{3·7}{2}$	1·1	80
[Art. 519]	P. 36		+ 2·2			

673] [Table given in Art. 370.]

The diam. was corrected on supposition that elect. spreads ·07 if the thickness of glass = ·21, and ·09 if thickness = ·08, and so in proportion in other thicknesses.

674] [Table given in Art. 371.]

The correction of the diameter is the same as would be used according to the preceding rule to a glass plate of 2ce the thickness, only the correction used is never less than $\frac{1}{10}$ inch.

675] *On the glass cylinders.*

			inc. el.	
503, P. 14,	gr. cyl. 2.	= H + 45 inc. el.	H + D + 14·3	= 690 glob. inc.
504, P. 15,		H + 55·6		
503, P. 14,	white jar	= H + 74 = H + M − 34	H + M − 27	= 717
504, P. 15,		H + M − 20		
504, P. 15,	gr. cyl. 1	= H + M + 30		= 754
502, P. 13,	gr. cyl. 4	= C + K + J × $\frac{23}{24}$		= 353
545, P. 75,	therm. tube 1	= D + E + F + 2 inc. el.		= 80·2
546, P. 77,	therm. tube 2	= D + E + F + 2·8		= 80·7

676] [Table given in Art. 383.]

The white jar and cyl. and the 3 green cyl. are corrected for the spreading of the electricity in the same manner as the flat plates, but the 2 therm. tubes are not.

677] *On the compound plates**.

P. 60, Art. 534.

The 3 plates A, B and C placed over each other with bits of lead between contained 8·9 inc. el. less than K or L, therefore its charge = 74 inc. The 3rd part of the charge of A, B, or C is 72·6 inc.

The coatings taken from the 3 plates A, B, & C, the plates placed close together and the outside surfaces coated with circles 6·6 in diam.

544, P. 75, it contained 7·5 inc. less than D + E + F.
546, P. 77, 6 less.
By mean it contains 6·7 less, therefore charge = 74·5.

The thickness of the 3 plates together is ·6309. The computed charge of a plate of that thickness with a coating 6·6 in diam. supposing the el. to spread ·07 inc. is 9·00, and the real charge of such a plate according to the mean ratio of the real and computed charges of D, E, and F is 74·2.

678] A plate of exper. rosin about 8 inc. square was pressed out, thickness irregular, but at a medium about ·122. It was coated with circles 6·61 in diam.†

Art. 548, P. 79, its charge = K + D + E × 1 + $\frac{1}{66}$,
in afternoon × 1 + $\frac{1}{44}$.

By mean it = K + D + E × 1 + $\frac{1}{55}$ = 135.

The real charge of this plate is to its computed, supposing the el. to spread ·07 inc., as 2·89 to 1.

The charge of this plate is the same as that of a glass one ·345 thick, supposing ratio of real and computed charge the same as in A or B.

* [Arts. 379, 534.] † [Arts. 381, 552.]

679] The coatings being taken from this plate it was included between the plates B and H, and the outside surfaces coated with circles of 6·6 in. diam.

552, P. 83, it cont. 6·9 inc. el. less than K,
6·5 ———
5·2 less than D + E + F,

by mean it contains 75·5 glob. inc.

The charge of plate glass of the same sort as Nairne's ·634 thick (*id est*, equal to the sum of the thicknesses of the two glass plates and a glass plate equiv. to the rosin) = 73·3, supposing the el. to spread the same on this plate as on the rosin.

680] [Same as Art. 368.]

681] By res. P. 5 [Art. 654] a globe of 12 inc. contains as much el. as a circle of 18·5 *, therefore by Prop. XXIX, $p = \frac{11}{13}$, therefore

	Charge of both plates when distant 18	26	36	Sing. pl.
if $p = \frac{11}{13}$	1	1·046	1·078	1·172
0	1	1·062	1·108	1·249
inf[inite]	1	1·035	1·059	1·126
or if $p = \frac{11}{13}$	·853	·892	·920	1
0	·801	·851	·887	1
inf.	·888	·919	·940	1

By 1st exp. 1772 [Art. 473] the proportions were

thus	·811	·859	·899	1†
By 2nd exp. [Art. 475]	·798	·840	·894	1

682] The charges of the following bodies are supposed to bear the following proportions to each other ‡.

globe 12·1 diam. =			1
circle 18·5 diam. =			·992
square of 15·5			·958
oblong 17·9 × 13·4			·964
cyl.	35·9 by	2·53	1·028
	54·2	·73	·978
	72	·185	·966

Charge by theory of {sh[ort] cyl. / long cyl. / wire} is between {·887 & 1·469 / ·896 & 1·573 / ·894 & 1·619} that of globe being one, and

* [Note 35.] † [Note 21.]
‡ [Exp. VII, Art. 281. The numbers here are different. See Art. 478.]

if charge cyl. is supposed to be to that of globe whose diam. = length cyl: :: $\frac{9}{8}$: N. L. $\frac{2\text{ length}}{\text{diam.}}$, their charge $= \begin{matrix} \cdot 998 \\ 1\cdot 008 \\ 1\cdot 006 \end{matrix}$.

This ratio approaches about 5 times nearer to the first proportion than the 2nd*.

The area of the oblong is the same as that of the square, and their charges are very nearly the same.

The charge of a square is to that of a circle whose diam. = side square as 1·153 to 1†.

683] In exper. P. 11, 1772 [Art. 477], the large wire should contain about $\frac{1}{93}$ less el. than if its diam. was double the small ones. Allowing for this, the charges of the large wire at 36, 24 & 18 inc. dist. should be between the two following proportions:

1	·942	·915	·891,
1	·901	·868	·844,

but I believe ought to approach about 5 times nearer to the former. The observed proportions are ‡

1	·903	·860	·850.

* [Note 12.]

† [This ratio is given in Art. 283 as 1·53 by a mistake of the Editor in copying, see Note 22.]

‡ [Note 13.]

RESULTS {OF EXPERIMENTS ON RESISTANCE OF SOLUTIONS, ARTS. 616—646}

{From MS. N°. 19. See Table of Contents at the beginning of this volume.}

684] Resistance of $\left.\begin{array}{l}\text{Pump-water is} \\ \text{Salt in 1000 of rain water} \\ \text{Sea water}\end{array}\quad\begin{array}{r}4\frac{1}{6} \\ 9 \\ 100\end{array}\right\}$ times less than that of rain water*.

685] A shock is diminished very nearly the same, but if anything rather more, by passing through 9 tubes, 37 inches of which hold 3373 grains of ☿, than through one tube, 37 inches of which hold 3480 grains of ☿ †.

686] A shock is as much diminished in passing through 6·8 inches of a tube, 37 inches of which hold 567 grains, as through 44¼ of one 37 inches of which hold 3480. So that resistance should seem as 1·03 power of velocity‡.

687] If resistance is as $\begin{cases}1{\cdot}03 \\ 1{\cdot}08\end{cases}$ power of velocity, the resistance of iron wire is $\begin{cases}437000 \\ 607000\end{cases}$ times less than that of saturated solution of sea salt§.

688] Resistance of sat. sol. S. S. in 99 of distilled water is 39 times greater than that of the sat. sol.

Resistance of distilled water is 18 times greater than that of sat. sol. in 99 of distilled water‖.

689¶] *Experiments in 1776 and 1777.*

No. of Exp.		Conducts times better than		Tubes	Electro-meter	
1	Sat. sol.	8·6	salt in 69	14 & 15	4	
2		8·94	——	22 & 23	1¼	
3		9·61	——	14 & 15	3	New solutions.
6		10·05		5 & 17	1	D° jars 1 & 2, new sol. battery.
7		10·31		5 & 17		
10		9·02		22 & 23	1	
9		7·79	salt in 29 diluted with 1⅓ of water supp. 2⅓.	22 & 23	1	

* [Arts. 398, 524.] † [Arts. 574, 575, &c.] ‡ [Arts. 575, 629.]
§ [Arts. 398, 576, and Note 32.] ‖ [Art. 577.]
¶ [Arts. 617–623.]

No. of Exp.		Conducts times better than		Tubes	Electro-meter
8	sat. sol.	3·51	salt in 29	22 & 23	1
15	——	4·38	——	22 & 23	1¼
11	sat. sol.	20·5	salt in 149	14 & 15	3½
12	——	19·6	——	22 & 23	1¼
4	salt in 69	9·57	salt in 999	22 & 23	1¾
5	salt in 999	9·92	salt in 20,000	12 & 20	1¾
13	salt in 149	17·3	salt in 2999	5 & 17	3
14	——	16·7	——	18 & 19	1¾

N.B. It is not said what water the solutions were made with. By the comparison of salt in 999 with salt in 20,000, it should seem either that they were not made with distilled water, or that some mistake was made in the experiment.

690] In Jan. 1777, salt in 2999 conducted about 70 or 90 times better than some water distilled in the preceding summer, or about 25 or 50 times better than the distilled water used in the year 1776*.

Salt in 2999 conducted about 25 times better than salt in 150,000.

691] Salt in 69 conducts 1·97 times better in heat of 105° than in that of 58°½†.

The proportion of the resistance of sat. sol. and salt in 999 to each other seems not much altered by varying heat from 50° to 95°‡.

692] Salt in 150,000 seemed to conduct rather better than the same water deprived of air by boiling in the same vial in which it was kept, and cooled quick in water to prevent its absorbing much air. But the difference was not more than might arise from error of experiment§.

693] Distilled water impregnated with fixed air from oil of vitriol and marble conducted 2½ times better than the same water deprived of its air by boiling||.

694] Conducting power of other saline solutions compared with that of salt in 29 of water¶.

Sal. Sylvii	1·08
Sal. amm.	1·13
Calc. S. S.	·852
Glaub. salt	·696
Quadran. Nitre	·887
F. alk.	·819
Spt. salt	1·72
Oil vitr.	·783
D° another parcel	1·12

* [Art. 621.] † [Art. 619.] ‡ [Art. 620.]
§ [See Art. 624.] || [Art. 625.] ¶ [Art. 626 and Note 34.]

N.B. The solutions of the neutral salts were all of such strength that the acid in them was equiv. to that in salt in 29.

The f. alk. also was equiv. to that in salt in 29, but the acids were equiv. to that in salt in 59.

695] *Experiments in Jan.*, 1781*.

					Tubes	Electro-meter	
Sat. Sol.	conducts	8·63	times better than	salt in 69	14 & 15	$3\frac{1}{4}$	8·8
——		9·03		——	——	—	
Sat. Sol.		4·1		salt in 29	——	—	3·97
——		3·85		——	——	—	
——		3·95		——	——	$1\frac{3}{8}$†	
Sat. Sol.		1·92		salt in 11	——	$1\frac{3}{8}$	1·91
——		1·88		——	——	—	
——		1·92		——	——	—	
Salt in 69		1·74		salt in 142	22 & 23	2	1·79
——		1·84		——	——	—	
Salt in 69		9·91		salt in 999	——	$3\frac{1}{2}$	10·57
——		10·3		——		—	
——		11·31		——	5 & 17	$1\frac{1}{2}$	
——		10·75		——		—	
Salt in 999		20		salt in 20,000	12 & 20	$2\frac{1}{2}$	19·6
——		19·5		——		2	
——		19		——		$2\frac{1}{2}$	
——		19·8 .		——		2	

Salt in 20,000 conducts about 7 times better than distilled water.

696] Therefore the resistance of water with different quantities of salt in [it] are as follows‡:

Quantity salt	Resistance	Log. do.	Resist. × quant. salt	Log. do.
1 by 3·78	1			
12	1·91	·2810	·602	9·7793
30	3·97	·5988	·500	9·6992
70	8·8	·9445	·475	9·6769
143	15·75	1·1973	·416	9·6195
1000	93·02	1·9686	·352	9·5461
20000	1823	3·2608	·345	9·5373

* [Art. 628.] † {Stated in Art. 628 as $1\frac{1}{2}$.} ‡ [See Note 33.]

NOTES BY THE EDITOR

{JAMES CLERK MAXWELL}

{See Table of Contents at the beginning of this volume.}

NOTE 1, ARTS. 5 AND 67.

On the theory of the Electric Fluid.

The theory of One Electric Fluid is here stated very completely by Cavendish*. The fluid, as imagined by him, is not a purely hypothetical substance, which has no properties except those which are attributed to it for the purpose of explaining phenomena. He calls it an elastic fluid, and supposes that its particles and those of other matter have certain properties of mutual repulsion or of attraction, just as he supposes that the particles of air are indued with a property of mutual repulsion, but according to a different law. See Art. 97 and Note 6. But in addition to these properties, which are all that are necessary for the theory, he supposes that the electric fluid possesses the general properties of other kinds of matter. In Art. 5 he speaks of the weight of the electric fluid, and of one grain of electric fluid, which implies that a certain quantity of the electric fluid would be dynamically equivalent to one grain, that is to say, in the language of Boscovich and modern writers, it would be equal in *mass* to one grain.

We must not suppose that the word weight is here used in the modern sense of the force with which a body is attracted by the earth, for in the case of the electric fluid this force depends entirely on the electrical condition of the earth, and would act upward if the earth were overcharged and downward if the earth were undercharged.

Cavendish also supposes that there is a limit to the quantity of the electric fluid which can be collected in a given space. He speaks (Art. 20) of the electric fluid being pressed close together so that its particles shall touch each other. This implies that when the centres of the particles approach to within a certain distance, the repulsion, which up to that point varied as the n^{th} power of the distance, now varies much more rapidly, so that for an exceedingly small diminution of distance the mutual repulsion increases to such a degree that no force which we can bring to bear on the particles is able to overcome it.

We may consider this departure from the simplicity of the law of force as introduced in order to extend the property of "impenetrability" to the particles of the electric fluid. It leads to the conclusion that there is a certain maximum

* For an earlier form of Cavendish's theory of electricity, see "Thoughts concerning electricity" (Arts. 195–216), and Note 18.

density beyond which the fluid cannot be accumulated, and that therefore the stratum of the electric fluid collected at the surface of electrified bodies has a finite thickness.

No experimental evidence, however, has as yet been obtained of any limit to the quantity of electricity which can be collected within a given volume, or any measure of the thickness of the electric stratum on the surface of conductors*, so that if we wish to maintain the doctrine of a maximum density, we must suppose this density to be exceedingly great compared with the density of the electric fluid in saturated bodies.

A difficulty of far greater magnitude arises in the case of undercharged bodies. It is a consequence of the theory that there is a stratum near the surface of an undercharged body which is entirely deprived of electricity, the rest of the body being saturated. Hence the electric phenomena of an undercharged body depend entirely upon the matter forming this stratum. Now, though on account of our ignorance of the electric fluid we are at liberty to suppose a very large quantity of it to be collected within a small space, we cannot make any such supposition with respect to ordinary matter, the density of which is known.

In the first place, it is manifestly impossible to deprive any body of a greater quantity of the electric fluid than it contains. It is found, indeed, that there is a limit to the negative† charge which can be given to a body, but this limit depends not on the quantity of matter in the body but on the area of its surface, and on the dielectric medium which surrounds it. Thus it appears from the experiments of Sir W. Thomson and those of Mr Macfarlane, that in air at the ordinary pressure and temperature a charge of more than 5 units of electricity [per cm.^{2},] positive or negative, can exist on the surface of an electrified body without producing a discharge. In other media the maximum charge is different. In paraffin oil, and in turpentine, for instance, it is much greater than in air‡. In air of a few millimetres pressure it is much less, but in the most perfect vacuum hitherto made, the charge which may be accumulated before discharge occurs is probably very great indeed.

Now this charge, or undercharge, whatever be its magnitude, can be accumulated on the surface of the thinnest gold leaf as well as on the most massive

* [The modern molecular theories of the phenomena of galvanic polarization touch on this subject.]

† [The phenomena of discharge in dielectrics, especially of negative electrons as indicated in the text, are of course now much more thoroughly understood. Cf. Sir J. J. Thomson's treatise on *Conduction of Electricity through Gases*, or Prof. J. S. Townsend's book on *The theory of ionization...by collision*.]

‡ By Messrs Macfarlane and Playfair's experiments the maximum electromotive intensity is 364 for paraffin oil and 338 for turpentine. For air it is 73, between disks one centimetre apart. (*Trans. R. S. Ed.* 1878.) They have since found that the electric strength of the vapour of a certain liquid paraffin at 50 mm. pressure is 1·7 times that of air at the same pressure, and that the electric strength of a solid paraffin which melts at 22°·7 C. is 2·5 when liquid and 5 when solid, that of air being 1.

conductors. Suppose that there is a deficiency of five units of electricity for each square centimetre of the surface on both sides of a sheet of gold leaf whose thickness is the hundred thousandth part of a centimetre. We have no reason to believe the gold leaf to be entirely deprived of electricity, but even if it were, we must admit that every cubic centimetre of gold requires more than a million units of electricity to saturate it.

But we have by no means reached the limit of our experimental evidence. For Cavendish shows in Art. 49 that if in any portion of a bent canal the repulsion of overcharged bodies is so great as to drive all the fluid out of that portion, then the canal will no longer allow the fluid to run freely from one end to the other, any more than a siphon will equalize the pressure of water in two vessels, when the water does not rise to the bend of the siphon.

Hence if we could make the canal narrow enough, and the electric repulsion of bodies near the bend of the canal strong enough, we might have two conductors connected by a conducting canal but not reduced to the same potential, and this might be tested by afterwards connecting them by means of a conductor which does not pass close to any overcharged body, for this conductor will immediately reduce the two bodies to the same potential.

Such an experiment, if successful, would determine at once which kind of electricity ought to be reckoned positive, for, as Cavendish remarks in Art. 50, the presence of an undercharged body near the bend of the canal would not prevent the flow of electricity.

But even if the electric fluid were not all driven out of the canal, but only out of a stratum near the surface, the effective conducting channel would thereby be narrowed, and the resistance of the canal to an electric current increased.

Now we may construct the canal of a strip of the thinnest gold leaf, and we may measure its electric resistance to within one part in ten thousand, so that if the presence of an overcharged body near the gold leaf were to drive the electric fluid out of a stratum of it amounting to the ten thousandth part of its thickness, the alteration might be detected. Hence we must admit either that the one-fluid theory is wrong, or that every cubic centimetre of gold contains more than ten thousand million units of electricity.

The statement which Cavendish gives of the action between portions of the electric fluid and between the electric fluid and ordinary matter is nearly, but not quite, as general as it can be made.

Since the mode in which the force varies with the distance is the same in all cases, we may suppose the distance unity. Two equal portions of the electric fluid which at this distance repel each other with a force unity are defined to be each one unit of electricity.

Let the attraction between a unit of the electric fluid and a gramme of matter be a. Since we may suppose this force different for different kinds of matter, we shall distinguish the attraction due to different kinds of matter by different suffixes, as a_1 and a_2. Let the repulsion between two grammes of matter entirely deprived of electricity be r_{12}, these two portions of matter being of the kinds corresponding to the suffixes 1 and 2.

Now consider a body containing M grammes of matter and F units of the electric fluid. The repulsion between this body and a unit of the electric fluid at distance unity is

$$F - Ma. \qquad \text{......(1)}$$

If this expression is zero, the body will neither repel nor attract the electric fluid. In this case the body is said to be saturated with the electric fluid, and the condition of saturation is that every gramme of matter contains a units of the electric fluid. From what we have already said, it is plain that a must be a number reckoned by thousands of millions at least. The definition of saturation as given by Cavendish is somewhat different from this, although on his own hypothesis it leads to identical results. He makes the condition of saturation to be (in Art. 6) "that the attraction of the electric fluid in any small part of the body on a given particle of *matter* shall be equal to the repulsion of the matter in the same small part on the same particle." Hence this condition is expressed by the equation

$$Fa = Mr. \qquad \text{......(2)}$$

But as the essential property of a saturated body is that it does not disturb the distribution of electricity in neighbouring conductors, we must consider the true definition of saturation to be that there is no action on the *electric fluid*.

Now [following Cavendish's ideas] consider two bodies of different kinds of matter M_1 and M_2, and let each of them be saturated.

The quantity of electric fluid in the first will be

$$F_1 = M_1 a_1, \qquad \text{......(3)}$$

and that in the second

$$F_2 = M_2 a_2. \qquad \text{......(4)}$$

The repulsion between the two bodies will be

$$F_1 F_2 - F_1 M_2 a_2 - F_2 M_1 a_1 + M_1 M_2 r_{12}, \qquad \text{......(5)}$$

or, substituting the values of F_1 and F_2, and changing the signs, it will be an *attraction* equal to

$$M_1 M_2 (a_1 a_2 - r_{12}). \qquad \text{......(6)}$$

Now we know that the action between two saturated bodies is an attraction equal to

$$M_1 M_2 k, \qquad \text{......(7)}$$

where k is the constant of gravitation.

Hence we must make

$$a_1 a_2 - r_{12} = k \qquad \text{......(8)}$$

for every two kinds of matter, k being the same for all kinds of matter.

According to Baily's repetition of Cavendish's experiment for determining the mean density of the earth*,

$$k = 6{\cdot}506 \times 10^{-8} \frac{(\text{centimetre})^3}{\text{gramme . second}}. \qquad \text{......(9)}$$

* Baily's adopted mean for the earth's density is 5·6604, which, with the values of the earth's dimensions and of the intensity of gravity at the earth's surface *used by Baily himself*, gives the above value of k as the direct result of his experiments. [Cavendish's value 5·45 has been shown by modern determinations to be too small by less than two per cent.]

This number is exceedingly small compared to the product a_1a_2, which is of the order 10^{20} at least. Hence r_{12}, the repulsion between two grammes of matter entirely deprived of electricity, is of the same order as a_1a_2.

If we consider the attraction of gravitation as something quite independent of the attractions and repulsions observed in electrical phenomena, we may suppose

$$a_1a_2 - r_{12} = 0, \qquad \text{......(10)}$$

so that two saturated bodies neither attract nor repel each other.

Now we have adopted as the condition of saturation, that neither body acts on the electric fluid in the other. But since neither body acts on the other as a whole [gravitation now being a separate phenomenon], each has no action on the matter in the other, so that our definition of saturation coincides with that given by Cavendish.

Lastly, let the two bodies not be saturated with electricity, but contain quantities $F_1 + E_1$ and $F_2 + E_2$ respectively, where $F_1 = a_1M_1$, and $F_2 = a_2M_2$, and E_1 and E_2 may be either positive or negative, provided that $F + E$ must in no case be negative.

The repulsion between the bodies is

$$(F_1 + E_1)(F_2 + E_2) - (F_1 + E_1) M_2a_2 - (F_2 + E_2) M_1a_1 + M_1M_2r_{12}, \text{...(11)}$$

and this by means of equations (3) (4) and (10) is reduced to

$$E_1 E_2.$$

Theory of Two Fluids.

In the theory of Two Electric Fluids, let V denote the quantity of the Vitreous fluid and R that of the Resinous.

Let the repulsion between two units of the same fluid be b, and let the attraction between two units of different fluids be c.

Let the attraction between a unit of either fluid and a gramme of matter be a, and let the repulsion between two grammes of matter be r.

If a body contains V_1 units of vitreous, R_1 units of resinous electricity, and M_1 grammes of matter, its repulsion on a unit of vitreous electricity will be

$$V_1b - R_1c - M_1a_1,$$

and the repulsion on a unit of resinous electricity

$$-V_1c + R_1b - M_1a_1.$$

The definition of saturation is that there shall be no action on either kind of electricity. Hence, equating each of these expressions to zero, we find as the conditions of saturation

$$V_1 = R_1 = M_1 \frac{a_1}{b - c}.$$

The total repulsion between the two bodies is

$$(V_1V_2+R_1R_2)\,b-(V_1R_2+V_2R_1)\,c-(V_1+R_1)\,M_2a_2-(V_2+R_2)\,M_1a_1+M_1M_2r_{12}.$$

If we now put
$$V_1=M_1\frac{a_1}{b-c}+\tfrac{1}{2}S_1+\tfrac{1}{2}E_1,$$

$$R_1=M_1\frac{a_1}{b-c}+\tfrac{1}{2}S_1-\tfrac{1}{2}E_1,$$

$$V_2=M_2\frac{a_2}{b-c}+\tfrac{1}{2}S_2+\tfrac{1}{2}E_2,$$

$$R_2=M_2\frac{a_2}{b-c}+\tfrac{1}{2}S_2-\tfrac{1}{2}E_2,$$

the total repulsion becomes

$$M_1M_2\left(r_{12}-\frac{2a_1a_2}{b-c}\right)+E_1E_2\frac{b+c}{2}+S_1S_2\frac{b-c}{2}-S_1M_2a_2-S_2M_1a_1.$$

The first term of this expression, with its sign reversed, represents the attraction of gravitation, and the second term represents the observed electric action, but the other terms represent forces of a kind which have not hitherto been observed, and we must modify the theory so as to account for their non-existence.

One way of doing so is to suppose $b=c$ and $a_1=a_2=0$. The result of this hypothesis is to reduce the condition of saturation to that of the equality of the two fluids in the body, leaving the amount of each quite undetermined. It also fails to account for the observed action between the bodies themselves, since there is no action between them and the electric fluids.

The other way is to suppose that $S_1=S_2=0$, or that the sum of the quantities of the two fluids in a body always remains the same as when the body is saturated. This hypothesis is suggested by Priestley in his account of the two-fluid theory, but it is not a dynamical hypothesis, because it does not give a physical reason why the sum of these two quantities should be incapable of alteration, however their difference is varied.

The only dynamical hypothesis which appears to meet the case is to suppose that the vitreous and resinous fluids are both incompressible, and that the whole of space not occupied by matter is occupied by one or other of them. In a state of saturation they are mixed in equal proportions.

The two-fluid theory is thus considerably more difficult to reconcile with the facts than the one-fluid theory.

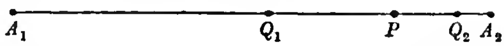
Note 2, Arts. 27 and 282.

[*Distribution in spheres and ellipsoids.*]

The problem of the distribution, in a sphere or ellipsoid, of a fluid, the particles of which repel each other with a force varying inversely as the n^{th} power of the distance, has been solved by Green*. Green's method is an extremely powerful one, and allows him to take account of the effect of any given system of external forces in altering the distribution.

If, however, we do not require to consider the effect of external forces, the following method enables us to solve the problem in an elementary manner. It consists in dividing the body into pairs of corresponding elements, and finding the condition that the repulsions of corresponding elements on a given particle shall be equal and opposite.

(1) *Specification of Corresponding Points on a line.*



Let A_1A_2 be a finite straight line, let P be a given point in the line, and let Q_1 and Q_2 be corresponding points in the segments A_1P and PA_2 respectively, the condition of correspondence being

$$\frac{1}{Q_1P} - \frac{1}{A_1P} = \frac{1}{PQ_2} - \frac{1}{PA_2}. \qquad \ldots\ldots(1)$$

It is easy to see that when Q_1 coincides with A_1, Q_2 coincides with A_2, and that as Q_1 moves from A_1 to P, Q_2 moves in the opposite direction from A_2 to P, so that when Q_1 coincides with P, Q_2 also coincides with P.

Let Q_1' and Q_2' be another pair of corresponding points, then

$$\frac{1}{Q_1'P} - \frac{1}{A_1P} = \frac{1}{PQ_2'} - \frac{1}{PA_2}. \qquad \ldots\ldots(2)$$

Subtracting (1) from (2)

$$\frac{1}{Q_1'P} - \frac{1}{Q_1P} = \frac{1}{PQ_2'} - \frac{1}{PQ_2}, \qquad \ldots\ldots(3)$$

or

$$\frac{Q_1Q_1'}{Q_1'P \,.\, Q_1P} = \frac{Q_2'Q_2}{PQ_2' \,.\, PQ_2}. \qquad \ldots\ldots(4)$$

If the points Q_1 and Q_1' are made to approach each other and ultimately to coincide, Q_1Q_1' ultimately becomes the fluxion of Q, which we may write $\dot{Q_1}$, and we have

$$\frac{\dot{Q_1}}{Q_1P^2} = \frac{\dot{Q_2}}{PQ_2^2}, \qquad \ldots\ldots(5)$$

* "Mathematical Investigations concerning the laws of the equilibrium of fluids analogous to the electric fluid, with other similar researches," *Transactions of the Cambridge Philosophical Society*, 1833. Read Nov. 12, 1832. See Mr Ferrers' Edition of Green's Papers, p. 119.

or corresponding elements of the two segments are in the ratio of the squares of their distances from P.

Let us now suppose that A_1PA_2 is a double cone of an exceedingly small aperture, having its vertex at P; let us also suppose that the density of the redundant fluid at Q_1 is ρ_1, and at Q_2 is ρ_2; then since the areas of the sections of the cone at Q_1 and Q_2 are as the squares of the distances from P, and since the lengths of corresponding elements are also, by (5), as the squares of their distances from P, the quantities of fluid in the two corresponding elements at Q_1 and Q_2 are as $\rho_1 Q_1P^4$ to $\rho_2 PQ_2^4$. If the repulsion is inversely as the n^{th} power of the distance, the condition of equilibrium of a particle of the fluid at P under the action of the fluid in the two corresponding elements at Q_1 and Q_2 is

$$\rho_1 Q_1 P^{4-n} = \rho_2 PQ_2^{4-n}. \qquad \ldots\ldots(6)$$

We have now to show how this condition may be satisfied by one and the same distribution of the fluid when P is any point within an ellipsoid or a sphere. We must therefore express ρ so that its value is independent of the position of P.

Transposing equation (1) we find

$$\frac{1}{Q_1P} + \frac{1}{PA_2} = \frac{1}{PQ_2} + \frac{1}{A_1P}. \qquad \ldots\ldots(7)$$

Multiplying the corresponding members of equations (1) and (7) and omitting the common factor $A_1P.PA_2$,

$$\frac{A_1Q_1 \cdot Q_1A_2}{Q_1P^2} = \frac{A_1Q_2 \cdot Q_2A_2}{PQ_2^2}, \qquad \ldots\ldots(8)$$

we may therefore write, instead of equation 6,

$$\rho_1 (A_1Q_1 \cdot Q_1A_2)^{\frac{4-n}{2}} = \rho_2 (A_1Q_2 \cdot Q_2A_2)^{\frac{4-n}{2}}. \qquad \ldots\ldots(9)$$

Let us now suppose that A_1A_2 is a chord of the ellipsoid, whose equation is

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1. \qquad \ldots\ldots(10)$$

If we write

$$1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} - \frac{z^2}{c^2} = p^2, \qquad \ldots\ldots(11)$$

then the product of the segments of the chord at Q_1 is to the product of the segments at Q_2 as the values of p^2 at these points respectively, or

$$A_1Q_1 \cdot Q_1A_2 : A_1Q_2 \cdot Q_2A_2 :: p_1^2 : p_2^2. \qquad \ldots\ldots(12)$$

We may therefore write, instead of equation (9),

$$\rho_1 p_1^{4-n} = \rho_2 p_2^{4-n}. \qquad \ldots\ldots(13)$$

If, therefore, throughout the ellipsoid,

$$\rho = Cp^{n-4}, \qquad \ldots\ldots(14)$$

where C is constant, every particle of the fluid within the ellipsoid will be in equilibrium.

We have in the next place to determine whether a distribution of this kind is physically possible.

Let E be the quantity of redundant fluid in the ellipsoid; then

$$E = C\int_0^1 p^{n-4}\, 4\pi abc\, p\,(1-p^2)^{\frac{1}{2}}\,dp$$

$$= 4\pi abc\, C\int_0^1 p^{n-3}\,(1-p^2)^{\frac{1}{2}}\,dp \qquad\qquad \text{......(15)}$$

$$= 2\pi abc\, C\,\frac{\Gamma\left(\frac{3}{2}\right)\Gamma\left(\frac{n-2}{2}\right)}{\Gamma\left(\frac{n+1}{2}\right)}. \qquad\qquad \text{......(16)}$$

Let ρ_0 be the density of the redundant fluid if it had been uniformly spread through the volume of the ellipsoid, then

$$E = \frac{4\pi}{3}\, abc\rho_0, \qquad\qquad \text{......(17)}$$

and if ρ is the actual density of the redundant fluid,

$$\rho = \rho_0\,\frac{2}{3}\,\frac{\Gamma\left(\frac{n+1}{2}\right)}{\Gamma\left(\frac{3}{2}\right)\Gamma\left(\frac{n-2}{2}\right)}\,p^{n-4}. \qquad\qquad \text{......(18)}$$

When n is not less than 2, there is no difficulty about the interpretation of this result.

The density of the redundant fluid is everywhere positive.

When $n = 4$ it is everywhere uniform and equal to ρ_0.

When n is greater than 4 the density is greatest at the centre and is zero at the surface, that is to say, in the language of Cavendish, the matter at the surface is saturated.

When n is between 2 and 4 the density of the redundant fluid at the centre is positive and it increases towards the surface. At the surface itself the density becomes infinite, but the quantity collected on the surface is insensible compared with the whole redundant fluid.

When n is equal to 2, $\Gamma\left(\frac{n-2}{2}\right)$ becomes infinite, and the value of ρ is zero for all points within the ellipsoid, so that the whole charge is collected on the surface, and the interior parts are exactly saturated, and this we find to be consistent with equilibrium.

When n is less than 2 the integral in equation (15) becomes infinite. Hence if we assume a value for C in the interior parts of the ellipsoid, we cannot extend the same law of distribution to the surface without introducing an infinite quantity of redundant fluid. We might therefore conclude that if the quantity of redundant fluid is given, we must make $C = 0$, and suppose the redundant fluid to be all collected at the surface, and the interior to be exactly saturated. But, on trying this distribution, we find that it is not consistent with equilibrium. For when n is less than 2, the effect of a shell of fluid on a particle within it is a force directed from the centre. If, therefore, a sphere of saturated matter is surrounded by a shell of electric fluid, the fluid in the sphere will be drawn towards the shell, and this process will go on till the different parts of the interior of the sphere are rendered undercharged to such a degree that each particle of fluid in the sphere is as much attracted to the centre by the matter of the sphere as it is repelled from it by the fluid in the sphere and the shell together. This is the same conclusion as that stated by Cavendish.

Green solves the problem, on the hypothesis of two fluids, in the following manner.

Suppose that the sphere, when saturated, contains a finite quantity, E, of the positive fluid, and an equal quantity of the negative fluid, and let a quantity, Q, of one of them, say the positive, be introduced into the sphere.

Let the whole of the positive fluid be spread uniformly over the surface of the sphere whose radius is a, so that if P' is the surface-density,

$$4\pi a^2 P' = E + Q.$$

Green then considers the equilibrium of fluid in an inner and concentric sphere of radius b, acted on by the fluid in the surface whose radius is a, and shows that if the density of the fluid is

$$\rho = \frac{2}{\pi} P'a \sin \frac{n-2}{2} \pi \, (a^2 - b^2)^{\frac{2-n}{2}} (a^2 - r^2)^{-1} (b^2 - r^2)^{\frac{n-2}{2}},$$

there will be equilibrium of the fluid within the inner sphere.

The value of ρ is evidently negative if n is less than 2.

Green then determines, from this value of the density, the whole quantity of fluid within the sphere whose radius is b, and then by equating this to $-E$, the whole quantity of negative fluid, he determines the radius, b, of the inner sphere, so that it shall just contain the whole of the negative fluid.

The whole of the positive fluid is thus condensed on the outer surface, the whole of the negative fluid distributed within the inner sphere, and the shell between the two spherical surfaces is entirely deprived of both fluids.

At the outer surface, the force on the positive fluid is from the centre, but the fluid there cannot move, because it is prevented by the insulating medium which surrounds the sphere.

In the shell between the two spherical surfaces the force on the positive fluid would be from the centre. Hence if any positive fluid enters this shell, it will be driven to the outer surface, and if any negative fluid enters, it will be driven to the inner surface.

But all the positive fluid is already at the outer surface, and all the negative fluid is already in the inner sphere, where, as Green has shown, it is in equilibrium, and thus the fluids are in equilibrium throughout the sphere.

It may be remarked that this solution, according to which a certain portion of matter becomes entirely deprived of both fluids, is inconsistent with the ordinary statements of the theory of two fluids, which usually assert that bodies, under all circumstances, contain immense quantities of both fluids.

In the two-fluid theory, by depriving matter of both fluids, we get an inactive substance which gives us no trouble, but in the one-fluid theory, matter deprived of fluid exerts a strong attraction on the fluid, the consideration of which would considerably complicate the mathematical problem.

[In connexion with this remark and with Note 3 a quotation from Green's Memoir of 1833 (§ 6) is relevant.

"In order to explain the phenomena which electrified bodies present, Philosophers have found it advantageous either to adopt the hypothesis of two fluids, the vitreous and resinous of Dufay for example, or to suppose with Æpinus and others, that the particles of matter when deprived of their natural quantity of electric fluid, possess a mutual repulsive force. It is easy to perceive that the mathematical laws of equilibrium deducible from these two hypotheses ought not to differ, when the quantity of fluid or fluids (according to the hypothesis we choose to adopt) which bodies in their natural state are supposed to contain is so great, that a complete decomposition shall never be effected by any forces to which they may be exposed, but that in every part of them a farther decomposition shall always be possible by the application of still greater forces. In fact the mathematical theory of electricity merely consists in determining ρ the analytical value of the fluid's density, so that the whole of the electrical actions exerted upon any point p, situated at will in the interior of the conducting bodies, may exactly destroy each other, and consequently p have no tendency to move in any direction. For the electric fluid itself, the exponent n is equal to 2, and the resulting value of ρ is always such as not to require that a complete decomposition should take place in the body under consideration; but there are certain values of n for which the resulting values of ρ will render $\int\rho dv$ greater than any assignable quantity; for some portions of the body it is therefore evident that how great soever the quantity of the fluid or fluids may be, which in a natural state this body is supposed to possess, it will then become impossible strictly to realize the analytical value of ρ, and therefore some modification at least will be rendered necessary, by the limit fixed to the quantity of fluid or fluids originally contained in the body, and as Dufay's hypothesis appears the more natural of the two, we shall keep this principally in view, when in what follows it may become requisite to introduce either."]

Infinite plate with plane parallel surfaces.

The distribution of the fluid in an infinite plate with plane parallel surfaces is given in the general solution which we have obtained for a body bounded by a quadric surface, namely, $\rho = Cp^{n-4}$.

In the case of the plate we must suppose it bounded by the planes $x = +a$, and $x = -a$, and then p is defined by the equation

$$x^2 = a^2 (1 - p^2).$$

If σ is the quantity of fluid in a portion of the plate whose area is unity,

$$\sigma = \int_{-a}^{+a} \rho\, dx = Ca \frac{\Gamma\left(\frac{n-2}{2}\right)\Gamma\left(\frac{1}{2}\right)}{\Gamma\left(\frac{n-1}{2}\right)}.$$

Thin disk.

The distribution in an infinitely thin disk may be deduced from that in an ellipsoid by making one of the axes infinitely small. It is better however to proceed by the method which we have already employed, only that instead of supposing the line A_1PA_2 (Fig. p. 358) to be a double cone, we suppose it to be a double sector cut from the disk. The breadth of this sector is proportional to the distance from P, so that the condition of equilibrium of the repulsions of two corresponding elements whose surface-densities are σ_1 and σ_2 is

$$\sigma_1 Q_1 P_1^{3-n} = \sigma_2 Q_2 P_2^{3-n},$$

whence we find, as before, that if the equation of the edge of the disk is

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1,$$

and if
$$1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} = p^2,$$

then the surface-density at any point is

$$\sigma = Cp^{n-3}.$$

The quantity of fluid in the disk is found by integrating over the surface of the disk, and is

$$Q = \frac{2\pi ab\, C}{n-1}.$$

Hence if σ_0 is the mean surface-density, the surface-density at any point is given by the equation

$$\sigma = \frac{n-1}{2}\sigma_0 p^{n-3}.$$

Thin rod.

The distribution on an infinitely thin rod is found by considering A_1PA_2 a rod of uniform section, which leads to the equation

$$\lambda_1 Q_1 P^{2-n} = \lambda_2 P Q_2^{2-n},$$

where λ is the linear density, and if the length of the rod is $2a$, and if x is the distance from the middle, and $x^2 = a^2(1 - p^2)$, the distribution of the linear density is given by

$$\lambda = Cp^{n-2}.$$

The charge of the whole rod is

$$Ca \frac{\Gamma\left(\frac{n}{2}\right)\Gamma\left(\frac{1}{2}\right)}{\Gamma\left(\frac{n+1}{2}\right)} = 2,$$

so that if λ_0 denotes the mean linear density,

$$\lambda = \lambda_0 \frac{2\Gamma\left(\frac{n+1}{2}\right)}{\Gamma\left(\frac{n}{2}\right)\Gamma\left(\frac{1}{2}\right)} p^{n-2},$$

when $n = 2$, $\lambda = \lambda_0$, or the density is uniform.

Since the fluid is in equilibrium in all these cases, the potential is uniform throughout the body. We may therefore determine the value of the potential at any point within the body by finding its value at any selected point, as for instance at the centre. If de be an element of the fluid, and r its distance from the given point, the corresponding element of the potential due to the force whose value is er^{-n} is $\frac{1}{n-1} er^{1-n}$.

We thus find for the potential of the sphere

$$V = C\, 2\pi a^{4-n} \frac{1}{n-1} \Gamma\left(\frac{n-2}{2}\right)\Gamma\left(\frac{4-n}{2}\right)$$

$$= Qa^{1-n} \frac{1}{n-1} \frac{\Gamma\left(\frac{n+1}{2}\right)\Gamma\left(\frac{4-n}{2}\right)}{\Gamma\left(\frac{3}{2}\right)}.$$

When n becomes equal to 4, V becomes infinite.

When n is equal to 2, $V = Qa^{-1}$.

For the plate bounded by parallel planes, V is infinite, except for values of n between 3 and 4, for which

$$V = \frac{2\pi\sigma_0}{(n-1)(n-3)} a^{3-n} \frac{\Gamma\left(\frac{n-1}{2}\right)\Gamma\left(\frac{4-n}{2}\right)}{\Gamma\left(\frac{1}{2}\right)},$$

where σ_0 is the quantity of fluid in unit of area of the plate.

For a circular disk

$$V = Qa^{1-n}\, \tfrac{1}{2}\, \Gamma\left(\frac{n-1}{2}\right)\Gamma\left(\frac{3-n}{2}\right),$$

in which n must be between 1 and 3.

When $n = 2$, $\qquad V = \frac{\pi}{2} Qa^{-1}$.

For an infinitely narrow rod

$$V = Qa^{1-n} \frac{\Gamma\left(\frac{n+1}{2}\right)\Gamma\left(\frac{2-n}{2}\right)}{(n-1)}.$$

Note 3, Art. 69.

On canals of incompressible fluid.

It appears from several passages (Arts. 40, 236, 273, 276, 278, 294, 348) that Cavendish considered that the weakest point in his theory was the assumption that the condition of electric equilibrium between two conductors connected by a fine wire is the same as if, instead of the wire, there were a canal of incompressible fluid defined as in Art. 69.

It is true that the properties of the electric fluid, as defined by Cavendish in Art. 3, are very different from those of an incompressible fluid. But it is easy to show that the results deduced by Cavendish from the hypothesis of a canal of incompressible fluid are applicable to the actual case in which the bodies are connected by a fine wire.

In what follows, when we speak of the electrified body or bodies, the canal or the wire is understood not to be included unless it is specially mentioned.

Cavendish supposes the canal to be everywhere exactly saturated with the electric fluid, and that the only external force acting on the fluid in the canal is that due to the electrification of the other bodies.

Since this resultant force is not in general zero at all points of the canal, the fluid in the canal cannot be in equilibrium unless it is prevented from moving by some other force. Now the condition of incompressibility excludes any such displacement of the fluid as would alter the quantity of fluid in a given volume, and the stress by which such a displacement is resisted is called isotropic (or hydrostatic) pressure. In a hypothetical case like this it is best, for the sake of continuity, to suppose that negative as well as positive values of the pressure are admissible.

In the electrified bodies themselves the properties of the fluid are those defined in Art. 3. The fluid is therefore incapable of sustaining pressure except when its particles are close packed together, and as it cannot sustain a negative pressure, the pressure must be zero in the electrified bodies, and therefore also in the canal at the points where it meets these bodies.

The condition of equilibrium of the fluid in the canal is

$$\rho \frac{dV}{ds} + \frac{dp}{ds} = 0,$$

where V denotes the potential of the electric forces due to the electrified bodies, ρ the density, and p the pressure of the fluid in the canal, and s the length of the canal reckoned from a fixed origin to the point under consideration.

Since by the hypothesis of incompressibility, ρ is constant,

$$\rho V + p = C,$$

where C is a constant; and if we distinguish by suffixes the symbols belonging to the two ends of the canal where it meets the bodies A_1 and A_2,

$$\rho V_1 + p_1 = \rho V_2 + p_2.$$

But we have seen that $p_1 = p_2 = 0$. Hence dividing by ρ we find for the condition of equilibrium

$$V_1 = V_2,$$

or the electric potential of the two bodies must be equal.

We arrive at precisely the same condition if we suppose the bodies connected by a fine wire which is made of a conducting substance.

Let V as before be the potential at any given point due to the electrified bodies, and let V_1 be its value in A_1, and V_2 its value in A_2, and let V' be the potential due to the electrification of the wire at the given point, then the condition of equilibrium of the electricity in the wire is that $V + V'$ must be constant for all points within the substance of the wire. Hence at the two ends of the wire

$$V_1 + V_1' = V_2 + V_2'.$$

Hence the actual potential due to the bodies and the wire together is the same in A_1 and A_2.

The only difference, then, between the actual case of the wire and the hypothetical case of the canal is that the surface of the wire is charged with electricity in such a way as to make its potential everywhere constant, whereas the canal is exactly saturated, and the effect of variation of potential is counteracted by variation of pressure.

Hence the canal produces no effect in altering the electrical state of the other bodies, whereas the wire acts like any other body charged with electricity.

The charge of the wire, however, may be diminished without limit by diminishing its diameter. It is approximately inversely proportional to the logarithm of the ratio of a certain length to the diameter of the wire. Hence by making the wire fine enough, the disturbance of the distribution of electricity on the bodies may be made as small as we please.

[Green on Cavendish's Theory.]

From the [beginning of the] Preface [1828] to Green's "Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism."

"After I had composed the following Essay, I naturally felt anxious to become acquainted with what had been effected by former writers on the same subject, and, had it been practicable, I should have been glad to have given, in this place, an historical sketch of its progress; my limited sources of information, however, will by no means permit me to do so; but probably I may here be allowed to make one or two observations on the few works which have fallen in my way, more particularly as an opportunity will thus offer itself, of noticing an excellent paper, presented to the Royal Society by one of the most illustrious members of that learned body, which appears to have attracted little attention, but which, on examination, will be found not unworthy the man who was able to lay the foundations of pneumatic chymistry, and to discover that water, far from being according to the opinions then received, an elementary substance, was a compound of two of the most important gases in nature.

"It is almost needless to say the author just alluded to is the celebrated CAVENDISH, who having confined himself to such simple methods as may readily be understood by any one possessed of an elementary knowledge of geometry and fluxions, has rendered his paper accessible to a great number of readers; and although, from subsequent remarks, he appears dissatisfied with an hypothesis which enabled him to draw some important conclusions, it will readily be perceived, on an attentive perusal of his paper, that a trifling alteration will suffice to render the whole perfectly legitimate.

"Little appears to have been effected in the mathematical theory of electricity, except immediate deductions from known formulae, that first presented themselves in researches on the figure of the earth, of which the principal are, —the determination of the law of the electric density on the surfaces of conducting bodies differing little from a sphere, and on those of ellipsoids, from 1771, the date of CAVENDISH's paper, until about 1812, when M. POISSON presented to the French Institute two memoirs of singular elegance, relative to the distribution of electricity on the surfaces of conducting spheres, previously electrified and put in presence of each other.

[Footnote.]—"In order to make this quite clear, let us select one of Cavendish's propositions, the twentieth for instance [Art. 71], and examine with some attention the method there employed. The object of this proposition is to show, that when two similar conducting bodies communicate by means of a long slender canal, and are charged with electricity, the respective quantities of redundant fluid contained in them will be proportional to the $n - 1$ power of their corresponding diameters; supposing the electric repulsion to vary inversely as the n power of the distance.

"This is proved by considering the canal as cylindrical, and filled with incompressible fluid of uniform density: then the quantities of electricity in the interior of the two bodies are determined by a very simple geometrical construction, so that the total action exerted on the whole canal by one of them shall exactly balance that arising from the other; and from some remarks in the 27$^{\text{th}}$ proposition [Arts. 94, 95] it appears the results thus obtained agree very well with experiments in which real canals are employed, whether they are straight or crooked, provided, as has since been shown by Coulomb, n is equal to two. The author, however, confesses he is by no means able to demonstrate this, although, as we shall see immediately, it may very easily be deduced from the propositions contained in this paper.

"For this purpose let us conceive an incompressible fluid of uniform density, whose particles do not act on each other, but which are subject to the same actions from all the electricity in their vicinity, as real electric fluid of like density would be; then supposing an infinitely thin canal of this hypothetical fluid, whose perpendicular sections are all equal and similar, to pass from a point a on the surface of one of the bodies through a portion of its mass, along the interior of the real canal, and through a part of the other body, so as to reach a point A on its surface, and then proceed from A to a in a right line, forming thus a closed circuit, it is evident from the principles of hydrostatics, and may be proved from our author's 23rd proposition [Art. 84], that the whole of the hypothetical canal will be in equilibrium, and as every particle of the portion contained within the system is necessarily so, the rectilinear portion aA must therefore be in equilibrium.

"This simple consideration serves to complete Cavendish's demonstration, whatever may be the form or thickness of the real canal, provided the quantity of electricity in it is very small compared with that contained in the bodies.

"An analogous application of it will render the demonstration of the 22nd proposition [Art. 74] complete, when the two coatings of the glass plate communicate with their respective conducting bodies by fine metallic wires of any form."

NOTE 4, ART. 83.

On the charges of two equal parallel disks, the distance between them being small compared with the radius.

The theory of two parallel disks, charged in any way, may be deduced from the consideration of two principal cases.

The first case is when the potentials of the two disks are equal. If the distance between the disks is very small compared with their diameter, we may consider the whole system as a single disk, the charge of which is approximately the same as if it were infinitely thin. Hence if V be the potential, and if we write A for the capacity of the first disk, and B for the coefficient of induction between the two disks, the charge of the first disk is

$$Q_1 = AV_1 - BV_2,$$

and that of the second is

$$Q_2 = AV_2 - BV_1.$$

If we make $$V_1 = V_2 = V,$$

$$Q_1 + Q_2 = 2(A - B)V.$$

Hence, by Note 2,

$$A - B = \frac{a^{n-1}}{\Gamma\left(\frac{n-1}{2}\right)\Gamma\left(\frac{3-n}{2}\right)}.$$

The second case is when the charges of the disks are equal and opposite. The surface-density in this case is approximately uniform except near the edges of the disks. I have not attempted to ascertain the amount of accumulation near the edge except when $n = 2$. If we suppose the density uniform, then for a charge of the first disk equal to πa^2, its potential, when b the distance between the disks is small compared with a the radius, will be approximately

$$V_1 = \frac{2\pi}{(n-1)(3-n)} b^{3-n}.$$

Hence, since $V_2 = -V_1$

$$A + B = \tfrac{1}{2}(n-1)(3-n)\,a^2 b^{n-3},$$

and we find

$$A = \tfrac{1}{4}(n-1)(3-n)\,a^2 b^{n-3} + \frac{a^{n-1}}{2\Gamma\left(\frac{n-1}{2}\right)\Gamma\left(\frac{3-n}{2}\right)},$$

$$B = \tfrac{1}{4}(n-1)(3-n)\,a^2 b^{n-3} - \frac{a^{n-1}}{2\Gamma\left(\frac{n-1}{2}\right)\Gamma\left(\frac{3-n}{2}\right)}.$$

When $n = 2$,

$$A = \frac{1}{4}\frac{a^2}{b} + \frac{a}{2\pi},$$

$$B = \frac{1}{4}\frac{a^2}{b} - \frac{a}{2\pi}.$$

In this case, however, we can carry the approximation further, for it is shown in Note 20 that

$$A - B = \frac{1}{\pi}\left(a + \frac{1}{2\pi}\,b\log\frac{a}{b}\right).$$

It is shown in "Electricity and Magnetism," Art. 202, that when two disks are charged to equal and opposite potentials, the density near the edge of each disk is greater than at a distance from it, and the whole charge is the same as if a strip of breadth $\frac{b}{2\pi}$ had been added all round the disk.

Hence

$$A + B = \frac{1}{2b}\left(a + \frac{b}{2\pi}\right)^2$$

$$= \frac{a^2}{2b} + \frac{a}{2\pi} + \frac{b}{8\pi^2},$$

and

$$A = \frac{a^2}{4b} + \frac{3}{4\pi}a + \frac{1}{4\pi^2}b\left(\log\frac{a}{b} + \frac{1}{4}\right),$$

$$B = \frac{a^2}{4b} - \frac{1}{4\pi}a - \frac{1}{4\pi^2}b\left(\log\frac{a}{b} - \frac{1}{4}\right).$$

NOTE 5, ART. 90.

[*On zero of Potential. See Note 7.*]

This proposition seems intended to justify those experimental methods in which the potential of the earth is assumed as the zero of potential.

Cavendish, by introducing the idea of degrees of electrification, as distinguished from the magnitudes of overcharge and undercharge, very nearly attained to the position of those who are in possession of the idea of potential*. But the very form of the phrases "positively or negatively electrified," which Cavendish uses, confers an importance on the limiting condition of "no electrification," which we hardly think of attributing to "zero potential." For we know that all electrical phenomena depend on differences of potential, and that the particular potential which we assume for our zero may be chosen arbitrarily, because it does not involve any physical consequences.

[* See vol. II of this Edition, Preface.]

It is true that the mathematicians define the zero of potential as the potential at an infinite distance from the finite system which includes the electric charges. This, however, is not a definition of which the experimentalist can avail himself, so he takes the potential of the earth as a zero accessible to all terrestrial electricians, and each electrician "makes his own earth."

The earth-connexion used by Cavendish is described in Art. 258. But when the whole apparatus of an electrical experiment is contained in a moderate space, such as a room, it is convenient to make an artificial "earth" by connecting by metal wires the case of the electrometer with all those parts of the apparatus which are intended to be at the same potential, and calling this potential zero.

It appears by observation, that in fine weather the electric potential at a point in the air increases with the distance from the earth's surface up to the greatest heights reached by observers, and in all parts of the earth. It is only when there are considerable disturbances in the atmosphere that the potential ever diminishes as the height increases. Hence the potential of the earth is probably always less than that of the highest strata of the atmosphere.

If the earth and its atmosphere together contain just as much electricity as will saturate them, and if there is no free electricity in the regions beyond, then the potential of the outer stratum of the atmosphere will be the same as that at an infinite distance, that is, it will be the zero of the mathematical theory, and the potential of the earth will be negative.

Note 6, Art. 97.

On the Molecular Constitution of Air.

The theory of Sir Isaac Newton here referred to is given in the ***Principia***, Lib. II, Prop. XXIII.

Newton supposes a constant quantity of air enclosed in a cubical vessel which is made to vary so as to become a cube of greater or smaller dimensions. Then since by Boyle's law the product of the pressure of the air on unit of surface into the volume of the cube is constant; and since the volume of the cube is the product of the area of a face into the edge perpendicular to it, it follows that the product of the total pressure on a face of the cube into the edge of the cube is constant, or the total pressure on a face is inversely as the edge of the cube.

Now if an imaginary plane be drawn through the cube parallel to one of its faces, the mutual pressure between the portions of air on opposite sides of this plane is equal to the pressure on a face of the cube. But the number of particles is the same, and their configuration is geometrically similar whether the cube is large or small. Hence the distance between any two given molecules must vary as the edge of the cube, and the force between the two molecules must vary as the total force between the sets of molecules separated by the imaginary plane, and therefore the product of the repulsion between two given molecules into the distance between them must be constant, in other words the repulsion varies inversely as the distance.

In this demonstration the repulsion considered is that between two *given* molecules, and it is shown that this must vary inversely as the distance between them in order to account for Boyle's law of the elasticity of air.

If, however, we suppose the same law of repulsion to hold for every pair of molecules, Newton shows in his Scholium that it would require a greater pressure to produce the same density in a larger mass of air.

We must therefore suppose that the repulsion exists, not between every pair of molecules, but only between each molecule and a certain definite number of other molecules, which we may suppose to be defined as those nearest to the given molecules. Newton gives as an example of such a kind of action the attraction of a magnet, the field of which is contracted when a plate of iron is interposed, so that the attractive power appears to be bounded by the nearest body attracted.

If the repulsion were confined to those molecules which are within a certain *distance* of each other, then, as Cavendish points out, the pressure arising from this repulsion would vary nearly as the square of the density, provided a large number of molecules are within this distance. Hence this hypothesis will not explain the fact that the pressure varies as the density.

On the other hand, if the repulsion were limited to particular pairs of particles, then since the particles are free to move, these pairs of particles would move away from each other till only those particles were near each other between which the repulsive force is supposed not to exist.

It would appear therefore that the hypothesis stated by Newton and adopted by Cavendish is the only admissible one, namely, that the repulsive force is inversely as the distance, but is exerted only between the nearest molecules.

Newton's own conclusion to his investigation of the properties of air on the statical molecular hypothesis is as follows:—"An vero Fluida Elastica ex particulis se mutuo fugantibus constent, Quæstio Physica est. Nos proprietatem Fluidorum ex ejusmodi particulis constantium mathematice demonstravimus, ut Philosophis ansam præbeamus Quæstionem illam tractandi."

The theory that the molecules of elastic fluids are in motion satisfies the conditions of the question as pointed out by Newton in a much more natural manner than any modification of the statical hypothesis.

According to the kinetic theory of gases, each molecule is in motion, and this motion is during the greater part of its course undisturbed by the action of other molecules, and is therefore uniform and in a straight line. When however it comes very near another molecule, the two molecules act on each other for a very short time, the courses of both are changed and they go on in the new courses till they encounter other molecules.

It would appear from the observed properties of gases that the mutual action between two molecules is insensible at all sensible distances. As the molecules approach, the action is at first attractive, but soon changes to a repulsive force of far greater magnitude, so that the general character of the encounter depends mainly on the repulsive force.

On this theory, the elasticity of the gas may still be said in a certain sense to arise from the repulsive force between its molecules, only instead of this repulsive force being in constant action, it is called into play only during the encounters between two molecules. The intensity of the impulse is not the same for all encounters, but as it does not depend on the interval between the encounters, we may consider its mean value as constant. The average value of the force between two molecules is in this case the value of the impulse divided by the time between two encounters. Hence we may say that the force is inversely as the distance between the molecules, and that it acts between those molecules only which encounter each other. . . .

For an earlier investigation by Cavendish of the properties of an elastic fluid, see Note 18.

Note 7, Art. 101.

[*On the idea of Electric Potential, as introduced by Cavendish: see p.* 18.]

Here Cavendish endeavours to fix a precise meaning to the terms "positively and negatively electrified," terms which he found current among electricians, but not well defined. The meaning which he here fixes to them, and which he afterwards makes much use of, is equivalent to the meaning of the modern term potential, as used by practical electricians. The idea of potential as used by mathematicians is expressed by Cavendish in his theory of canals of incompressible fluid.

In the "Thoughts concerning Electricity," and in the unpublished papers, degrees of electrification are spoken of. These degrees of electrification are measured in the experimental researches by means of electrometers of different kinds, and since he has compared the indications of his electrometers with the degrees of electrification required to make a spark pass between the balls of Lane's discharging electrometer, we may express all these measurements in modern units, though Cavendish's original electrometers no longer exist.

I have not been able to trace the idea of electric potential in the work of Æpinus, so that Cavendish seems the first to have made use of it. The relation between the charge of a body and the degree of its electrification is the main object of Cavendish's experimental researches, and the results of his work were expressed in the material form of a collection of coated plates, each of which had a capacity equal to that of a sphere of known diameter.

The leading idea in the great experimental work of Coulomb seems to be the measurement of the charges of the different bodies of a system and of parts of these bodies. Perhaps the most valuable of Coulomb's many contributions to experimental physics was the measurement of the surface-density of the distribution of electricity on a conductor on different parts of its surface by means of the proof plane. The numerical results obtained by Coulomb led directly to the great mathematical work of Poisson. I have not been able, however, to trace, even in those parts of Coulomb's papers where it would greatly facilitate the exposition, any idéa of potential as a quantity which has the same value for all parts of a system of conductors communicating with each other.

NOTE 8 [ART. 118].

Cases of Attraction and Repulsion.

The statements of Cavendish may be illustrated by the case of two spheres A and B, whose radii are a and b, and the distance between their centres c.

If the charge of A is 1, and that of B is 0, the attraction is

$$2\frac{b^3}{c^5} + 3\frac{b^5}{c^7} + 4\frac{b^7}{c^9} + 5\frac{b^9 + 4b^6a^3}{c^{11}} + \&c.,$$

an expression which shows that it depends chiefly on the value of b, the radius of the sphere without charge.

If the sphere B, instead of being without charge, is at potential zero, that is, if it is not insulated, the attraction is

$$\frac{b}{c^3} + 2\frac{b^3}{c^5} + 3\frac{a^3b^2 + b^5}{c^7} + \&c.$$

This expression exceeds the former by

$$\frac{b}{c^3} + 3\frac{a^3b^2}{c^7} + \&c.$$

The number of times that the attraction of an uninsulated sphere exceeds that of a sphere without charge is therefore approximately

$$2\frac{c^2}{b^2},$$

which is greater as the sphere is smaller. This agrees with what Cavendish says in Art. 108.

With respect to two bodies at the same potential, Cavendish remarks in Art. 113, that it may be said that one of them may be rendered undercharged in the part nearest to the other, and he shows that even in this case, the two bodies must repel each other. But it may be shown that each of the bodies must be overcharged in every part of its surface. For in the first place no part can be undercharged, for the lines of force which terminate in an undercharged surface must have come from an overcharged surface at which the potential is higher than at the surface. But there is no body in the field at a higher potential than the two bodies considered. Hence no part of their surface can be undercharged.

Nor can any finite part of the surface be free from charge, for it may be shown that if a finite portion of the surface of a conductor is free from charge, every point which can be reached by continuous motion from that part of the surface without passing through an electrified surface must be at the same potential. Hence no finite portion of a surface can be free from charge, unless the whole surface is free from charge.

NOTE 9, ART. 124.

[*On electric leakage.*]

The rate at which electricity passes from a conductor to the surrounding air or from the surrounding air to a conductor was believed to be much greater by Cavendish and his contemporaries than is consistent with modern experiments. Judging from the statements of the electricians of each generation, it would seem as if this rate had been diminishing steadily during the last hundred years in exact correspondence with the improvements which have been made in the construction of solid insulating supports for electrified conductors.

Whenever the intensity of the electromotive force at the surface of a conductor is sufficiently great, the air no doubt becomes charged*. This is the case at a sharp point connected with the conductor even when the potential is low, but when the curvature of the surface is continuous and gentle, the conductor must be raised to a high potential before any discharge to air begins to take place.

Thus in Thomson's portable electrometer, in which there are two disks placed parallel to each other at different potentials, the percentage loss of electricity from day to day is very small, and seems to depend principally on the solid insulators, for when the disks are placed very near each other, less loss is observed than when they are further apart, though the intensity of the force urging the electricity through the intervening stratum of air is greater the nearer the disks are to each other.

On the surface density of electricity near the vertex of a cone.

Green has given in a note to his Essay, section (12), the following results of an investigation which, so far as I am aware, he never published†.

"Since this was written, I have obtained formulae serving to express, generally, the law of the distribution of the electric fluid near the apex O of a cone, which forms part of a conducting surface of revolution having the same axis. From these formulae it results that, when the apex of the cone is directed inwards, the density of the fluid at any point p, near to it, is proportional

* M. R. Nahrwold (Wiedemann's *Annalen* v. (1878), p. 440) finds that the discharge from a sharp point communicates a charge to dusty air which can be detected in the air for some time afterwards. This does not occur in air free from dust. But the discharge from an incandescent platinum wire communicates a lasting charge even to air free from dust. [Cf. the modern knowledge of leakage as depending on the migrations of free electrons.]

† [The results of Green were at length demonstrated by Prof. H. M. Macdonald, *Cambridge Phil. Trans.* vol. XVIII, 1900 (Stokes Memorial Volume), pp. 292–8.]

to r^{n-1}; r being the distance Op, and the exponent n very nearly such as would satisfy the simple equation

$$(4n + 2)\,\beta = 3\pi,$$

where 2β is the angle at the summit of the cone.

If 2β exceeds π, this summit is directed outwards, and when the excess is not very considerable, n will be given as above: but 2β still increasing, until it becomes $2\pi - 2\gamma$, the angle 2γ at the summit of the cone, which is now directed outwards, being very small, n will be given by

$$2n \log \frac{2}{\gamma} = 1,$$

and in case the conducting body is a sphere whose radius is b, on which P represents the mean density of the electric fluid; ρ, the value of the density near the apex O, will be determined by the formula

$$\rho = \frac{2Pbn}{(a + b)\,\gamma} \left(\frac{r}{a}\right)^{n-1},$$

a being the length of the cone."

Professor F. G. Mehler* of Elbing has investigated the distribution of electricity on a cone under the influence of a charged point on the axis, and the inverse problem of the distribution on a spindle formed by the revolution of the segment of a circle about its chord.

He finds that when the segment is a very small portion of the circle, so that the conical points of the spindle are very acute, the surface-density at any point is inversely proportional to the product of the distances of that point from the two conical points.

NOTE 10 [ART. 139].

[*Conditions for Disruptive Discharge*†.]

Sir W. Thomson‡ has determined in absolute measure the electromotive force required to produce a spark in air between two electrodes in the form of disks, one of which was plane, and the other slightly convex, placed at different distances from each other. Mr Macfarlane§ has recently made a more extensive series of experiments on the disruptive discharge of electricity. He finds that in air at the ordinary pressure and temperature the electromotive force required to produce a spark between disks, 10 cm. diameter, and from 1 to 0·025 cm. apart, is expressed by the empirical equation

$$V = 66{\cdot}940\,(s^2 + {\cdot}20503s)^{\frac{1}{2}},$$

where s is the distance between the disks.

If we suppose that in the space between the disks the potential varies

* Ueber eine mit den Kugel- und Cylinderfunctionen verwandte Function, und ihre Anwendung in der Theorie der Electricitätsvertheilung. (Elbing, 1870.)

† [See for modern knowledge Sir J. J. Thomson, *Conduction...in Gases*.]

‡ *Proc. R. S.* 1860, or *Papers on Electrostatics*, chap. XIX.

§ *Trans. R. S. Edin.* vol. XXVIII, Part II (1878), p. 633.

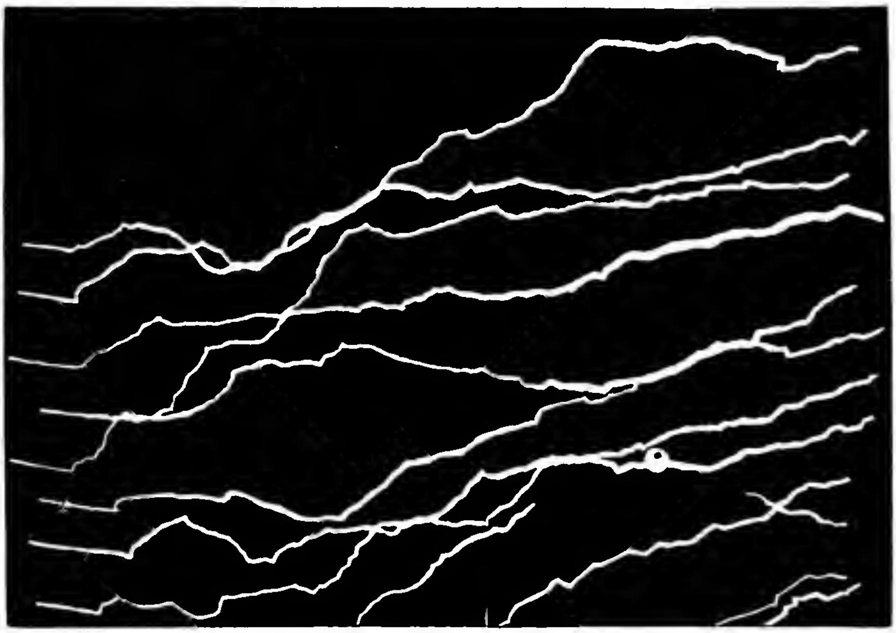
uniformly, as it does between two infinite planes, then the resultant electromotive intensity is $R = \dfrac{V}{s}$.

If, on the other hand, we suppose that the variation of the potential near the surface of the disks is affected by unknown causes, we would get a better estimate of the intensity by taking

$$R = \frac{dV}{ds}.$$

Both $\dfrac{V}{s}$ and $\dfrac{dV}{ds}$ diminish as the distance increases, approximating to the limit 66·940.

This corresponds to a surface-density of 5·327 units of electricity per square centimetre, and to a tension of 178·3 dynes per square centimetre. As the ordinary pressure of the atmosphere is about a million dynes per square centimetre, the pressure with which the electricity tends to break through the air is only about $\dfrac{1}{5600}$ of the pressure of the atmosphere.



If the electrodes are convex surfaces, whose radii of curvature, a and b, are large compared with the least distance c between the surfaces, then if

$$\frac{1}{s} = \frac{1}{c} + \frac{2}{3a} - \frac{1}{3b},$$

the greatest electric force at the surface whose radius is a will be equal to that at either of two parallel plane surfaces at the same potentials whose distance is s.

Hence the electromotive force required to produce a spark between convex surfaces, as in Lane's electrometer, is less than if the surfaces had been plane and at the same distance.

When the air-space is large, the path of the sparks, and therefore the electromotive force required to produce them, is exceedingly irregular. The accompanying figure is from a photograph of a succession of sparks taken between the same electrodes from four Leyden jars charged by Holtz's machine.

A portion of the path near the positive electrode is nearly straight, there is then a sharp turn, which, in all the sparks represented, is in the same direction. Beyond this the course of the spark is very irregular, although its general direction is deflected towards the same side as the first sharp turn.

NOTE 11, ART. 141.

Theory of two circular disks on the same axis, their radii being small compared with the distance between them.

A circular disk may be considered as an ellipsoid, two of whose axes are equal, while the third is zero, and we may apply the method of ellipsoidal co-ordinates to the calculation of the potential*. In the case before us everything is symmetrical about the axis, so that we have to consider only the zonal harmonics, and of these only those of even order, unless we wish to distinguish between the surface-density on opposite sides of the same element of the disk, for this depends on the harmonics of odd orders.

Let a be the radius of the first disk, b that of the second, and c the distance between them.

We shall use ellipsoidal co-ordinates confocal with the first disk. Let r_1 and r_2 be the greatest and least distances respectively of a given point from the edge of the disk, and let

$$a^2 - \tfrac{1}{4}(r_1 - r_2)^2 = a^2\mu^2, \qquad \text{......(1)}$$

$$\tfrac{1}{4}(r_1 + r_2)^2 - a^2 = a^2\nu^2, \qquad \text{......(2)}$$

then if z is the distance of the point from the plane of the disk, and r its distance from the axis,

$$z = a\mu\nu, \qquad \text{......(3)}$$

$$r^2 = a^2(1 - \mu^2)(\nu^2 - 1). \qquad \text{......(4)}$$

If the surface-density of the electricity on the disk is a function of the distance from the axis, it may be expressed in the form

$$\sigma = \sigma_0 + \sigma_2 + \text{\&c.}, \qquad \text{......(5)}$$

where

$$\sigma_n = \frac{1}{2\pi a^2\mu} A_{2n} P_{2n}(\mu), \qquad \text{......(6)}$$

and P_{2n} is the zonal harmonic of order $2n$. Only even orders are admissible, for since every element of the disk corresponds to two values of μ, numerically

* See Ferrers' *Spherical Harmonics*, p. 135.

equal but of opposite signs, a term involving an harmonic of odd order would give the surface-density everywhere zero.

The potential arising from this distribution at any point whose ellipsoidal co-ordinates are $\omega = a\mu$ and $\eta = a\nu$

is $$V = V_0 + V_2 + \&c. + V_n, \qquad \ldots\ldots(7)$$

where $$V_{2n} = A_{2n} \frac{1}{a} \frac{2n!}{2^{2n} n!\, n!} P_{2n}(\mu)\, Q'_{2n}(\nu). \qquad \ldots\ldots(8)$$

In this expression $Q'_{2n}(\nu)$ denotes a series, the terms of which are numerically equal to those of $Q_{2n}(\nu)$, the zonal harmonic of the second kind, but with the second and all even terms negative. If we put i for $\sqrt{-1}$, we may write

$$Q'_{2n}(\nu) = (-)^n\, iQ_n(i\nu) \qquad \ldots\ldots(9)$$

$$= \frac{1 \cdot 2 \cdot 3\; 2n}{1 \cdot 3 \cdot 5\; 4n+1} \nu^{-(2n+1)} - \frac{3 \cdot 4 \cdot 5 \ldots (2n+2)}{3 \cdot 5 \cdot 7 \ldots (4n+3)} \nu^{-(2n+3)} + \&c. \quad \ldots(10)$$

This expression is an infinite series, the terms of which increase without limit when ν is diminished without limit.

It may, however, be expressed in the finite form*

$$Q'_{2n}(\nu) = P'_{2n}(\nu) \tan^{-1}\left(\frac{1}{\nu}\right) - Z_{2n}(\nu), \qquad \ldots\ldots(11)$$

where $$P'_{2n}\nu = (-)^n P_{2n}(i\nu), \qquad \ldots\ldots(12)$$

that is to say $P'_{2n}(\nu)$ is a zonal harmonic of the first kind with all its terms positive, and $Z_{2n}(\nu)$ is a rational and integral function of ν of $2n-1$ degrees, which is such as to cancel all the terms of $P'_{2n}(\nu)\tan^{-1}\left(\frac{1}{\nu}\right)$ which do not vanish when ν becomes infinite.

The expression (11) is applicable to small as well as great values of ν. Thus we find when ν is 0, as it is at the surface of the disk,

$$Q'_{2n}(0) = \frac{2n!}{2^{2n} n!\, n!} \frac{\pi}{2}. \qquad \ldots\ldots(13)$$

The potential at any point of the disk is therefore the sum of a series of terms, the general form of which is

$$V_{2n} = A_{2n} \frac{\pi}{2a} \frac{(2n!)^2}{2^{4n}(n!)^4} P_{2n}(\mu). \qquad \ldots\ldots(14)$$

On the axis, $\mu = 1$ and $a\nu = z$, and the potential is the sum of a series of terms, the general form of which is

$$U_{2n} = A_{2n} \frac{1}{a} \frac{2n!}{2^{2n} n!\, n!} Q'_{2n}(\nu). \qquad \ldots\ldots(15)$$

Since we have to determine the value of the potential arising from the first disk at a point in the second disk for which $z = c$ at a distance r from the axis, and if we write $$r^2 = b^2(1 - p^2), \qquad \ldots\ldots(16)$$

* See Heine, *Handbuch der Kugelfunctionen*, § 28, 20.

where b is the radius of the second disk, and p is a quantity corresponding to μ in the first disk, then the most convenient expression for the potential due to the first disk at a point (p) in the second, is

$$V = U - \frac{1}{2^2}\frac{b^2}{a^2}\frac{d^2U}{dv^2}(1 - p^2) + \frac{1}{2^2 \cdot 4^2}\frac{b^4}{a^4}\frac{d^4U}{dv^4}(1 - p^2)^2 - \&c., \quad \text{......(17)}$$

where U denotes the value of the potential at the axis, and where, after the differentiations, va is to be made equal to c.

To investigate the mutual action of the two disks, let us assume that the surface-density on the second disk is the sum of a number of terms of which the general form is

$$\frac{1}{2\pi b^2} B_{2n} \frac{1}{p} P_{2n}(p). \quad \text{......(18)}$$

The potential at the surface of the second disk arising from this distribution will be the sum of a series of terms of the form

$$\frac{\pi}{2}\frac{1}{b}\frac{(2n!)^2}{2^{4n}(n!)^4} B_{2n} P_{2n}(p). \quad \text{......(19)}$$

The potential arising from the presence of the first disk is given in equation (17).

Having thus expressed the most general symmetrical distribution of electricity on the two disks and the potentials thence arising, we are able to calculate the potential energy of the system in terms of the squares and products of the two sets of coefficients A and B.

If W denotes the potential energy,

$$W = \frac{1}{2}\iint \sigma V ds, \quad \text{......(20)}$$

when the integration is to be extended over every element of surface ds.

Confining our attention to the second disk, the part of W thence arising is

$$\pi b^2 \int_0^1 \sigma V p\, dp, \quad \text{......(21)}$$

and the part arising from the term in the density whose coefficient is B_{2n} is

$$\tfrac{1}{2} B_{2n} \int_0^1 V P_{2n}(p)\, dp. \quad \text{......(22)}$$

The part of the value of V which arises from the electricity on the second disk itself is the sum of a series of terms of the form (19). The surface-integral of the product of any two of these of different orders is zero, so that in finding the potential energy of the disk on itself we have to deal only with terms of the form

$$B^2_{2n} \frac{\pi}{4}\frac{1}{b}\frac{(2n!)^2}{2^{4n}(n!)^4}\frac{1}{4n+1}. \quad \text{......(23)}$$

The energy arising from the mutual action of the disks consists of terms whose coefficients are products of A's and B's, and in calculating these we meet with the integral*

$$\int_0^1 (1-p^2)^m P_{2n}(p)\,dp = (-1)^n \frac{2^{2m} \,.\, \overline{m+n}\,!\, m\,!\, m\,!\, 2n\,!}{\overline{2m+2n+1}\,!\,\overline{m-n}\,!\, n\,!\, n\,!}. \quad \ldots(24)$$

We have, therefore, for the harmonic of order zero.—

Surface-density on the first disk, $\sigma_0 = \frac{A}{2\pi a^2}\frac{1}{\mu}$, where A is the charge of the first disk.

Potential at the surface of the first disk $V_0 = \frac{\pi}{2}\frac{1}{a} A$.

Potential at the surface of the second disk, arising from this distribution of electricity on the first,

$$V_0 = A\frac{1}{c}\left[1 - \frac{1}{3}\frac{a^2}{c^2} + \frac{1}{5}\frac{a^4}{c^4} - \frac{1}{7}\frac{a^6}{c^6} + \&c.\right]$$
$$- A\frac{b^2(1-p^2)}{2^2c^3}\left[2 - 4\frac{a^2}{c^2} + 6\frac{a^4}{c^4} - \&c.\right]$$
$$+ A\frac{b^4(1-p^2)^2}{2^2\,.\,4^2\,.\,c^5}\left[2\,.\,3\,.\,4 - 4\,.\,5\,.\,6\frac{a^2}{c^2} + \&c.\right]$$
$$- \&c.$$

Order 2.— $$\sigma_2 = A_2\frac{1}{2\pi a^2\mu}\left(\tfrac{3}{2}\mu^2 - \tfrac{1}{2}\right).$$

Potential at the surface of first disk,

$$V_2 = \frac{\pi}{2}\frac{1}{a}A_2\frac{1}{2^2}\left(\tfrac{3}{2}\mu^2 - \tfrac{1}{2}\right).$$

Potential at the surface of the second disk,

$$V_2 = A_2\frac{1}{2}\frac{a^2}{c^3}\left[\frac{2}{3\,.\,5} - \frac{4}{5\,.\,7}\frac{a^2}{c^2} + \frac{6}{7\,.\,9}\frac{a^4}{c^4} - \&c.\right]$$
$$- A_2\frac{1}{2}\frac{a^2b^2(1-p^2)}{2^2\,.\,c^5}\left[\frac{2\,.\,4}{5} - \frac{4\,.\,6}{7}\frac{a^2}{c^2} + \&c.\right]$$
$$+ A_2\frac{1}{2}\frac{a^2b^4(1-p^2)}{2^2\,.\,4^2\,.\,c^7}\left[2\,.\,4\,.\,6 - \&c.\right].$$

Order 4.— $$\sigma_4 = A_4\frac{1}{2\pi a^2\mu}\left[\frac{35}{8}\mu^4 - \frac{30}{8}\mu^2 + \frac{3}{8}\right].$$

Potential at first disk,

$$V_4 = \frac{\pi}{2a}A_4\frac{1^2\,.\,3^2}{2^2\,.\,4^2}\left[\frac{35}{8}\mu^4 - \frac{30}{8}\mu^2 + \frac{3}{8}\right].$$

Potential at second disk,

$$V_4 = A_4\frac{1\,.\,3}{2\,.\,4}\frac{a^4}{c^5}\left[\frac{2\,.\,4}{5\,.\,7\,.\,9} - \frac{4\,.\,6}{7\,.\,9\,.\,11}\frac{a^2}{c^2} + \&c.\right]$$
$$- A\frac{1\,.\,3}{2\,.\,4}\frac{a^4b^2(1-p^2)}{2^2c^7}\left[\frac{2\,.\,4\,.\,6}{7\,.\,9} - \&c.\right],$$

and so on.

* I am indebted for the general value of this integral to Mr W. D. Niven, of Trinity College. [See Sir W. Niven's memoir in *Phil. Trans.* 1879.]

We have next to calculate the energy arising from this distribution on the first disk, together with a corresponding distribution on the second disk, the coefficients of the harmonics for the second disk being B, B_2, B_4, &c.

It will consist of three parts, the potential energy of the first disk on itself, of the first and second on each other, and of the second on itself.

The first part will involve only terms having for coefficients the squares of the coefficients A, for those involving products of harmonics of different orders will vanish on integration.

The third part will, for the same reason, involve only squares of the coefficients B.

The second part will involve all products of the form AB.

Performing the integrations, putting $a = cx$ and $b = cy$,

$$
\begin{aligned}
W = {} & A^2 \frac{1}{a}\frac{\pi}{4} + A_2^2 \frac{1}{a}\frac{\pi}{4}\frac{1}{2^2 . 5} + A_4^2 \frac{1}{a}\frac{\pi}{4}\frac{1}{2^6} \\
& + B^2 \frac{1}{b}\frac{\pi}{4} + B_2^2 \frac{1}{b}\frac{\pi}{4}\frac{1}{2^2 . 5} + B_4^2 \frac{1}{b}\frac{\pi}{4}\frac{1}{2^6} \\
& + AB\frac{1}{c}\left[1 - \tfrac{1}{3}(x^2+y^2) + \tfrac{1}{5}(x^4+y^4) + \tfrac{2}{3}x^2y^2 - \tfrac{1}{7}(x^6+y^6) - x^2y^2(x^2+y^2)\right. \\
& \qquad \left. + \tfrac{1}{9}(x^8+y^8) + \tfrac{4}{3}x^2y^2(x^4+y^4) + \tfrac{14}{5}x^4y^4 - \&c.\right] \\
& + AB_2\frac{y^2}{c}\frac{1}{3\cdot 5}\left[1 - 2x^2 - \frac{2\cdot 3}{7}y^2 + 3x^4 + \frac{2\cdot 3\cdot 5}{7}x^2y^2 + \frac{5}{7}y^4\right. \\
& \qquad \left. - 4x^6 - 3\cdot 4x^4y^2 - \frac{4\cdot 5}{3}x^2y^4 - \frac{4\cdot 5}{3\cdot 11}y^4 + \&c.\right] \\
& + AB_4\frac{y^4}{c}\frac{1}{3\cdot 5\cdot 7}\left[1 - 5x^2 - \frac{3\cdot 5}{11}y^2 + 2.7x^4 + \frac{4\cdot 5\cdot 7}{11}x^2y^2 + \frac{2\cdot 3\cdot 5\cdot 7}{11 . 13}y^4 - \&c.\right] \\
& + A_2B\frac{x^2}{c}\frac{1}{3\cdot 5}\left[1 - \frac{2\cdot 3}{7}x^2 - 2y^2 + \frac{5}{7}x^4 + \frac{2\cdot 3\cdot 5}{7}x^2y^2 + 3y^4\right. \\
& \qquad \left. - \frac{4\cdot 5}{3\cdot 11}x^6 - \frac{4\cdot 5}{3}x^4y^2 - 4\cdot 3x^2y^4 + 4y^6 - \&c.\right] \\
& + A_2B_2\frac{x^2y^2}{c}\frac{2}{3\cdot 5^2}\left[1 - \frac{3\cdot 5}{7}(x^2+y^2) + \frac{2\cdot 5}{3}(x^4+y^4) + \frac{3\cdot 4\cdot 5}{7}x^2y^2 - \&c.\right] \\
& + A_2B_4\frac{x^2y^4}{c}\frac{1}{3\cdot 5\cdot 7}\left[1 - 4x^2 - \frac{4\cdot 7}{11}y^2 + \&c.\right] \\
& + A_4B\frac{x^4}{c}\frac{1}{3\cdot 5\cdot 7}\left[1 - \frac{3\cdot 5}{11}x^2 - 5y^2 + \frac{2\cdot 3\cdot 5\cdot 7}{11 . 13}x^4 + \frac{4\cdot 5\cdot 7}{11}x^2y^2 + 2.7y^4 - \&c.\right] \\
& + A_4B_2\frac{x^4y^2}{c}\frac{1}{3\cdot 5\cdot 7}\left[1 - \frac{4\cdot 7}{11}x^2 - 4y^2 + \&c.\right] \\
& + A_4B_4\frac{x^4y^4}{c}\frac{1}{5\cdot 7}.
\end{aligned}
$$

In this expression for the energy of the system the coefficients A_2, A_4, B_2, B_4 are treated as independent of A and B. To determine the nearest approach to equilibrium which can be obtained from a distribution defined by this limited number of harmonics, we must make W a minimum with respect to A_2, B_2, A_4 and B_4.

We thus find for the values of these coefficients

$$A_2 = -B\frac{2}{\pi}\frac{2^2}{3}x^3\left[1 - \frac{2\cdot 3}{7}x^2 - 2y^2 + \tfrac{5}{7}x^4 + \frac{2\cdot 3\cdot 5}{7}x^2y^2 + 3y^4 - \&c.\right]$$

$$+ A\frac{4}{\pi^2}\frac{2^5}{3^2\cdot 5}x^3y^5\,[1 - \&c.].$$

$$B_2 = -A\frac{2}{\pi}\frac{2^2}{3}y^3\left[1 - 2x^2 - \frac{2\cdot 3}{7}y^2 + 3x^4 + \frac{2\cdot 3\cdot 5}{7}x^2y^2 + \tfrac{5}{7}y^4 - \&c.\right]$$

$$+ B\frac{4}{\pi^2}\frac{2^5}{3^2\cdot 5}x^5y^3\,[1 - \&c.].$$

$$A_4 = -B\frac{2}{\pi}\frac{2^6}{3\cdot 5\cdot 7}x^5\left[1 - \frac{3\cdot 5}{11}x^2 - 5y^2 + \&c.\right] + A\frac{4}{\pi^2}\frac{2^8}{3^2\cdot 5\cdot 7}x^5y^5.$$

$$B_4 = -A\frac{2}{\pi}\frac{2^6}{3\cdot 5\cdot 7}y^5\left[1 - 5x^2 - \frac{3\cdot 5}{11}y^2 + \&c.\right] + B\frac{4}{\pi^2}\frac{2^8}{3^2\cdot 5\cdot 7}x^5y^5.$$

We are now able to express the energy in the form

$$W = \tfrac{1}{2}p_{11}A^2 + p_{12}AB + \tfrac{1}{2}p_{22}B^2,$$

where A and B are the charges, and p_{11}, p_{12}, and p_{22} are the coefficients of potential, the values of which we now find to be

$$p_{11} = \frac{\pi}{2a} - \frac{1}{\pi}\frac{2^3}{3^2\cdot 5}\frac{b^5}{c^6}\left[1 - 4\frac{a^2}{c^2} - \frac{12}{7}\frac{b^2}{c^2} + 10\frac{a^4}{c^4} + 12\frac{a^2b^2}{c^4} + \frac{2\cdot 3\cdot 13}{5\cdot 7}\frac{b^4}{c^4} - \&c.\right].$$

$$p_{12} = \frac{1}{c} - \frac{1}{3}\frac{a^2+b^2}{c^3} + \frac{1}{5}\frac{a^4+b^4}{c^5} + \frac{2}{3}\frac{a^2b^2}{c^5} - \frac{1}{7}\frac{a^6+b^6}{c^7} - \frac{a^2b^2(a^2+b^2)}{c^7} + \&c.$$

$$p_{22} = \frac{\pi}{2b} - \frac{1}{\pi}\frac{2^3}{3^2\cdot 5}\frac{a^5}{c^6}\left[1 - \frac{12}{7}\frac{a^2}{c^2} - 4\frac{b^2}{c^2} + \frac{2\cdot 3\cdot 13}{5\cdot 7}\frac{a^4}{c^4} + 12\frac{a^2b^2}{c^4} + 10\frac{b^4}{c^4} - \&c.\right].$$

NOTE 12, ART. 151.

On the electrical capacity of a long narrow cylinder.

The problem of the distribution of electricity on a finite cylinder is still, so far as I know, in the state in which it was left by Cavendish. It is sometimes assumed that the electric properties of a long narrow cylinder may be represented, to a sufficient degree of approximation, by those of the ellipsoid inscribed in the cylinder. The electrical capacity of the cylinder must be greater than that of the ellipsoid, because the electric capacity of any figure is greater than that of any part of that figure.

It is easier to state the conditions of the problem than to obtain an exact solution.

Let $2l$ be the length of the cylinder, and let b be its radius.

Let the axis of the cylinder be the axis of x, and let the origin be taken at the middle point of the axis. Let y be the distance of any point from the axis.

Let λdx be the quantity of electricity on that part of the curved surface of the cylinder for which x is between x and $x + dx$; we may call λ the linear density of the electricity on the cylinder.

Let σ be the surface-density on the flat ends.

Let ψ be the potential at a point on the axis for which $x = \xi$.

$$\psi = \int_{-l}^{+l} \lambda \left[(\xi - x)^2 + b^2\right]^{-\frac{1}{2}} dx + \int_0^b 2\pi\sigma y \left[(l - \xi)^2 + y^2\right]^{-\frac{1}{2}} dy$$
$$+ \int_0^b 2\pi\sigma y \left[(l + \xi)^2 + y^2\right]^{-\frac{1}{2}} dy, \qquad \ldots\ldots(1)$$

the first integral representing the part of the potential due to the curved surface, and the other two the parts due to the positive and the negative flat ends respectively.

The condition of equilibrium of the electricity is that ψ must be constant for all points within the cylinder, and therefore for all points on the axis between the two ends.

If, by giving proper values to λ and σ, we can make the value of ψ constant for any finite length along the axis, then, by Art. 144 of "Electricity and Magnetism," ψ will be constant for all points within the surface of the cylinder.

It was shown in Note 2 that the distribution of electricity in equilibrium on a straight line without breadth is a uniform one. We may expect, therefore, that the distribution on a cylinder will approximate to uniformity as the radius of the cylinder diminishes.

If we suppose λ and σ to be each of them constant,

$$\psi = \lambda \log \frac{(f_1 + l - \xi)(f_2 + l + \xi)}{b_2} + 2\pi\sigma (f_1 + f_2 - 2l), \qquad \ldots\ldots(2)$$

where f_1 and f_2 are the distances of the point (ξ) on the axis from the edges of the curved surface at the + and − ends of the cylinder respectively.

Just within the positive flat end of the cylinder, where ξ is just less than l,

$$\frac{d\psi}{d\xi} = -\lambda\left(\frac{1}{b} - \frac{1}{f_2}\right) + 2\pi\sigma. \qquad \ldots\ldots(3)$$

If the electricity were in equilibrium, this would be zero, and if the cylinder is so long that we may neglect the reciprocal of f_2, we find

$$\lambda = 2\pi b\sigma, \qquad \ldots\ldots(4)$$

or the surface-density on the end must be equal to the surface-density on the curved surface.

The whole charge is therefore

$$E = \lambda (2l + b). \qquad \ldots\ldots(5)$$

The greatest value of the potential is at the middle of the axis, where $\xi = 0$. Calling it $\psi_{(0)}$ and putting $f = l$,

$$\psi_{(0)} = \lambda\left(2 \log \frac{2l}{b} + \frac{b}{l}\right). \qquad \ldots\ldots(6)$$

The potential at the end of the axis is

$$\psi_{(l)} = \lambda\left(\log \frac{4l}{b} + 1\right). \qquad \ldots\ldots(7)$$

The potential at the curved edge is approximately

$$\psi_{(e)} = \lambda\left(\log\frac{4l}{b} + \frac{2}{\pi}\right). \qquad \text{......(8)}$$

This is the smallest value of the potential for any point of the cylinder.

The capacity of the cylinder cannot therefore be less than

$$\frac{E}{\psi_{(0)}} = \frac{2l+b}{2\log\frac{2l}{b} + \frac{b}{l}}, \qquad \text{......(9)}$$

nor greater than

$$\frac{E}{\psi_{(e)}} = \frac{2l+b}{\log\frac{4l}{b} + \frac{2}{\pi}}. \qquad \text{......(10)}$$

Cavendish does not take into account the flat ends of the cylinder, but in other respects these limits are the same as those between which he shows that the capacity must lie. The approximation, however, is by no means a close one, for when the cylinder is very narrow the upper limit is nearly double the lower. Indeed Cavendish, in Arts. 479, 682, has recourse to experiment to determine the best form of the logarithmic expression.

We may obtain a much closer approximation by the following method, which is applicable to many cases in which we cannot obtain a complete solution.

Let W be the potential energy of any arbitrary distribution of electricity on a body of any form

$$W = \tfrac{1}{2}\Sigma\,(e\psi), \qquad \text{......(11)}$$

where e is the charge of any element of the body, and ψ the potential at that element.

The charge is

$$E = \Sigma\,(e). \qquad \text{......(12)}$$

Let us now suppose the electricity to become moveable and to distribute itself so as to be in equilibrium. The potential will then be uniform. Let its value be ψ_0, and since the charge remains the same the potential energy of the electrification in the state of equilibrium will be

$$W_0 = \tfrac{1}{2}\psi_0 E. \qquad \text{......(13)}$$

If K_0 is the capacity of the conductor,

$$E = K_0\psi_0, \qquad \text{......(14)}$$

and

$$K_0 = \frac{1}{2}\frac{E^2}{W_0}. \qquad \text{......(15)}$$

Since W, the potential energy due to any arbitrary distribution of the charge, may be greater, but cannot be less than W_0, the energy of the same charge when in equilibrium, the capacity may be greater, but cannot be less, than

$$\frac{1}{2}\frac{E^2}{W} \text{ or } \frac{[\Sigma\,(e)]^2}{\Sigma\,(e\psi)}. \qquad \text{......(16)}$$

This inferior limit of the capacity is greater than that derived from the maximum value of the potential, and, as we shall see, sometimes gives a very close approximation to the true capacity.

In the case of the cylinder, if we suppose λ to be uniform, and neglect the electrification of the flat ends,

$$E = 2\lambda l, \quad W = 2\lambda^2 l\left(\log\frac{4l}{b} - 1\right), \qquad \ldots\ldots(17)$$

$$K_0 > \frac{l}{\log\frac{4l}{b} - 1}. \qquad \ldots\ldots(18)$$

When the length of the cylinder is more than 100 times the diameter this value of the capacity is sufficiently exact for all practical purposes. The capacity of the inscribed ellipsoid is

$$\frac{l}{\log\frac{2l}{b}}.$$

To obtain a closer approximation let us suppose that the linear density λ is expressed in the form $\lambda_0 + \lambda_1 + \&c. + \lambda_i$, the general term being

$$\lambda_n = A_n P_n\left(\frac{x}{l}\right), \qquad \ldots\ldots(19)$$

where P_n is the zonal harmonic of order n.

If we consider a line of length $2l$ on which there is a distribution of electricity according to this law, and if f_1 and f_2 are the distances of a given point from the ends of the line, and if we write

$$\alpha = \frac{1}{2}\frac{f_2 + f_1}{l}, \quad \beta = \frac{1}{2}\frac{f_2 - f_1}{l}, \qquad \ldots\ldots(20)$$

then the potential, ψ_n, at the given point (α, β), due to the distribution λ_n, is

$$\psi_n = A_n Q_n(\alpha)\, P_n(\beta), \qquad \ldots\ldots(21)$$

where P_n is the same zonal harmonic as in equation (19), and Q_n is the corresponding zonal harmonic of the second kind*, and is of the form

$$Q_n(\alpha) = P_n(\alpha)\log\frac{\alpha+1}{\alpha-1} + R_n(\alpha), \qquad \ldots\ldots(22)$$

where $R_n(\alpha)$ is a rational function of α of $n - 1$ degrees, and is such that $Q_n(\alpha)$ vanishes when α is infinite. The values of the first five harmonics of the second kind are

$$\left.\begin{aligned}
Q_0(\alpha) &= \log\frac{\alpha+1}{\alpha-1}, \\
Q_1(\alpha) &= \alpha\log\frac{\alpha+1}{\alpha-1} - 2, \\
Q_2(\alpha) &= (\tfrac{3}{2}\alpha^2 - \tfrac{1}{2})\log\frac{\alpha+1}{\alpha-1} - 3\alpha, \\
Q_3(\alpha) &= (\tfrac{5}{2}\alpha^3 - \tfrac{3}{2}\alpha)\log\frac{\alpha+1}{\alpha+1} - 5\alpha^2 + \tfrac{4}{3}, \\
Q_4(\alpha) &= (\tfrac{35}{8}\alpha^4 - \tfrac{15}{4}\alpha^2 + \tfrac{3}{8})\log\frac{\alpha+1}{\alpha-1} - \tfrac{35}{4}\alpha^3 + \tfrac{55}{12}\alpha.
\end{aligned}\right\} \qquad \ldots\ldots(23)$$

In applying these results to the determination of the potential at any point of the axis of the cylinder we must remember that a point on the axis is at the

* See Ferrers' *Spherical Harmonics*, chap. v.

distance b from any one of the generating lines of the cylinder, and therefore the potential at any point on the axis is the same as if the whole charge had been collected on one generating line.

Hence at the point on the axis for which $x = \xi$, if we write

$$L = \log\frac{f_1 + l - \xi}{b} + \log\frac{f_2 + l + \xi}{b}, \qquad \text{......(24)}$$

the potential due to the distribution whose linear density is

$$\lambda_n = A_n P_n\left(\frac{x}{l}\right), \qquad \text{......(25)}$$

is

$$\psi_n = A_n P_n\left(\frac{\xi}{l}\right)\left[L - n + \frac{n(n-1)}{2 \cdot 2} - \frac{n(n-1)(n-2)}{2 \cdot 3 \cdot 3} + \frac{n(n-1)(n-2)(n-3)}{2 \cdot 3 \cdot 4 \cdot 4} - \&c.\right] \qquad \text{......(26)}$$

approximately, provided ξ is between $\pm\, l$.

Thus, if

$$\left.\begin{aligned} \lambda_0 &= A_0, \\ \lambda_1 &= A_1\frac{x}{l}, \\ \lambda_2 &= A_2\left(\frac{3}{2}\frac{x^2}{l^2} - \frac{1}{2}\right), \\ \lambda_3 &= A_3\left(\frac{5}{2}\frac{x^3}{l^3} - \frac{3}{2}\frac{x}{l}\right), \\ \lambda_4 &= A_4\left(\frac{35}{8}\frac{x^4}{l^4} - \frac{15}{4}\frac{x^2}{l^2} + \frac{3}{8}\right), \end{aligned}\right\} \qquad \text{......(27)}$$

then

$$\left.\begin{aligned} \psi_0 &= A_0 L, \\ \psi_1 &= A_1\frac{\xi}{l}(L - 2), \\ \psi_2 &= A_2\left(\frac{3}{2}\frac{\xi^2}{l^2} - \frac{1}{2}\right)(L - 3), \\ \psi_3 &= A_3\left(\frac{5}{2}\frac{\xi^3}{l^3} - \frac{3}{2}\frac{\xi}{l}\right)(L - \tfrac{11}{3}), \\ \psi_4 &= A_4\left(\frac{35}{8}\frac{\xi^4}{l^4} - \frac{15}{4}\frac{\xi^2}{l^2} + \frac{3}{8}\right)(L - \tfrac{25}{6}). \end{aligned}\right\} \qquad \text{......(28)}$$

These values of the potential are calculated for the axis of the cylinder. The potential at the curved surface may be found from that at the axis by remembering that within the cylinder $\nabla^2\psi = 0$. At a distance b from the axis the potential is therefore

$$\psi_b = \psi - \frac{1}{4}\frac{d^2\psi}{d\xi^2}b^2 + \frac{1}{64}\frac{d^4\psi}{d\xi^4}b^4 - \&c., \qquad \text{......(29)}$$

where the values of ψ and its derivatives are those at the axis.

For a uniform distribution

$$\frac{d^2\psi}{d\xi^2} = -A_0\left(\frac{l-\xi}{f_1^{\,2}} + \frac{l+\xi}{f_2^{\,2}}\right), \qquad \text{......(30)}$$

which is approximately $-\dfrac{2A_0}{l}$, when $\xi = 0$, and $-\dfrac{A_0}{2l}$, when $\xi = \pm\, l$. Hence, when the length of the cylinder is many times its diameter, the potential at the axis may be taken for that at the surface in approximations of the kind here made.

We have next to find the integral of the product of the density into the potential. We may consider the product of each pair of terms by itself. If we write $\mathfrak{L}$ for the value of L when $\xi = l$, or approximately

$$\mathfrak{L} = \log \frac{4l}{b}, \qquad \text{......(31)}$$

$$\left.\begin{aligned}
\int \lambda_0 \psi_0 dx &= 4A_0{}^2 l\,(\mathfrak{L} - 1),\\
\int \lambda_0 \psi_2 dx &= \int \lambda_2 \psi_0 dx = -\tfrac{2}{3} A_0 A_2 l,\\
\int \lambda_2 \psi_2 dx &= \tfrac{4}{5} A_2{}^2 l\,(\mathfrak{L} - \tfrac{101}{30}),\\
\int \lambda_0 \psi_4 dx &= \int \lambda_4 \psi_0 dx = -\tfrac{1}{5} A_0 A_4 l,\\
\int \lambda_2 \psi_4 dx &= \int \lambda_4 \psi_2 dx = -\tfrac{2}{7} A_2 A_4 l,\\
\int \lambda_4 \psi_4 dx &= \tfrac{4}{9} A_4{}^2 l\,(\mathfrak{L} - \tfrac{6989}{1260}).
\end{aligned}\right\} \qquad \text{......(32)}$$

The charge is
$$E = \int \lambda dx = 2A_0 l. \qquad \text{......(33)}$$

Determining A_2 so as to make $\int (\lambda_0 + \lambda_2)(\psi_0 + \psi_2)\,dx$ a minimum, we find

$$A_2 = \tfrac{5}{6} A_0{}^2 \frac{1}{L - \frac{101}{30}},$$

and we obtain a second approximation to K,

$$K > \frac{l}{\mathfrak{L} - 1 - \dfrac{5}{36}\dfrac{1}{\mathfrak{L} - \frac{101}{30}}}. \qquad \text{......(34)}$$

This approximation is evidently of little use unless the length of the cylinder considerably exceeds 7·245 times its diameter, for this ratio makes the second term of the denominator infinite. It shows, however, that when the ratio of the length to the diameter is very great, the true capacity approximates to the value of K_0 given in (18).

We may proceed in the same way to determine A_2 and A_4 so that

$$\int (\lambda_0 + \lambda_2 + \lambda_4)(\psi_0 + \psi_2 + \psi_4)\,dx$$

shall be a minimum, and we thus find a third approximation to the value of the capacity, in which

$$A_2 = \tfrac{5}{6} A_0 \frac{\mathfrak{L} - \frac{3373}{630}}{(\mathfrak{L} - \frac{101}{30})(\mathfrak{L} - \frac{6989}{1260}) - \frac{45}{196}}, \quad A_4 = \tfrac{9}{20} A_0 \frac{\mathfrak{L} - \frac{457}{210}}{(\mathfrak{L} - \frac{101}{30})(\mathfrak{L} - \frac{6989}{1260}) - \frac{45}{196}},$$

so that when $\mathfrak{L}$ is very large the distribution approximates to

$$\lambda = A_0 \left[1 + \frac{1}{\mathfrak{L}} \frac{7}{32} \left\{9 \frac{x^4}{l^4} - 2 \frac{x^2}{l^2} - \frac{17}{15}\right\}\right].$$

The value of the inferior limit of the capacity, as given by this approximation, is

$$K_4 > \frac{l}{\mathfrak{L} - 1 - \dfrac{5}{36}\dfrac{1}{\mathfrak{L} - \frac{101}{30}} - \dfrac{9}{400}\dfrac{(\mathfrak{L} - \frac{457}{210})^2}{(\mathfrak{L} - \frac{101}{30})\left[(\mathfrak{L} - \frac{101}{30})(\mathfrak{L} - \frac{6989}{1260}) - \frac{45}{196}\right]}}.$$

As $\mathfrak{L}$ increases, K approaches to the value found by the first approximation.

To indicate the degree of approximation, the value of $\mathfrak{L}$ and of the successive terms of the denominator are given below.

$\frac{l}{b}$	$\mathfrak{L}$	Denominator of (34) and (35)		
		1st term	2nd term	3rd term
10	3·68888	2·68888	− 0·43151	
20	4·38203	3·38203	− 0·13680	
30	4·78749	3·78749	− 0·09775	
50	5·29832	4·29832	− 0·07191	
100	5·99146	4·99146	− 0·05291	− 0·13566
1000	8·29405	7·29405	− 0·02818	− 0·00892

The observed capacities of Cavendish's cylinders may be deduced from the numbers given in Art. 281 by taking the capacity of the globe of 12·1 inches diameter equal to 6·05, and their capacities as calculated by the formula of this note are given in the following table.

Length	Diameter	Capacity by formula	As measured by Cavendish
72	·185	5·668	5·669
54·2	·73	5·775	5·754
35·9	2·53	5·907	6·044

The agreement of the calculated and measured values is remarkable.

NOTE 13, ARTS. 152, 280.

[*Electric Distribution on*] *two cylinders.*

In the case of two equal and parallel cylinders at distance c, the linear densities being uniform and equal to λ_1 and λ_2, the part of the potential energy arising from their mutual action is

$$\tfrac{1}{2}\int\lambda_1\psi_2 dx = \int\lambda_2\psi_1 dx = \lambda_1\lambda_2\left(4l\log\frac{r+2l}{c} - 2r\right),$$

where

$$r^2 = 4l^2 + c^2.$$

If the two cylinders are in electric communication with each other $\lambda_1 = \lambda_2$, and the capacity of the two cylinders together is approximately

$$\frac{2l}{\log\frac{4l}{b} - 1 + \log\frac{r+2l}{c} - \frac{r-c}{2l}}.$$

If a cylinder is placed at a distance d from a conducting plane surface and parallel to it, then the electric image of the cylinder will be at a distance $c = 2d$, and its charge will be negative, so that the capacity of the cylinder will be increased. The capacity of the cylinder in presence of a conducting plane at distance $\tfrac{1}{2}c$, is

$$\frac{l}{\log\frac{4l}{b} - 1 - \log\frac{r+2l}{c} + \frac{r-c}{2l}}.$$

Thus in Cavendish's experiment he used a brass wire 72 inches long and 0·185 in diameter. The capacity of this wire at a great distance from any other body would be 5·668 inches. Cavendish placed it horizontally 50 inches from the floor. The inductive action of the floor would increase its capacity to 5·994 inches; Cavendish, by comparison with his globe, makes it 5·844.

To compare with this he had two wires each 36 inches long and 0·1 inch diameter.

The capacity of one of these at a distance from any other body would be 2·8697 inches, or the two together would be 5·7394 inches.

The two wires were placed parallel and horizontal at 50 inches from the floor. Each wire was therefore influenced by the other wire, and also by the negative images of itself and the other wire.

The denominator of the fraction expressing the capacity is therefore

Distance	Wire itself		Other wire		Own image		Other image		
18	6·2724	+	0·8256	−	0·1759	−	0·1754	=	6·7467
24	6·2724	+	0·6596	−	0·1759	−	0·1733	=	6·5828
36	6·2724	+	0·4672	−	0·1759	−	0·1678	=	6·3959

The numerator of the fraction which expresses the capacity of both wires together is 36, so that the capacity of the two is

		From Cavendish's results
At 18 inches	5·334	4·967
24	5·469	5·026
36	5·629	5·277
Wire of 72 inches	5·994	5·844

Note 14, Art. 155.

Lemma XVI.

If we suppose the plate AB to be overcharged and the plate DF to be equally undercharged, the redundant fluid in any element of AB being numerically equal to the deficient fluid in the corresponding element of DF, then what Cavendish calls the repulsion on the column CE in opposite directions becomes in modern language the excess of the potential at C over that at E. Hence the object of the Lemma is to determine approximately the difference of the potentials of two curved plates when their equal and opposite charges are given, and to deduce their charges when the difference of their potentials is given. [Compare Green's formula, *Essay*, § 8.]

Note 15, Art. 169.

On the Theory of Dielectrics.

Cavendish explains the fact discovered by him, that the charge of a coated glass plate is much greater than that of a plate of air of the same dimensions, by supposing that in certain portions of the glass the electric fluid is free to move, while in the rest of the glass it is fixed.

Probably for the sake of being able to apply his mathematical theorems, he takes the case in which the conducting parts of the glass are in the form of strata parallel to the surfaces of the glass. He is perfectly aware that this is not a true physical theory, for if such conducting strata existed in a plate of glass, they would make it a good conductor for an electric current parallel to its surfaces. As this is not the case, Cavendish is obliged to stipulate, as in this proposition, that the conducting strata conduct freely perpendicularly to their surfaces, but do not conduct in directions parallel to their surfaces.

The idea of some peculiar structure in plates of glass was not peculiar to Cavendish. Franklin had shown that the surface of glass plates could be charged with a large quantity of electricity, and therefore supposed that the electric fluid was able to penetrate to a certain depth into the glass, though it was not able to get through to the other side, or to effect a junction with the negative charge on the other side of the plate.

The most obvious explanation of this was by supposing that there was a stratum of a certain thickness on each side of the plate into which electricity can penetrate, but that in the middle of the plate there was a stratum impervious to electricity. Franklin endeavoured to test this hypothesis by grinding away five-sixths of the thickness of the glass from the side of one of his vials, but he found that the remaining sixth was just as impervious to electricity as the rest of the glass*.

It was probably for reasons of this kind, as well as to ensure that his thin plates were of the same material as his thick ones, that Cavendish prepared his thin plate of crown glass by grinding equal portions off both sides of a thicker plate. [Art. 378.]

It appears, however, from the experiments, that the proportion of the thickness of the conducting to the non-conducting strata is the same for the thin plates as the thick ones, so that the operation of grinding must have removed non-conducting portions as well as conducting ones, and we cannot suppose the plate to consist of one non-conducting stratum with a conducting stratum on each side, but must suppose that the conducting portions of the glass are very small, but so numerous that they form a considerable part of the whole volume of the glass. If we suppose the conducting portions to be of small dimensions in every direction, and to be completely separated from each other by non-conducting matter, we can explain the phenomena without introducing the possibility of conduction through finite portions of glass.

It was probably because Cavendish had made out the mathematical theory of stratified condensers, but did not see his way to a complete mathematical theory of insulating media, in which small conducting portions are disseminated, that he here expounds the theory of strata which conduct electricity perpendicularly to their surfaces but not parallel to them.

* Franklin's Works, 2nd Edition, vol. 1, p. 301, Letter to Dr Lining, March 18, 1755.

In forming a theory of the magnetization of iron, Poisson was led to the hypothesis that the magnetic fluids are free to move within certain small portions of the iron, which he calls magnetic molecules, but that they cannot pass from one molecule to another, and he calculates the result on the supposition that these molecules are spherical, and that their distances from each other are large compared with their radii.

When Faraday had afterwards rediscovered the properties of dielectrics, Mossotti, noticing the analogy between these properties and those of magnetic substances, constructed a mathematical theory of dielectrics, by taking Poisson's memoir and substituting electrical terms for magnetic, and Italian for French, throughout.

A theory of this kind is capable of accounting for the specific inductive capacity being greater than unity, without introducing conductivity through portions of the substance of sensible size.

Another phenomenon which we have to account for is that of the residual charge of condensers, and what Faraday called electric absorption. The only notice which Cavendish has left us of a phenomenon of this kind is that recorded in Arts. 522, 523, in which it appeared "that a Florence flask contained more electricity when it continued charged a good while than when charged and discharged immediately."

To illustrate this phenomenon, I gave in "Electricity and Magnetism," Art. 328, a theory of a dielectric composed of strata of different dielectric and conducting properties*.

Professor Rowland has since shown† that phenomena of the same kind would be observed if the medium consisted of small portions of different kinds well mingled together, though the individual portions may be too small to be observed separately.

It follows from the property of electric absorption that in experiments to determine the specific inductive capacity of a substance, the result depends on the time during which the substance is electrified. Hence most of those who have attempted to determine the value of this quantity for glass have obtained results so inconsistent with each other as to be of no use. It is absolutely necessary, in working with glass, to perform the experiment as quickly as possible.

Cavendish does not give the exact duration of one of his "trials," but each trial probably took less than two or three seconds. His results are therefore comparable with those recently obtained by Hopkinson‡, who effected the different operations by hand.

* [See J. Hopkinson's collected *Scientific Papers* for theory and experiment.]

† *American Journal of Mathematics*, No. 1, 1878, p. 53.

‡ *Proceedings of the Royal Society*, June 14, 1877; *Phil. Trans.* 1878, Part 1, p. 17. [Reprinted in collected *Scientific Papers*.]

The results obtained by Gordon*, who employed a break which gave 1200 interruptions per second, and those obtained by Schiller† by measuring the period of electric oscillations, which were at the rate of about 14,000 per second, are much smaller than those obtained by Cavendish and by Hopkinson.

Hopkinson finds that the quotient of the specific inductive capacity divided by the specific gravity does not vary much in different kinds of flint glass. As Cavendish always gives the specific gravity, I have compared his results with those of Hopkinson for glass of corresponding specific gravity.

Electrostatic capacity of glass.

	Specific gravity	Cavendish	Hopkinson	Wüllner	Gordon	Schiller
Flint-glass	3·279	7·93				
Do., a thinner piece	3·284	7·65				
Light flint	3·2		6·85		3·013	2·96
Dense flint	3·66		7·4		3·054	3·66
Double extra-dense flint ...	4·5		10·1		3·164	
Very light flint	2·87		6·57			5·83
Plate-glass	2·8	8		6·10		6·43
Crown-glass...	2·53	8·6			3·108	

NOTE 16, ART. 185.

Mutual Influence of two Condensers.

To find the effect on the capacity of a condenser arising from the presence of another condenser at a distance which is large compared with the dimensions of either condenser.

Let A and B be the electrodes of the first condenser, let L and N be the capacities of A and B respectively, and M their coefficient of mutual induction, then if the potential of A is 1 and that of B is 0, the charge of A will be L and that of B will be M, and if both A and B are at potential 1 the charge of the whole will be $L + 2M + N$, and this cannot be greater than half the greatest diameter of the condenser.

Let a and b be the electrodes of the second condenser, let its coefficients be l, m, n, and let its distance from the first condenser be R.

Let us first take the condenser AB by itself, and let us suppose that the potentials of A and B are x and y respectively, then their charges will be $Lx + My$ and $Mx + Ny$ respectively.

At a distance R from the condenser the potential arising from these charges will be

$$\{Lx + M(x + y) + Ny\} R^{-1} = P,$$

* *Proc. R. S.* Dec. 12, 1878.

† *Pogg. Ann.* 152 (1874), p. 535.

and if the second condenser, whose capacity when its electrodes are in contact is $l + 2m + n$, is placed at a distance R from the first and connected to earth, its charge will be

$$-P(l + 2m + n) = Q.$$

This charge of the second condenser will produce a potential QR^{-1} at a distance R, and will therefore alter the potentials of A and B by this quantity, so that the potentials of A and B will be $x + QR^{-1}$ and $y + QR^{-1}$ respectively.

To find the capacity of A as altered by the presence of the second condenser, we must make the potential of $A = 1$ and that of $B = 0$, which gives

$$x - \{Lx + M(x + y) + Ny\}(l + 2m + n) R^{-2} = 1,$$

$$y - \{Lx + M(x + y) + Ny\}(l + 2m + n) R^{-2} = 0.$$

Hence $$x = y + 1,$$

and $$y = \{L + M + (L + 2M + N) y\}(l + 2m + n) R^{-2},$$

or $$y = \frac{(L + M)(l + 2m + n) R^{-2}}{1 - (L + 2M + N)(l + 2m + n) R^{-2}},$$

and the capacity of A is $Lx + My$ or $L + (L + M) y$, or

$$[AA] = L + \frac{(L + M)^2 (l + 2m + n)}{R^2 - (L + 2M + N)(l + 2m + n)}.$$

The charge of B is $Mx + Ny$ or $M + (M + N) y$, or

$$[AB] = M + \frac{(L + M)(M + N)(l + 2m + n)}{R^2 - (L + 2M + N)(l + 2m + n)}.$$

The charges of a and b are $-(l + m) P$ and $-(m + n) P$ respectively, or

$$[Aa] = -\frac{R(L + M)(l + m)}{R^2 - (L + 2M + N)(l + 2m + n)},$$

$$[Ab] = -\frac{R(L + M)(m + n)}{R^2 - (L + 2M + N)(l + 2m + n)}.$$

In these expressions we must remember that M is a negative quantity, that $L + M$ and $M + N$ can neither of them be negative, and that their sum $L + 2M + N$ cannot be greater than the largest semidiameter of the condenser. Hence if R is large compared with the dimensions of the condensers, the second terms of the values of $[AA]$ and $[AB]$ will be quite insensible, and even if the condensers are placed very near together these terms will be small compared with L, M, or N.

If a, instead of being part of a condenser, is a conductor at a considerable distance from any other conductor, we may put $m = n = 0$, and if A is also a simple conductor, $M = N = 0$, and we find

$$[AA] = L + \frac{L^2 l}{R^2 - Ll},$$

$$[Aa] = -\frac{RLl}{R^2 - Ll},$$

by which the capacities and mutual induction of two simple conductors at a distance R can be calculated when we know their capacities when at a great distance from other conductors. See Note 24.

Note 17, Art. 194.

Theory of the Experiment with the Trial Plate.

Let A and B be the inner, a and b the outer coatings of the Leyden jars.

Let C be the body tried and D the trial plate, M the wire connecting A with C, and N the wire connecting b with D.

Let E be the electrometer with its connecting wires.

Let the coefficients of induction be expressed by pairs of symbols within square brackets, thus, let $[(A + C)(C + D)]$ denote the sum of the charges of A and C when C and D are both raised to potential 1 and all the other conductors are at potential 0.

First Operation.—The insides of the two jars are charged to potential P_0, the outsides and all other bodies being at potential 0.

The charge of A is $[A(A + B)] P_0$, and that of b is $[b(A + B)] P_0$.

Second Operation.—The outside coating of b is insulated, the charging wire is removed, and the inside of B is connected to earth. The charges of A and of b remain as before.

Third Operation.—A is connected to C by the wire M, and b is connected to D by the wire N.

The charge of A is communicated to A, C, and M, and the potential of this system is P_1, and the charge of b is communicated to b, D, and N, and the potential of this system is P_2.

Hence we have the following equations to determine P_1 and P_2 in terms of P_0,

$$[(A + C + M)(A + C + M)] P_1 + [(A + C + M)(b + D + N)] P_2 = [A(A + B)] P_0, \qquad \text{......(1)}$$

$$[(A + C + M)(b + D + N)] P_1 + [(b + D + N)(b + D + N)] P_2 = [b(A + B)] P_0. \qquad \text{......(2)}$$

Fourth Operation.—The wires M and N are disconnected from C and D respectively, and the jars A and b are discharged and kept connected to earth.

The charges of C and D remain the same as before.

Fifth Operation.—The bodies C and D are connected with each other and with the electrometer E, and the final potential of the system CDE is observed by the electrometer to be P_3.

Equating the final charge of the system CDE to that of the system CD at the end of the fourth equation,

$$[(C + D + E)(C + D + E)] P_3 = [(C + D)(A + C + M)] P_1 + [(C + D)(b + D + N)] P_2. \qquad \text{......(3)}$$

Eliminating P_1 and P_2 from equations (1), (2) and (3),

$$P_3\,[(C+D+E)^2]\,\{[(A+C+M)^2]\,[(b+D+N)^2] - [(A+C+M)\,(b+D+N)]^2\}$$

$$= P_0 \left\{\begin{array}{l} [A\,(A+B)]\,\{[(C+D)\,(A+C+M)]\,[(b+D+N)^2] \\ \quad - [(C+D)\,(b+D+N)]\,[(A+C+M)\,(b+D+N)]\} \\ + [b\,(A+B)]\,\{[(C+D)\,(b+D+N)]\,[(A+C+M)^2] \\ \quad - [(C+D)\,(A+C+M)]\,[(A+C+M)\,(b+D+N)]\} \end{array}\right\} \quad \text{......(4)}$$

By means of his gauge electrometer, Art. 249, Cavendish made the value of P_0 the same in every trial, and altered the capacity of D, the trial plate, so that P_3 in one trial had a particular positive value, and in another an equal negative value. He then wrote down the difference of the two values of D as an indication to guide him in the choice of trial plates, and the sum of the two values, by means of which he compared the charges of different bodies.

He then substituted for C a body, C', of nearly equal capacity, and repeated the same operations, and finally deduced the ratio of C to C' from the equation

$$C : C' :: D_1 + D_2 : D_1' + D_2'.$$

The capacities of the two jars were very much greater than any of the other capacities or coefficients of induction in the experiment, and of these $[b\,(B+b)]$ was less than half the greatest diameter of the second jar, and may therefore be neglected in respect of $[b^2]$ or $[Bb]$. We may therefore put $[Bb] = -[b^2]$, and in equation (4) neglect all terms except those containing the factors $[A^2]\,[b^2]$ or $[A^2]\,[Bb]$.

We thus reduce equation (4) to the form

$$\begin{aligned} P_3\,[(C+D+E)^2] &= P_0\,\{[(C+D)\,(A+C+M)] - [(C+D)\,(b+D+N)]\} \\ &= P_0\,\{[C^2] + [C\,(A+M)] - [C\,(b+N)] \\ &\quad - [D^2] - [D\,(b+N)] + [D\,(A+M)]\}. \end{aligned} \quad \text{......(5)}$$

The bodies to be compared were either simple conductors, such as spheres, disks, squares and cylinders, and those trial plates which consisted of two conducting plates sliding on one another, or else coated plates or condensers.

Now the coefficient of induction between a coated plate and a simple conductor is much less than that between two simple conductors of the same capacity at the same distance, and the coefficient of induction between two coated plates is still smaller. See Note 16.

Hence if both the bodies tried are coated plates, the equation (5) is reduced to the form

$$P_3\,([C^2] + [D^2] + [E^2]) = P_0\,([C^2] - [D^2]), \quad \text{......(6)}$$

so that the experiment is really a comparison of the capacities of the two bodies C and D.

But if either of them is a simple conductor, we must add to its capacity its coefficient of induction on the wire and jar with which it is connected, and subtract from it its coefficient of induction on the other wire and jar. These two coefficients of induction are both negative, but that belonging to its own wire and jar is probably greater than the other, so that the correction on the whole is negative.

Hence in Cavendish's trials the capacity deduced from the experiment will be less for a simple conductor than for a coated plate of equal real capacity.

This appears to be the reason why the capacities of the plates of air when expressed in "globular inches," that is, when compared with the capacity of the globe, are about a tenth part greater than their computed values. See Art. 347.

It would have been an improvement if Cavendish, instead of charging the inside of both jars positively and then discharging the outside of B, had charged the inside of A and the outside of B from the same conductor, and then connected the outside of both to earth, using the inside of B instead of the outside, to charge the trial plate negatively. In this way the excess of the negative electricity over the positive in B would have been much less than when the outside was negative.

With a heterostatic electrometer, such as those of Bohnenberger or Thomson, in which opposite deflections are produced by positive and negative electrification, the determination of the zero electrification may be made more accurately than any other, and with such an electrometer P_3 should be adjusted to zero. But the only electrometer which Cavendish possessed was the pith ball electrometer, in which the repulsion between the balls when at any given distance depends on the square of the electrification, and in which therefore the indications are very feeble for low degrees or electrification. Cavendish therefore first adjusted his trial plate so as to produce a given amount of separation of the balls by positive electrification, and then altered the trial plate so as to produce an equal separation by negative electrification. In each case he has recorded a number expressing the side of a square electrically equivalent to the trial plate, together with the difference and the mean of the two values.

He seems to have adopted the arithmetical mean as a measure of the charge of the body to be tried. It is easy to see, however, that the geometrical mean would be a more accurate value. For, if we denote the values of the final potential of the trial plate by accented letters in the second trial, we have

$$P_3' ([C^2] + [D'^2] + [E^2]) = P_0 ([C^2] - [D'^2]). \qquad \ldots\ldots(7)$$

Since $P_3 + P_3' = 0$, we find by (6) and (7)

$$[C^2] ([C^2] + [E^2]) = [D^2] [D'^2] + \tfrac{1}{2} [E^2] ([D^2] + [D'^2]).$$

If we neglect the capacity of the pith ball electrometer, which is much less than that of the bodies usually tried, this equation becomes

$$[C^2]^2 = [D^2] [D'^2],$$

or the capacity of the body tried is the geometrical mean of the capacities of the trial plate in its positive and negative adjustments.

NOTE 18, ART. 216.

On the "Thoughts Concerning Electricity," and on an early draft of the Propositions in Electricity.

The theory of electricity sketched in the "Thoughts" is evidently an earlier form of that developed in the published paper of 1771. We must therefore consider the "Thoughts" as the first recorded form of Cavendish's theory, and this for the following reasons.

(1) Nothing is said in the "Thoughts" of the forces exerted by ordinary matter on itself and on the electric fluid. The only agent considered is the electric fluid itself, the particles of which are supposed to repel each other. This fluid is supposed to exist in all bodies whether apparently electrified or not, but when the quantity of the fluid in any body is greater than a certain value, called the *natural* quantity for the body, the body is said to be overcharged, and when the quantity is less than the natural quantity the body is said to be undercharged.

The forces exerted by undercharged bodies are ascribed, not, as in the later theory, to the redundant matter in the body, but to the repulsion of the fluid in other parts of space.

The theory is therefore simpler than in its final form, but it tacitly assumes that the fluid could exist in stable equilibrium if spread with uniform density over all space, whereas it appears from the investigations of Cavendish himself that a fluid whose particles repel each other with a force inversely as any power of the distance less than the cube would be in unstable equilibrium if its density were uniform.

This objection does not apply to the later form of the theory, for in it the equilibrium of the electric fluid in a saturated body is rendered stable by the attraction exerted by the fixed particles of ordinary matter on those of the electric fluid.

(2) The hypotheses are reduced in the later theory to one, and the third and fourth hypotheses of the "Thoughts" are deduced from this.

(3) In the "Thoughts" Cavendish appears to be acquainted only with those phenomena of electricity which can be observed without quantitative experiments. Some of his remarks, especially those on the spark, he repeats in the paper of 1771, but in that paper (Art. 95) he refers to certain quantitative experiments, the particulars of which are now first published [Art. 265].

The "Thoughts," however, though Cavendish himself would have considered them entirely superseded by the paper of 1771, have a scientific interest of their own, as showing the path by which Cavendish arrived at his final theory.

He begins by getting rid of the electric atmospheres which were still clinging to electrified bodies, and he appears to have done this so completely that he does not think it worth while even to mention them in the paper of 1771.

He then introduces the phrase "degree of electrification" and gives a quantitative definition to it, so that this, the leading idea of his whole research, was fully developed at the early date of the "Thoughts."

Several expressions which Cavendish freely used in his own notes and journals, but which he avoided in his printed papers, occur in the "Thoughts."

Thus he speaks of the "compression" or pressure of the electric fluid.

Besides the "Thoughts," which may be considered as the original form of the introduction to the paper of 1771, there is a mathematical paper corresponding to the Propositions and Lemmata of the published paper, but following the earlier form of the theory, in which the forces exerted by ordinary matter are not considered, and referring directly to the "Hypotheses" of the "Thoughts."

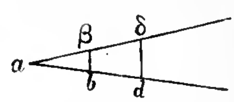
The first part of this paper is carefully written out, but it gradually becomes more and more unfinished, and at last terminates abruptly, though, as this occurs at the end of a page, we may suppose that the end of the paper has been lost. I think it probable, however, that when Cavendish had advanced so far, he was beginning to see his way to the form of the theory which he finally published, and that he did not care to finish the manuscript of the imperfect theory.

The general theory of fluids repelling according to any inverse power of the distance is given much more fully than in the paper of 1771, and the remarks [at the beginning] on the constitution of air are very interesting.

I have therefore printed this paper, but in order to avoid interrupting the reader with a repetition of much of what he has already seen, I have placed it at the end of this Note.

CAVENDISH'S FIRST MATHEMATICAL THEORY FROM MS. BUNDLE 17.

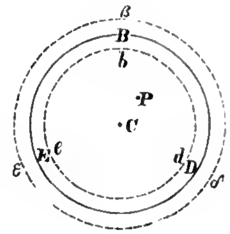
Let a fluid whose particles mutually repel each other be spread uniformly through infinite space. Let a be a particle of that fluid; draw the cone $ba\beta$ continued infinitely, and draw the section $b\beta$: if the repulsion of the particles is inversely as any higher power of the distance than the cube, the particle a will be repelled with infinitely more force from the particles between a and $b\beta$ than from all those situated beyond it, but if their repulsion is inversely as any less power than the cube, then the repulsion of the particles placed beyond $b\beta$ is infinitely greater than that of those between a and $b\beta$.



If the repulsion of the particles is inversely as the n power of the distance, n being greater than 3, it would constitute an elastic fluid of the same nature as air, except that its elasticity would be inversely as the $n + 2$ power of the distance of the particles, or directly as the $\frac{n+2}{3}$ power of the density of the fluid.

But if n is equal to, or less than 3, it will form a fluid of a very different kind from air, as will appear from what follows.

COR. I. Let a fluid of the above-mentioned kind be spread uniformly through infinite space except in the hollow globe BDE, and let the sides of the globe be so thin that the force with which a particle placed contiguous to the sides of the globe would be repelled by so much of the fluid as might be lodged within the space occupied by the sides of the globe should be trifling in respect of the repulsion of the whole quantity of fluid in the globe.



If the fluid within the globe was of the same density as without, the particles of the fluid adjacent to either the inside or outside surface of the globe would not press against those surfaces with any sensible force, as they would be repelled with the same force by the fluid on each side of them. But if the fluid within the globe is denser than that without, then any particle adjacent to the inside surface of the globe will be pressed against by the repulsion of so much of the fluid within the globe as exceeds what would be contained in the same space if it was of the same density as without, and consequently will be greater if the globe be large than if it be small. Consequently the pressure against a given quantity (a square inch for example) of the inside surface of the globe will be greater if the globe is large than if it is small.

If the particles of the fluid repel each other with a force inversely as their distance, the pressure against a given quantity of the inside surface would be as the square of the diameter of the globe. So that it is plain that air cannot consist of particles repelling each other in the above-mentioned manner.

If the repulsion of the particles was inversely as some higher power of the distance than the cube, then any particle of the fluid would not be sensibly affected except by the repulsion of those particles which were almost close to it, so that the pressure of the fluid against a given quantity of the inside surface would be the same whatever was the size of the globe, but then the elasticity [would] be in a greater proportion than that of the $\frac{5}{3}$ power of the density.

If the repulsion of the particles is inversely as some less power than the cube of the distance, and the density of the fluid within the globe is less than it is without, then the particles on the outside of the globe will press against it, and the force will be greater if the globe is large than if it be small.

If the density of the fluid within the globe be greater than without, then the density will not be the same in all parts of the globe, but will be greater near the surface and less near the middle, for if you suppose the density to be everywhere the same, then any particle of the fluid, as d, would be pressed with more force towards a, the nearest part of the surface of the sphere, than it would [be] in the contrary direction.

If the repulsion of the particles is inversely as the square of the distance, I think the inside of the sphere would be uniformly coated with the fluid to a certain thickness, in which the density would be infinite, or the particles would be pressed close together, and in all the space within that, the density would be the same as on the outside of the sphere.

The pressure of a particle adjacent to the inside surface against it is equal to the repulsion of all the redundant matter in the sphere collected in the center, and the force with which a particle is pressed towards the surface of the sphere diminishes in arithmetical progression in going from the inside surface to that point at which its density begins to be the same as without, therefore the whole pressure against the inside of the sphere is equal to that of half the redundant matter in the sphere pressed by the repulsion of all the redundant matter collected in the center of the sphere.

Therefore, if the quantity of fluid in the sphere is such that its density, if uniform, would be $1 + d$, and the radius of the sphere be called r, the whole pressure against the inside surface will be as $\frac{dr^3}{2} \times \frac{dr^3}{r^2}$, and the pressure against a given space of the inside surface will be as $d^2 r^2$.

If this pressure be called P, d is as $\frac{\sqrt{P}}{r}$, and dr^3 is as $r^2\sqrt{P}$. Consequently, supposing the fluid to be pumped into different sized globes, the quantity of fluid pumped in will be as the square root [of the force] with which it is pumped, multiplied by the square of the diameter of the globe.

If the density within the sphere is less than without, then the density within the sphere will not be uniform, but will be greater towards the middle and less towards the outside, and if the repulsion of the particles is inversely as the square of the distance, there would be a sphere concentric to the hollow globe in which the density would be the same as on the outside of the globe, and all between that and the inside surface of the globe would be a vacuum.

From these corollaries it follows that if the electric fluid is of the nature here described, and is spread uniformly through bodies, except when they give signs of electricity, that then if two similar bodies of different sizes be equally electrified, the larger body will receive much less additional electricity in proportion to its bulk than the smaller one, and moreover when a body is electrified, the additional electricity will be lodged in greater quantity near the surface of the body than near the middle.

Let us now suppose the fluid within the globe BDE to be denser than without, and let us consider [in what manner] the fluid without will be affected thereby.

1st. There will be a certain space surrounding the globe, as $\beta\delta\epsilon$, which will be a perfect vacuum, for first let us suppose that the density without the globe is uniform, then any particle would be repelled with more force from the globe than in the contrary direction.

2ndly. Let us suppose that the space $\beta\delta\epsilon$, BDE is not a vacuum, but rarer than the rest of the fluid; still a particle placed close to the surface of the globe would be repelled from it with more force than in the contrary direction.

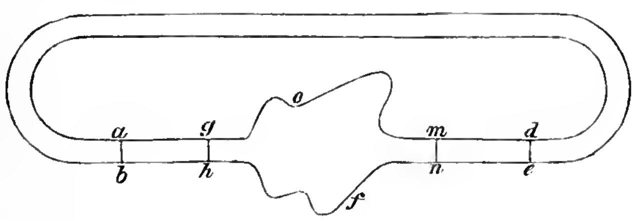
3rdly. Let us [suppose that] the density in the space between BDE and $\beta\delta\epsilon$ is greater than without, then according to some hypothesis of the law of repulsion a particle placed at B might be in equilibrium, but one placed at β could by no means be so.

So that there is no way by which the particles can be in equilibrium, unless there is a vacuum all round the globe to a certain distance. How the density of the fluid will be affected beyond this vacuum I cannot exactly tell, except in the following case:—

If the repulsion of the particles is inversely as the square of the distance, there will be a perfect vacuum between BDE and $\beta\delta\epsilon$, and beyond that the density will be perfectly uniform, $\beta\delta\epsilon$ being a sphere concentric to BDE, and of such a size, that if the matter in BDE was spread uniformly all over the sphere $\beta\delta\epsilon$, its density would be the same as beyond it.

For any quantity of matter spread uniformly over the globe $\beta\delta\epsilon$ or BDE affects a particle of matter placed without that sphere just in the same manner as if the whole fluid was collected in the center of the sphere, so that any particle of matter placed without the sphere $\beta\delta\epsilon$ will be in perfect equilibrio.

In like manner if the fluid within BDE is rarer than without, there will be a certain space surrounding the globe, as that between BDE and $\beta\delta\epsilon$, in which the density will be infinite, or in which the particles will be pressed close together, and if the repulsion of the particles is inversely as the square of the distance, the density of the fluid beyond that will be uniform: the diameter of $\beta\delta\epsilon$ being such that if all the matter within it was spread uniformly, its density would be the same as without.



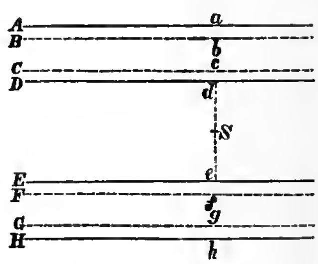
Let a fluid of the above-mentioned kind be spread uniformly through infinite space except in the canal *acdef* of any shape whatsoever, except that the ends *aghb* and *mden* are straight canals of an equal diameter, and of such a length that a particle placed at *a* or *d* shall not be sensibly affected by the repulsion of the matter in the part *gcmnfh*, and let there be a greater quantity of the fluid in this canal than in an equal space without.

Then the density of the fluid in different parts of the canal will be very different, but I imagine the density will be just the same at *a* as at *d*. For suppose *ab* and *de* to be joined, as in the figure, by a canal of an uniform diameter and regular shape, nowhere approaching near enough to *gcmnfh* to be affected by the repulsion of the particles within it. If the matter was not of the same density [at *a* and *d*] the matter therein could not be at rest, but there would be a continual current through the canal, which seems highly improbable.

COR. Let C be a conductor of electricity of any shape, *em* and *fn* wires extending from thence to a great distance. Let a and b be two equal bodies placed on those wires at such a distance from C as not to be sensibly affected

by the electricity thereof, and let the conductor or wires be electrified by any part: the quantity of electric fluid in the bodies a and b will not be sensibly different, or they will appear equally electrified.

Case 1. Let the parallel planes *Aa*, *Bb*, &c., be continued infinitely. Let all infinite space except the space contained between *Aa* and *Cc*, and between *Ee* and *Hh*, be filled uniformly with particles repelling inversely as the square of their distance; let the space between *Ee* and *Hh* be filled with fluid of the same density, the particles of which can move from one part to another; and let the space between *Aa* and *Cc* be filled with matter whose density is to [that in] the rest of space as *AD* to *AC*.



Take $EF = \frac{1}{2}CD$, and *GH* such that the matter between *Ee* and *Ff* when pressed close together, so that the particles touch each other, shall occupy the space between *Gg* and *Hh*.

The space between *Ee* and *Ff* will be a vacuum, that between *Ff* and *Gg* of the same density as the rest of space; and between *Gg* and *Hh* the particles will touch one another.

Case 2. Let everything be as in case the first, except that there is a canal opening into the plane *Hh*, by which the matter in the space *EH* is at liberty to escape; part of the matter will then run out, and the density therein will be everywhere the same as without, except in the space *EF*, which will be a vacuum, *EF* being equal to *CD*.

Case 3. Suppose now that a canal opens into the plane *Aa* by which the fluid in the space *AC* may escape. It will have no tendency to do so, for the repulsion of the redundant fluid in *AC* on a particle at a will be exactly equal to [the] want of repulsion of the space *EH*.

Case 4. Let now the space between *Aa* and *Cc* be filled with matter whose density is to the rest of space as *AB* to *AC*.

Then the space between *Hh* and *Gg* will be a vacuum, *GH* being equal to $\frac{1}{2}BC$. In the space *EF* the particles of matter will be pressed together so as to touch each other, the quantity of matter therein exceeding what is naturally contained in that space by as much as is driven out of the space *GH*; and in the space between *Ff* and *Gg* the matter will be of the same density as without.

Case 5. Suppose now that a canal opens into the plane *Hh* as in Case 2, then will matter run into the space *EH*, and the density will be everywhere the same as without, except in the space *EF*, where the particles will be pressed close together, the quantity of matter therein exceeding the natural quantity by as much as is naturally contained in the space *BC*.

Case 6. Suppose now that a canal opens into the plane *Aa*, the fluid will have no tendency to run out thereat.

Case 7. Let us now consider what will be the result if the repulsion of the particles is inversely as some other power of the distance between that of the square and the cube; and first let us suppose matters as in the first case. There will be a certain space, as *EF*, which will be a vacuum, and a certain space, as *FG*, in which the particles will be pressed close together; for if the matter is uniform in *EH*, all the particles will be repelled towards *H* if there is not a vacuum at *E*, nor the particles pressed close together at *G*, but only the density less at *E* than at *H*, then the repulsion of space *EH* at *E* will be less on [a] particle at *E* and greater on a particle at *H* than if the density was uniform therein, consequently on that account as well as on account of the repulsion of *AC* a particle at *E* or *H* will be repelled towards *H*, but if the space *EF* is a vacuum and the particles in *GH* pressed close together, then if the spaces *EF* and *GH* are of a proper size, a particle at *F* or *G* may be in equilibrio.

Case 8. If you now suppose a canal to open into the plane *Hh* as in the 3rd case, some of the matter will run out thereat, so that the whole quantity of matter in the space *EH* will be less than natural. For if not, it has already been shown that a particle at *H* will be repelled from *A*, but the quantity of matter which runs out will not be so much as the redundant matter in *AC*, for if there was, the want of repulsion of the space *EH* on a particle at *h* would be greater than the excess of repulsion of the space *AC*.

Case 9. Suppose now that a canal opens into the plane *Aa* as in Case 3; a particle at *a* will be repelled from *Dd*, but not with so much force as if there had been the natural quantity of fluid in the space *EH*, so that some of the fluid will run out at the canal, but not with so much force, nor will so much of the fluid run out as if there had been the natural quantity of fluid in *EH*.

Case 10. If you suppose matters to be as in the 4th case, then there must be a certain space adjacent to *Ee*, in which the particles will be pressed close together, and a certain space adjacent to *Hh* in which there must be a vacuum.

Case 11. If you suppose a canal to open into the plane *Hh*, some matter will run into the space *EH* thereby, so that the whole quantity of matter therein will be greater than natural.

The proof of these two cases is exactly similar to that of the two former.

Case 12. If you now suppose a canal to open into *Aa*, some fluid will run into it, but not with so much force nor in so great quantity as if the natural quantity of fluid had been contained in the space *Hh*.

I have supposed the planes *Aa*, &c. to be extended infinitely, because by that means I was enabled to solve the question accurately in the cases where the repulsion is supposed inversely as the square of the distance, which I could not have done otherwise, but it is evident that the phenomena will be nearly of the same kind if the planes are not infinitely extended.

For if the distance *ag* be small in respect of the length and breadth of the plane *Aa*, a particle placed at *a* will be repelled by the plane *Aa* with very nearly the same force as if the plane was infinitely extended.

It is plain that these 6 last cases agree very exactly with the laws of electricity laid down in the 3rd and 4th hypotheses [Thoughts...Art. 202].

If the lines *Bb* and *Dd* touch one another so that

[Here the MS. ends. ED.]

NOTE 19, ART. 234.

[*Determination of the Law of Electric Attraction.*]

Cavendish's Experiment on the Charge of a Globe between two Hemispheres.

This experiment has recently been repeated* at the Cavendish Laboratory in a somewhat different manner.

The hemispheres were fixed on an insulating stand, so as to form a spherical shell concentric with the globe, which stood inside the shell upon a short piece of a wide ebonite tube.

By this arrangement, since during the whole experiment the potentials of the globe and sphere remained sensibly equal, the insulating support of the globe was never exposed to the action of any sensible electromotive force, and therefore had no tendency to become charged.

If the other end of the insulator supporting the globe had been connected to earth, then, when the potential of the globe was high, electricity would have crept from it along the insulator, and would have crept back again when, in the second part of the experiment, the potential of the globe was sensibly zero. In fact this was the chief source of disturbance in Cavendish's experiment. See Art. 512.

Instead of removing the hemispheres before testing the potential of the globe, they were left in their position, but discharged to earth. The effect on the electrometer of a given charge of the globe was less than if the hemispheres had been removed, but this disadvantage was more than compensated by the perfect security from all external electric disturbances afforded by the conducting shell.

The short wire which formed the communication between the shell and the globe was fastened to a small metal disk hinged to the shell, and acting as a lid to a small hole in it, so that when the lid and its wire were lifted up by

* [By Sir Donald McAlister in 1878.]

means of a silk string, the electrode of the electrometer could be made to dip into the hole in the shell and rest on the globe within.

The electrometer was Thomson's Quadrant Electrometer.

The case of the electrometer, and one of the electrodes, were permanently connected to earth, and the testing electrode was also kept connected to earth, except when used to test the potential of the globe.

To estimate the original charge of the shell, a small brass ball was placed on an insulating stand at a distance of about 60 cm. from the centre of the shell.

The operations were conducted as follows:—

The lid was closed, so that the shell communicated with the globe by the short wire.

A Leyden jar was charged from a machine in another room, the shell was charged from the jar, and the jar was taken out of the room again.

The small brass ball was then connected to earth for an instant, so as to give it a negative charge by induction, and was then left insulated.

The lid was then lifted up by means of the silk string, so as to take away the communication between the shell and the globe.

The shell was then discharged and kept connected to earth.

The testing electrode of the electrometer was then disconnected from earth, and made to pass through the hole in the shell so as to touch the globe within without touching the shell.

Not the slightest deflexion of the electrometer could be observed.

To test the sensitiveness of the apparatus, the shell was disconnected from earth and connected to the electrometer. The small brass ball was then discharged to earth.

This produced a large positive deflexion of the electrometer.

Now in the first part of the experiment, when the brass ball was connected to earth, it became charged negatively, the charge being about $\frac{1}{54}$ of the original positive charge of the shell.

When the shell was afterwards connected to earth the small ball induced on it a positive charge equal to about one-ninth of its own negative charge. When at the end of the experiment the small ball was discharged to earth, this charge remained on the shell, being about $\frac{1}{486}$ of its original charge.

Let us suppose that this produces a deflexion D of the electrometer, and let d be the largest deflexion which could escape observation in the first part of the experiment.

Then we know that the potential of the globe at the end of the first part of the experiment cannot differ from zero by more than

$$\pm \frac{1}{486} \frac{d}{D} V,$$

where V is the potential of the shell when first charged.

But it appears from the mathematical theory that if the law of repulsion had been as $r^{-(2+q)}$, the potential of the globe when tested would have been by equation (25), p. 425,

$$0\cdot1478 \times qV.$$

Hence q cannot differ from zero by more than $\pm \frac{1}{72}\frac{d}{D}$.

Now, even in a rough experiment, D was certainly more than $300d$. In fact no sensible value of d was ever observed. We may therefore conclude that q, the excess of the true index above 2, must either be zero, or must differ from zero by less than

$$\pm \tfrac{1}{21600}.$$

Theory of the Experiment.

Let the repulsion between two charges e and e' at a distance r be

$$f = ee' \phi(r), \qquad \text{......(1)}$$

where $\phi(r)$ denotes any function of the distance which vanishes at an infinite distance.

The potential at a distance r from a charge e is

$$V = e\int_r^\infty \phi(r)\, dr. \qquad \text{......(2)}$$

Let us write this in the form

$$V = e\frac{1}{r} f'(r), \qquad \text{......(3)}$$

where

$$f'(r) = \frac{df(r)}{dr}, \qquad \text{......(4)}$$

and $f(r)$ is a function of r equal to $\int r\left[\int_r^\infty \phi(r)\, dr\right] dr$.

We have in the first place to find the potential at a given point B due to an uniform spherical shell.

Let A be the centre of the shell, a its radius, α its whole charge, and σ its surface-density, then

$$\alpha = 4\pi a^2\sigma. \qquad \text{......(5)}$$

Take A for the centre of spherical co-ordinates and AB for axis, and let $AB = b$.

Let P be a point on the sphere whose spherical co-ordinates are θ and ϕ, and let $BP = r$, then

$$r^2 = a^2 - 2ab\cos\theta + b^2. \qquad \text{......(6)}$$

The charge of an element of the shell at P is

$$\sigma a^2 \sin\theta\, d\theta\, d\phi = \frac{1}{4\pi}\alpha \sin\theta\, d\theta\, d\phi. \qquad \text{......(7)}$$

The potential at P due to this element is

$$\frac{1}{4\pi}\alpha\frac{f'(r)}{r}\sin\theta\, d\theta\, d\phi, \qquad \text{......(8)}$$

and the potential due to the whole shell is therefore

$$V = \int_0^{2\pi}\int_0^{\pi} \frac{1}{4\pi}\,\alpha\,\frac{r}{f'(r)}\sin\theta\,d\theta\,d\phi. \qquad \ldots\ldots(9)$$

Integrating with respect to ϕ from 0 to 2π,

$$V = \int_0^{\pi} \tfrac{1}{2}\,\alpha\,\frac{r}{f'(r)}\sin\theta\,d\theta. \qquad \ldots\ldots(10)$$

Differentiating (6) with respect to θ,

$$rdr = ab\sin\theta\,d\theta. \qquad \ldots\ldots(11)$$

Hence,
$$V = \int_{r_1}^{r_2} \frac{1}{2}\frac{\alpha}{ab} f'(r)\,dr = \frac{1}{2}\frac{\alpha}{ab}\{f(r_2) - f(r_1)\}, \qquad \ldots\ldots(12)$$

the upper limit r_2 being always $a + b$, and the lower limit r_1 being $a - b$ when $a > b$, and $b - a$ when $a < b$.

Hence, for a point inside the shell

$$V = \frac{\alpha}{2ab}\{f(a+b) - f(a-b)\}, \qquad \ldots\ldots(13)$$

for a point on the shell itself

$$V = \frac{\alpha}{2a^2} f(2a), \qquad \ldots\ldots(14)$$

and for a point outside the shell

$$V = \frac{\alpha}{2ab}\{f(b+a) - f(b-a)\}. \qquad \ldots\ldots(15)$$

We have next to determine the potentials of two concentric spherical shells, the radius of the outer shell being a and its charge α, and that of the inner shell being b and its charge β.

Calling the potential of the outer shell A, and that of the inner B, we find by what precedes,

$$A = \frac{\alpha}{2a^2} f(2a) + \frac{\beta}{2ab}\{f(a+b) - f(a-b)\}, \qquad \ldots\ldots(16)$$

$$B = \frac{\beta}{2b^2} f(2b) + \frac{\alpha}{2ab}\{f(a+b) - f(a-b)\}. \qquad \ldots\ldots(17)$$

In the first part of the experiment the shells communicate by the short wire and are both raised to the same potential, say V.

Putting $A = B = V$ and solving equations (16), (17), we find for the charge of the inner shell

$$\beta = 2Vb\,\frac{bf(2a) - a\{f(a+b) - f(a-b)\}}{f(2a)f(2b) - \{f(a+b) - f(a-b)\}^2}. \qquad \ldots\ldots(18)$$

In the original experiment of Cavendish the hemispheres forming the outer shell were removed altogether from the globe and discharged. The potential of the inner shell or globe would then be

$$B_1 = \frac{\beta}{2b^2} f(2b). \qquad \ldots\ldots(19)$$

In the form of the experiment as repeated at the Cavendish Laboratory, the outer shell was left in its place, but was connected to earth, so that $A = 0$.

In this case we find for the potential of the inner shell when tested by the electrometer

$$B_2 = V\left\{1 - \frac{a}{b}\frac{f(a+b) - f(a-b)}{f(2a)}\right\}. \qquad \text{......(20)}$$

Let us now assume with Cavendish, that the law of force is some inverse power of the distance, not differing much from the inverse square, that is to say, let

$$\phi(r) = r^{-(2+q)}, \qquad \text{......(21)}$$

then

$$f(r) = \frac{1}{1-q^2} r^{1-q}. \qquad \text{......(22)}$$

If we suppose q to be a small numerical quantity, we may expand $f(r)$ by the exponential theorem in the form

$$f(r) = \frac{1}{1-q^2} r\left\{1 - q\log r + \frac{1}{1\,.\,2}(q\log r)^2 - \ldots\right\}, \qquad \text{......(23)}$$

and if we neglect terms involving q^2, equations (19) and (20) become

$$B_1 = \frac{1}{2}\frac{a}{a-b} Vq\left[\frac{a}{b}\log\frac{a+b}{a-b} - \log\frac{4a^2}{a^2-b^2}\right], \qquad \text{......(24)}$$

$$B_2 = \tfrac{1}{2} Vq\left[\frac{a}{b}\log\frac{a+b}{a-b} - \log\frac{4a^2}{a^2-b^2}\right]. \qquad \text{......(25)}$$

Laplace [*Méc. Cel.* I. 2] gave the first direct demonstration that no function of the distance except the inverse square can satisfy the condition that a uniform spherical shell exerts no force on a particle within it.

If we suppose that β, the charge of the inner sphere, is always accurately zero, or, what comes to the same thing, if we suppose B_1 or B_2 to be zero, then

$$bf(2a) - af(a+b) - af(a-b) = 0.$$

Differentiating twice with respect to b, a being constant, and dividing by a, we find

$$f''(a+b) = f''(a-b),$$

or, if $a - b = c$,

$$f''(c+2b) = f''(c),$$

which can be true only if

$$f''(r) = C_0 \text{ a constant.}$$

Hence,

$$f'(r) = C_0 r + C_1,$$

and

$$\int_r^\infty \phi(r)\,dr = \frac{1}{r}f'(r) = C_0 + \frac{1}{r}C_1,$$

whence,

$$\phi(r) = C_1\frac{1}{r^2}.$$

We may notice, however, that though the assumption of Cavendish, that the force varies as some inverse power of the distance, appears less general than that of Laplace, who supposes it to be any function of the distance, it is the most general assumption which makes the ratio of the force at two different distances a function of the ratio of those distances.

If the law of force is not a power of the distance, the ratio of the forces at two different distances is not a function of the ratio of the distances alone, but also of one or more linear parameters, the values of which if determined by experiment* would be absolute physical constants, such as might be employed to give us an invariable standard of length.

Now although absolute physical constants occur in relation to all the properties of matter, it does not seem likely that we should be able to deduce a linear constant from the properties of anything so little like ordinary matter as electricity appears to be.

NOTE 20, ART. 272.

On the Electric Capacity of a Disk of sensible Thickness.

Consider two equal disks having the same axis, let the radius of either disk be a, and the distance between them b, and let b be small compared with a.

Let us begin by supposing that the distribution on each disk is the same as if the other were away, and let us calculate the potential energy of the system.

We shall use elliptical co-ordinates, such that the focal circle is the edge of the lower disk. In other words we define the position of a given point by its greatest and least distances from the edge of the lower disk, these distances being

$$a\,(\alpha+\beta) \text{ and } a\,(\alpha-\beta).$$

The distance of the given point from the axis is

$$r = a\alpha\beta, \qquad \text{......(1)}$$

and its distance from the plane of the lower disk is

$$z = a\,(\alpha^2-1)^{\frac{1}{2}}\,(1-\beta^2)^{\frac{1}{2}}. \qquad \text{......(2)}$$

If A_1 is the charge of the lower disk, the potential at the given point is

$$\psi = Aa^{-1}\operatorname{cosec}^{-1}\alpha, \qquad \text{......(3)}$$

or, if we write

$$\alpha^2 = \gamma^2 + 1, \qquad \text{......(4)}$$

$$\psi = Aa^{-1}\left(\frac{\pi}{2} - \tan^{-1}\gamma\right). \qquad \text{......(5)}$$

If A_2 is the charge of the upper disk, the density at any point is

$$\sigma = \frac{A_2}{2\pi a^2 p}, \qquad \text{......(6)}$$

where

$$p^2 = a^{-2}\,(a^2-r^2) = 1 - \alpha^2\beta^2. \qquad \text{......(7)}$$

Putting $z = b$ in equation (2),

$$b^2 = a^2\gamma^2\,(1-\beta^2) \quad \text{or} \quad \beta^2 = 1 - \frac{b^2}{a^2\gamma^2}. \qquad \text{......(8)}$$

Hence

$$p^2 = \frac{b^2}{a^2\gamma^2} - \gamma^2 + \frac{b^2}{a^2}. \qquad \text{......(9)}$$

* [This implies that such parameters are of sensible magnitude, and not determined by the dimensions of the electron.]

We have now to multiply the charge of an element of the upper disk into the potential due to the lower disk, and integrate for the whole surface of the upper disk,

$$\int 2\pi r\,dr\,\sigma\psi = A_1 A_2 a^{-1} \int_0^1 \left(\frac{\pi}{2} - \tan^{-1}\gamma\right) dp$$

$$= A_1 A_2 a^{-1} \left(\frac{\pi}{2} - \int_0^1 \tan^{-1}\gamma\, dp\right). \qquad \text{......(10)}$$

Between the limits of integration we may write with a sufficient degree of approximation,

$$\tan^{-1}\gamma = \gamma = \frac{b}{a}\left\{1 + \left(\frac{b}{a}\right)^{\frac{1}{2}}\right\}\left\{p + \left(\frac{b}{a}\right)^{\frac{1}{2}}\right\}^{-1}. \qquad \text{......(11)}$$

At the centre of the disk $p = 1$ and

$$\gamma = \frac{b}{a}, \text{ which agrees with (9).}$$

At the circumference,

$$p = 0 \text{ and } \gamma = \left(\frac{b}{a}\right)^{\frac{1}{2}} + \frac{1}{8}\left(\frac{b}{a}\right)^{\frac{3}{2}} \text{ by (9),}$$

whereas the equation (11) gives

$$\gamma = \left(\frac{b}{a}\right)^{\frac{1}{2}} + \frac{b}{a},$$

so that when b is very small compared with a, the value of γ cannot differ greatly from that given by equation (11). Hence we may write the expression (10)

$$A_1 A_2 a^{-1}\left[\frac{\pi}{2} - \frac{b}{a}\left\{1 + \left(\frac{b}{a}\right)^{\frac{1}{2}}\right\} \log\left\{\left(\frac{a}{b}\right)^{\frac{1}{2}} + 1\right\}\right]. \qquad \text{......(12)}$$

The corresponding quantity for the action of the upper disk on itself is got by putting $A_1 = A_2$ and $b = 0$, and is

$$A_2^2 a^{-1} \frac{\pi}{2}. \qquad \text{......(13)}$$

In the actual case $A_1 = A_2 = \frac{1}{2}E$, where E is the whole charge, and the capacity is

$$K > \frac{2a}{\pi - \frac{b}{a}\left\{1 + \left(\frac{b}{a}\right)^{\frac{1}{2}}\right\} \log\left\{\left(\frac{a}{b}\right)^{\frac{1}{2}} + 1\right\}}, \qquad \text{......(14)}$$

or, since in our approximation we have neglected $\left(\frac{b}{a}\right)^{\frac{1}{2}}$, our result may be expressed with sufficient accuracy in the form

$$K > \frac{2}{\pi}\left(a + \frac{1}{2\pi} b \log\frac{a}{b}\right), \qquad \text{......(15)}$$

showing that the capacity of two disks very near together is equal to that of an infinitely thin disk of somewhat larger radius.

If the space between the two disks is filled up, so as to form a disk of sensible thickness, there will be a certain charge on the curved surface, but at the same time the charge on the inner sides of the disks will disappear, and that on the outer sides near the edges will be diminished, so that the capacity of a disk of sensible thickness is very little greater than that given by (15).

We may apply this result to estimate the correction for the thickness of the square plates used by Cavendish. The factor by which we must multiply the thickness in order to obtain the correction for the diameter of an infinitely thin plate of equal capacity is $\frac{1}{2\pi}\log\frac{a}{b}$.

	$\frac{a}{b}$	$\frac{1}{2\pi}\log_e\frac{a}{b}$
Tin plate	600	1·017
Hollow plate........................	11	0·381
Portland stone, &c.	30	0·540
Slate	75	0·686

The correction is in every case much smaller than Cavendish supposed.

NOTE 21, ARTS. 277, 452, 473, 681.

Calculation of the Capacity of the Two Circles in Experiment VI.

The diameter of one of the circles was 9·3 inches, so that its capacity when no other conductor is in the field is $\frac{9{\cdot}3}{\pi} = 2{\cdot}960$. The distance between their centres was 36, 24, and 18 inches, which we may call c_1, c_2, and c_3.

The height of the centres of the circles above the floor was about 45 inches, so that the distance of the image of the circle would be about 90 inches and that of the image of the other circle would be about

$$r = (90^2 + c^2)^{\frac{1}{2}}.$$

Hence, if P is the potential of the circles when the charge of each is 1,

$$P = \frac{\pi}{2a} + \frac{1}{c} - \frac{2}{3}\frac{a^2}{c^3} + \&\text{c.} - 90^{-1} - r^{-1},$$

where the first term is due to the circle itself, the second and third to the other circle, as in Note 11, and the two last to the images of the two circles. We thus find for the three distances

$$P_1 = 0{\cdot}3438, \qquad P_2 = 0{\cdot}3567, \qquad P_3 = 0{\cdot}3689.$$

The capacity is $2P^{-1}$, and the number of "inches of electricity," according to the definition of Cavendish, is $4P^{-1}$,

or 11·636, 11·212, 10·844,

for the three cases.

The large circle was 18·5 inches in diameter and its centre was 41 inches from the floor, so that its charge would be 12·69 inches of electricity.

Hence the relative charges are as follows:

	Calculated	Measured by Cavendish, Art. 276
The large circle	1·000	1·000
The two small ones at 36 inches	·917	·899
24	·884	·859
18	·855	·811

NOTE 22, ART. 283.

Electric Capacity of a Square.

I am not aware of any method by which the capacity of a square can be found exactly. I have therefore endeavoured to find an approximate value by dividing the square into 36 equal squares and calculating the charge of each so as to make the potential at the middle of each square equal to unity.

The potential at the middle of a square whose side is 1 and whose charge is 1, distributed with uniform density, is

$$4 \log (1 + \sqrt{2}) = 3{\cdot}52549.$$

In calculating the potential at the middle of any of the small squares which do not touch the sides of the great square I have used this formula, but for those which touch a side I have supposed the value to be 3·1583, and for a corner square 2·9247.

If the 36 squares are arranged as in the margin, and if the charges of the corner squares be taken for unity, the charges will be as follows:

A	B	C	C	B	A
B	D	E	E	D	B
C	E	F	F	E	C
C	E	F	F	E	C
B	D	E	E	D	B
A	B	C	C	B	A

A	B	C	D	E	F
1·000	·599	·562	·265	·210	·201

and the capacity of a square whose side is 1 will be 0·3607.

The ratio of the capacity of a square to that of a globe whose diameter is equal to a side of the square is therefore 0·7214.

In Art. 654 Cavendish deduces this ratio from the measures in Art. 478 and finds it 0·73, which is very near to our result. If, however, we take the numbers given in Art. 478, we find the ratio 0·79. From Art. 281 we obtain the ratio 0·747.

The ratio of the charge of a square to that of a circle whose diameter is equal to a side of the square is by our calculation 1·133.

In Art. 648 Cavendish says that the ratio is that of 9 to 8 or 1·128, which is very close to our result, but in Arts. 283 and 682 he makes it 1·153.

The numbers in Art. 281 from which Cavendish deduces this would make it 1·1514.

The numbers given in Art. 478 would make it 1·176.

Cavendish supposes that the capacity of a rectangle is the same as that of a square of equal area, and he deduces this from a comparison of the square 15·5 with the rectangle 17·9 × 13·4.

It is not easy to calculate the capacity of a rectangle in terms of its sides, but we may be certain that it is greater than that of a square of equal area.

For if we suppose the electricity on the square rendered immoveable, and if we cut off portions from two sides of the square and place them on the other two sides so as to form a rectangle, we are carrying electricity from a place of higher to a place of lower potential, and are therefore diminishing the energy of the system.

If we now make the electricity moveable, it will re-arrange itself on the rectangle and thereby still further diminish the energy. Hence the energy of a given charge on the rectangle is less than that of the same charge on the square, and therefore the capacity of the rectangle is greater than that of the square*.

Note 23, Arts. 288 and 542.

On the Charge of the Middle Plate of Three Parallel Plates.

The plates used by Cavendish were square, but for the purpose of a rough estimate of the distribution of electricity between the three plates we may suppose them to be three circular disks.

First consider two equal disks on the same axis, at a distance small compared with the radius of either.

If the disks were in contact, the distribution on each would be the same as on each of the two surfaces of a single disk, and it would be entirely on the outer surface.

If the distance between the disks is very small compared with their radii, the force exerted by one of the disks at any point of the other will be nearly but not quite normal to its surface. The component in the plane of the disk will be directed outwards from the centre, so that the density will be greater near the edge than in a single disk having the same charge, but as a first approximation we may assume that the sum of the surface-densities on both sides of any element of the disk is the same as if the other disk were away.

But the density on the outer surface of the disk will be increased, and the density on the inner surface diminished, by a quantity numerically equal to the normal component of the repulsion of the other disk divided by 4π, and the whole charge of the outer surface will be increased, and the whole charge of the inner surface diminished, by a quantity equal to the charge of that part of the other disk, the lines of force from which cut the disk under consideration.

* [Approximate results may be readily obtained for such problems by the principle that the potential energy of a system is stationary in the neighbourhood of a position of equilibrium. Thus the energy of a given charge on a square plate is very nearly the same as on a circular plate of equal area: therefore the capacities of the two plates are nearly the same—according to Maxwell's result the capacity is greater for the square plate, in the ratio 1·0027, in agreement with the final argument in the text. So also a cube may be reduced to a sphere of equal volume. The degree of error may be elucidated by comparing the known results for elliptic and circular plates of equal area, or for ellipsoidal and spherical bodies of equal volume.]

Hence the charges of the inner and outer surfaces of the disk are

$$\frac{A}{a}\omega \text{ and } \frac{A}{a}(a-\omega)$$

respectively, where the value of the elliptic co-ordinate ω is that corresponding to the edge of the other disk.

If a is the radius of either disk, and c the distance between them,

$$\omega = \frac{1}{\sqrt{2}}(c\sqrt{4a^2+c^2}-c^2)^{\frac{1}{2}}.$$

If we now place another equal disk on the same axis at a distance c from one of them, the potential being the same for all three, the new disk will greatly diminish the charge of the surface of the disk which is next to it, but it will not have much effect on the charges of the other surfaces.

The result will therefore be that the charges of the two outer disks will together be greater, but not much greater, than that of a single disk at the same potential, but the charge of each of the surfaces of the middle disk will be the same as that of one of the inner surfaces of a pair of disks at distance c. Hence the charge of the middle disk will be to that of the two outer disks together as ω to a.

If we substitute for the square plates of twelve inches in the side disks of 13·8 inches diameter which would have nearly the same capacity, then if the distance between the outer disks is 1·15 inches, c = ·575 and ω = 1·936 and $a = 3{\cdot}5\,\omega$, or the charge of the middle disk would be 3·5 times greater if the outer disks had been removed.

If the distance between the outer disks is 1·65 inches, c = ·875 and ω = 2·293, whence $a = 2{\cdot}2\,\omega$, or the charge of the middle disk would have been 2·2 times greater if the outer disks had been removed.

It is evident, however, that in the assumed distribution the potential is less at the edges of the outer disks than at their centres. The electricity will therefore flow more towards the edges of the outer disks, and, as this will raise the potential near the edge of the middle disk, the charge of the middle disk will be less than on our assumption. I have not attempted to estimate the distribution more approximately.

Cavendish found the charge of the middle disk $\frac{1}{7}$ and $\frac{1}{8}$ of what it would have been without the outer disks. This is much less than the first approximation here given, but much greater than Cavendish's own estimate, founded on the assumption that the distribution of electricity follows the same law in the three plates.

NOTE 24, ARTS. 338, 652.

On the Capacity of a Conductor placed at a finite distance from other Conductors.

Cavendish has not given any demonstration of the very remarkable formula given in Art. 338 for the capacity of a conductor at a finite distance from other conductors. We may obtain it, however, in the following manner.

If the distance of all other conductors is considerable compared with the dimensions of the positively charged conductor, C, whose capacity is to be tried, the negative charge induced on any one of the other conductors will depend only on the charge of the conductor C and not on its shape. This induced charge will produce a negative potential in all parts of the field; let us suppose that the potential thus produced at the centre of the conductor C is $-\frac{E}{x}$, where E is the charge of C and x is a quantity of the dimensions of a line.

If L is the capacity of C when no other conductor is in the field, then the potential due to the charge E will be $\frac{E}{L}$, and the potential, which arises from the negative charge induced on other conductors, will be $-\frac{E}{x}$, so that the actual potential will be $E\left(\frac{1}{L}-\frac{1}{x}\right)$.

Dividing the charge by the potential we obtain for the actual capacity

$$L\frac{x}{x-L},$$

or the capacity is increased in the ratio of x to $x-L$.

The idea of applying this result to determining the value of x by comparing the charges of bodies, the ratio of whose capacities is known, is entirely peculiar to Cavendish, and no one up to the present time seems to have attempted anything of the kind.

The height of the centre of the circles above the floor seems to have been about 45 inches. If we neglect the undercharge of other conductors and consider only the floor, x would be about 90 inches in modern measure, but as a capacity x is reckoned by Cavendish as $2x$ "inches of electricity," the value of x in "inches of electricity" would be 180.

If we could take into account the undercharged surfaces of the other conductors, such as the walls and ceiling, the "machine," etc., the value of x would be diminished, and it is probable that the value obtained from his experiments by Cavendish, $166\frac{1}{2}$, is not far from the truth.

NOTE 25, ARTS. 360, 539, 666.

Capacities of the large tin Cylinder and Wires.

The dimensions of the cylinder are given more accurately in Art. 539. It was 14 feet 8·7 inches long, and 17·1 inches circumference. Its capacity when not near any conductor would be, by the formula in Note 12, 22·85 inches, and when its axis was 47 inches from the floor it would be 31·3 inches, or in Cavendish's language 62·6 inches of electricity. Cavendish makes its computed charge 48·4, and its real charge 73·6. See Art. 666. Now the charge of either of the plates D and E was, by Art. 671, 26·3 inches of electricity, so that

$$\text{tin cylinder} = 1{\cdot}19\,(D + E).$$

The capacities of the different wires mentioned in Arts. 360 and 539 are, by calculation,

length	diameter	capacity
29	$\frac{1}{6}$	2·67
22	$\frac{1}{6}$	2·09
37	·15	3·13
27·6	·15	2·46
20·8	·15	1·88
31	·15	2·71
24	·15	2·28

The ratio of the charge of the first of these wires to that of the second is 1·37:

NOTE 26, ART. 369.

Action of Heat on Dielectrics.

The effect of heat in rendering glass a conductor of electricity is described in a letter from Kinnersley to Franklin* dated 12th March, 1761. He found that when he put boiling water into a Florence flask he could not charge the flask, and that the charge of a three pint bottle went freely through without injuring the flask in the least.

Franklin in his reply describes some experiments of Canton's on thin glass bulbs, charged and hermetically sealed and kept under water, showing "that when the glass is cold, though extremely thin, the electric fluid is well retained by it."

He then describes an experiment by Lord Charles Cavendish, showing that a thick tube of glass required to be heated to 400° F. to render it permeable to the common current.

A portion of a glass tube near the middle of its length was made solid, and wires were thrust into the tube from each end reaching to the solid part. The middle portion of the tube was bent, so that a portion, including the solid part,

* Franklin's Works, edited by Sparks (1856), vol. v. p. 367.

could be placed in an iron pot filled with iron-filings. A thermometer was put into the filings; a lamp was placed under the pot; and the whole was supported upon glass.

The wire which entered one end of the tube was electrified by a machine, a cork ball electrometer was hung on the other, and a small wire, reaching to the floor, was tied round the tube between the pot and the electrometer, in order to carry off any electricity that might run along upon the tube.

"Before the heat was applied, when the machine was worked, the cork balls separated at first upon the principle of the Leyden phial. But after the middle part of the tube was heated to 600, the corks continued to separate, though you discharged the electricity by touching the wire, the electrical machine continuing in motion. Upon letting the whole cool, the effect remained till the thermometer was sunk to 400."

Experiments on the conductivity of glass at different temperatures have been made by Buff*, Perry†, and Hopkinson‡.

Hopkinson finds that if B is the specific conductivity divided by the specific inductive capacity and multiplied by 4π, then for

$$\text{glass N}^{\text{o}}.\ 2,\quad \log B = \bar{1}\cdot 35 + 0\cdot 0415\theta,$$
$$\text{glass N}^{\text{o}}.\ 7,\quad \log B = \bar{4}\cdot 17 + 0\cdot 0283\theta,$$

where θ is the temperature centigrade.

Glass N°. 2 is of a deep blue colour; it is composed of silica, soda, and lime.

Glass N°. 7 is "optical light flint," density 3·2, composed of silica, potash, and lead; almost colourless, the surface neither "sweats" nor tarnishes in the slightest degree. This glass at ordinary temperatures is sensibly a perfect insulator.

The conductivity of glass when heated makes it very difficult to determine its capacity as a dielectric. It appears from the experiments of Hopkinson on glasses of known composition, that the glasses made with soda and lime conduct more, and are also more subject to "electric polarization" and "residual charge" than those made with potash and lead.

Both the conductivity and the susceptibility to residual charge increase as the temperature rises, and this makes it very doubtful whether the apparent increase of dielectric capacity, which was observed by Cavendish and also by recent experimenters, is a real increase of the specific inductive capacity, or merely an effect of increased conductivity.

The experiments of Messrs Ayrton and Perry§ on wax at different temperatures would seem to indicate a real increase of dielectric capacity, as well as of conductivity, as the temperature rises up to the melting point. During the process of melting the capacity decreases and at higher temperatures begins to increase again, but the conductivity continues to increase as the temperature rises.

* *Annalen der Chemie und Pharmacie*, XC. (1854), p. 257.

† *Proc. R. S.* 1875, p. 468.

‡ *Phil. Trans.* 167 (1877), p. 599.

§ *Phil. Mag.* August, 1878.

NOTE 27, ART. 376.

Electrostatic capacity of different substances.

	Cavendish	Boltzmann	Wüllner	Gordon
Shellac*	4·47		2·95 to 3·73	2·746
Rosin		2·55		
Rosin and bees'-wax	3·38			
Dephlegmated bees'-wax	3·7			
Plain bees'-wax	4			
	Schiller			
Sulphur		3·84	2·88 to 3·21	2·579
Ebonite	2·21 to 2·76	3·15	2·56	2·284
Paraffin	1·81 to 2·47	2·32	1·96	1·994
Black caoutchouc	2·12			2·22
vulcanized	2·69			2·497

NOTE 28, ART. 383.

Capacity of a Cylindrical Condenser.

The rule by which Cavendish computed the charge of a condenser consisting of two cylindrical surfaces having the same axis is given at Art. 313.

If R is the external and r the internal radius, and l the length of the cylinders, then Cavendish's expression for the "computed charge" is $\frac{1}{2}\frac{R+r}{R-r}l$.

The true expression for the capacity is

$$\frac{1}{2}\frac{l}{\log R - \log r}$$

when the logarithms are Naperian.

We may express $\log R - \log r$ in the form of the series

$$2\frac{R-r}{R+r} + \frac{2}{3}\left(\frac{R-r}{R+r}\right)^3 + \frac{2}{5}\left(\frac{R-r}{R+r}\right)^5 + \text{\&c.},$$

and we thus find as an approximate value of the capacity

$$\frac{1}{4}l\frac{R+r}{R-r}\left\{1 - \frac{1}{3}\left(\frac{R-r}{R+r}\right)^2 - \frac{4}{45}\left(\frac{R-r}{R+r}\right)^4 - \text{\&c.}\right\}.$$

The first term agrees with Cavendish's rule, for the "capacity" is half the "inches of electricity," but the other terms show that Cavendish's rule gives too large a value for the computed charge.

* [In Kaye and Laby, *Physical and Chemical Constants*, 1911, the value for shellac is given as 3—3·7 and for rosin as 1·8—2·6. The value for crown glass is given as 5—7 and for flint as 7—10, both well above shellac, as Cavendish found, but in a different order. Cavendish's results are tabulated in Note 15, *supra*. It is found by Eguchi, *Proc. Phys. Math. Soc. of Japan*, 1920, that a mixture of resin and beeswax, solidified under pressure, exhibits permanent polarization, which is absent in the separate substances.]

The following table gives the charge as computed by Cavendish compared with that given by the correct formula.

	Cavendish	True	Observed charge by computed
Flint jar	85·9	72·55	9·88
...... cylinder	87·1	73·59	8·83
Therm. I.	11·0	8·37	9·58
.... II.	11·1	7·84	10·29
Green cyl. 1	77·2	65·92	11·15
.... 2	76·6	61·54	11·22
.... 3	40·8	34·29	10·29

NOTE 29, ART. 437.

Electrical Fishes.

The fishes which are known to possess the power of giving electric shocks belong to two genera of Teleostean Fishes and one of Elasmobranch Fishes, and the position and relations of the electric organs are different in each.

In every instance, however, the electric organ may be roughly described as being divided in the first place into parallel prisms or columns by septa, which we may call (with reference to the organ, not the fish) longitudinal septa, and in the second place each column is divided transversely by diaphragms, the structure of which is different in the different families, but in every case the terminations of the nerves lie on that surface of each diaphragm which during the discharge becomes its negative surface.

In the large family of the Torpedos the electric organs are formed of a large number of short columns, the columns running from the belly to the back of the fish. The nerves terminate on the ventral surface of each diaphragm, and the electric discharge is from belly to back through the organ, or in other words, the back of the fish becomes positive with respect to the belly.

There seems to be but one species of Gymnotus. It is a long eel-like fish. Its electric organs consist of a smaller number of very long columns running from the tail to the head of the fish. The nerves terminate on the posterior surface of the diaphragms, and the electric discharge is from tail to head through the organ, or the head of the fish becomes positive with respect to the tail.

There are three species of Malapterurus which are known to be electrical. In these the electric organs run longitudinally. Bilharz, observing that the nerves appear to terminate in an expansion like the head of a nail on the posterior surface of the diaphragms, concluded that the electric discharge must be from tail to head through the organ, as in the Gymnotus. Ranzi* however, and afterwards, independently of him, Du Bois Reymond† found that the discharge

* *Nuovo Cimento,* Tomo II, Dicembre 1856, p. 447, quoted by Du Bois Reymond "Zur Geschichte der Entdeckungen am Zitterwelse," *Archiv für Anatomie u. Physiologie,* &c. Leipzig, 1859, p. 210.

† *Monatsbericht d. k. Akad. Berlin,* 1858.

is really from head to tail through the organ, so that the tail becomes positive with respect to the head, and Schultze, who had been led to believe, from a comparison of his own observations on the organs of pseudo-electric fishes with the drawings of Bilharz, that the nerves might pass through the diaphragms and terminate on their anterior surfaces, found, on examining the preparations sent him by Du Bois Reymond, that this was really the case in Malapterurus, so that we may now assert that in every known case the terminations of the nerves are on that side of each diaphragm which during discharge becomes negative.

The origin of the nerves which supply the electric organs is different in the three families.

In the Torpedos the electric nerves are derived from the posterior division of the brain. Irritation of this lobe produces an electric discharge of the organ, but no muscular contraction. Irritation of other parts of the brain produces muscular contractions, but not electric discharges, unless the disturbance produced affects the electric nerves.

In the Gymnotus the electric nerves arise from the whole length of the spinal cord, and in Malapterurus the electric organs are supplied by the 2nd and 3rd pair of spinal nerves.

The electric nerves are so called because they govern the discharges of the electric organ. No essential difference has been observed between the electric phenomena in these nerves and those in other nerves. They must be classed, with respect to origin as well as function, among the motor nerves. The only difference is that their function is to govern the electric discharge of a peculiar organ, instead of the contraction of a muscle.

The experiments of Dr Davy* and those of Matteucci† showed that the discharge of the Torpedo produces all the known phenomena of an electric discharge. Faraday‡ did the same for the Gymnotus, and Du Bois Reymond§ for the Malapterurus.

M. Marey‖ has recently investigated some of the electrical phenomena of the discharge of the Torpedo. He employed three methods of indicating the discharge, the prepared leg of a frog, which is extremely sensitive to the feeblest current, but has the disadvantage that the time required for the contraction of the muscles, and still more the time required for their relaxation, is many times the period of the recurrence of the electric discharges of the Torpedo, so that the rapidly changing phases of the discharge cannot be distinguished by this method.

The second indicator used by Marey was the electromagnetic signal of M. Deprez, which can register 500 electric currents in a second by the motion of a tracing point over the smoked surface of a revolving cylinder. The action

* *Phil. Trans.* 1834.

† *Comptes Rendus*, 1836.

‡ *London Medical Gazette*, 1838.

§ *Berlin Monatsb.* 1858.

‖ *Travaux du Laboratoire de M. Marey*, III. (1877).

of this instrument was sufficiently prompt to register the number of the separate currents of which the "continued discharge" of the Torpedo consists. It was not, however, sufficiently sensitive to trace the curve of the intensity of the current when the strength of the current was less than that required to work the tracing point, and the trace therefore represents only the phases of greatest strength of current in each separate discharge.

M. Marey calls each separate discharge of the Torpedo an electric *flux*.

The whole discharge consists of a rapid succession of these fluxes, at the rate of from 60 to 140 per second, gradually decreasing in intensity, but remaining sensible sometimes for a second or a second and a half. In one of the tracings 120 fluxes may be counted quite distinctly, with a somewhat irregular continuation of feebler fluxes.

The electromagnetic signal, however, depending on the attraction of a soft iron armature, is acted on by a force varying nearly as the square of the strength of the current. It is therefore unable to respond to feeble currents, and it does not indicate the direction of the currents, even when improved in certain particulars by M. Marey.

The third indicator used by M. Marey was the capillary electrometer of M. Lippmann. In this instrument a capillary glass tube is filled in one part with mercury and in the other with dilute sulphuric acid. The pressure of the mercury is so adjusted that the division between the two liquids appears in the middle of the field of a microscope. The electrodes of the instrument are connected with the two liquids respectively, and when a small electromotive force acts from one electrode to the other, the surface of separation of the two liquids is seen to move in the same direction as the electromotive force, that is to say, the mercury advances if the electromotive force is from the mercury to the acid, and retreats if it is in the opposite direction.

This instrument, therefore, is admirably suited for the investigation of small electromotive forces, and the mass of the moving parts is so small that it responds most promptly to every variation of the electromotive force. Its only defect is that its range is limited to the electromotive force required to decompose the acid, and the electromotive force of the Torpedo, as we know, is of far greater intensity than this. M. Marey therefore used a shunt, so as to diminish the force acting on the electrometer to such a degree as to be within the working limits of the instrument.

He thus ascertained that the back of the fish is positive with respect to the belly, not only on the whole, but during every phase of each flux, and that it does not sink to zero between the fluxes.

The modern researches on the electric fishes would seem to point to the conclusion that the electric organ is not like a battery of Leyden jars in which electricity is stored up ready to be discharged at the will of the animal, but rather like a Voltaic battery, the metals of which are lifted out of the cells containing the electrolyte, but are ready to be dipped into them.

There seems to be no electric displacement in the organ till the electric nerve acts on it. The energy of the electric discharge which then takes place is not supplied to the organ by the nerve; the nerve only sets up an action which is carried on by the expenditure of energy previously supplied to the organ by the materials which nourish it.

During the discharge certain chemical changes take place in the organ. These changes involve a loss of intrinsic energy, and the chemical products found in the organ after repeated electric discharges are similar to the products found in muscles after they have performed mechanical work.

The organ, by repeated discharges, becomes incapable of responding to stimulation, and can only recover its power by the gradual process by which it is nourished.

Faraday proposed to try whether sending an artificial current through the Gymnotus would exhaust the organ, if sent in the direction of the natural discharge, or would restore it more rapidly to vigour if sent in the opposite direction. The only experiments on the effect of electricity on electric fishes seem to be those of Dr Davy, who found that an artificial current did not excite the electric organs of the Torpedo, though it had an effect on the muscles, but less than on those of other fishes, and of Du Bois Reymond, who found that Malapterurus was very slightly affected by induction currents passing through the water of his tub, though they were strong enough to stun and even to kill other fishes. When the induction currents were made very strong, the fish swam about till he had placed his body transverse to the lines of discharge, but did not appear to be much annoyed by them*.

The most valuable experiments hitherto made are probably those of Dr Carl Sachs, who went out to Venezuela in 1876 for the express purpose of studying the Gymnotus in its native rivers, with all the resources of Du Bois Reymond's methods. Dr Sachs lost his life in an Alpine accident in 1878, and as he did not himself publish his researches, it is to be feared that their results are lost to science.

* A somewhat extensive account of the subject is given in a dissertation, *De' Pesci elettrici e pseudoelettrici*, per Stefano St. Sihleanu (di Bucuresti, Romania), Napoli, 1876.

[Much attention has more recently been given to the subject by physiologists. It appears from microscopic observations (cf. Bayliss' *Physiology*, 1917, p. 661) that in *Malapterurus* the electric organ consists of a large number of parallel plates arranged along the fish, all innervated from a single neurone on each side: it gives a discharge at about 450 volts, lasting about ·005 sec. The manipulations of polarization and arrangement of cells by which discharges of high tension were obtained by Planté from his secondary batteries about thirty years ago may perhaps be regarded as in analogy with the organic activities that go on in mutual correlation in the electric organ of the fish.]

NOTE 30, ART. 560.

Excess of redundant fluid on positive side above deficient fluid on negative side of a coated plate.

When two equal disks have the same axis, the first being at potential V and the other connected to the earth, the algebraic sum of the charges of the two disks is just half the charge of the two disks together if they were both raised to potential V.

If the two disks are very near each other, the charge of the two together is very little greater than that of one by itself at the same potential.

Hence the excess of the redundant fluid above the deficient, when one of the disks is raised to potential V and the other connected with the earth, is very little greater than $\pi^{-1}aV$, where a is the radius. (See Note 4.)

NOTE 31, ART. 573.

Intensity of the Sensation produced by an Electric Discharge.

Cavendish tried this and several other experiments (Arts. 406, 573, 597, 610, 613) to determine in what way the intensity of the sensation of an electric shock is affected by the two quantities on which the physical properties of the discharge depend, namely the quantity of redundant fluid discharged, and the degree of electrification before it is discharged, the resistance of the discharging circuit being supposed constant.

He seems to have expected (Art. 597) that the strength of the shock would be "as the quantity of electricity into its velocity," or in modern language, as the product of the quantity into the mean strength of the current of discharge. Since the electromotive force acting on the body of the operator is measured by the product of the strength of the current into the resistance of the body, which we may suppose constant, Cavendish's hypothesis would make the intensity of the shock proportional to the work done by the discharge within the body.

According to this hypothesis, if a jar charged to a given degree produces a shock of a certain intensity, then a charge equal to n times the charge of this jar, communicated to n^2 similar jars, and discharged through the same resistance, would give a shock of equal intensity.

By the experiment recorded in Arts. 406 and 573, in which $n = 2$, it appeared that the shock given by four jars charged with the electricity of two jars, was rather greater than that of a single jar.

In the experiment in Art. 610 Cavendish compared the shock of jar 1 electrified to $2\frac{1}{2}$, with that of $B + 2A$ electrified to the same degree and communicated to the whole battery. Here the capacity of $B + 2A$ was equal to 6 times jar 1, and that of the whole battery was 154 times jar 1, so that 6 times the quantity of electricity communicated to 154 jars gave a shock of about the same strength, though as Cavendish remarks, "as there is a good deal of difference between the sensations of the two, it is not easy comparing them."

Here 154 is the $2\frac{7}{8}$ power of 6, so that the shock seems to depend rather more on the quantity of electricity than on the degree of electrification. This is the only experiment which Cavendish has worked out to a numerical result.

By the other experiments recorded in Art. 610, $34\frac{1}{2}$ communicated to 7 rows, gives a shock equal to 22 communicated to one row. This would make the number of jars as the 4·3 power of the charges. By Art. 613 the number of jars would be as the 3·3 power of the charge.

Cavendish had not the means of producing a steady current of electricity, such as we now obtain by means of a Voltaic battery, so that he could not discover the most important of the facts now known about the physiological action of the current, namely, that the effects of the current, whether in producing sensations, or in causing the contraction of muscles, depend far more on the rapidity of the changes in the strength of the current than on its absolute strength. It is true that a steady current, if of sufficient strength, produces effects of both kinds, but a current so weak that its effect, when steady, is imperceptible, produces strong effects, both of sensation and contraction, at the moments when the circuit is closed and broken.

But although this may be considered as established, I am not aware of any researches having been made, from the results of which it would be possible to determine, from the knowledge of the physical character of two electric discharges, which would produce the greater physiological effect.

The kind of discharges most convenient for experiments of this kind is that in which the current is a simple exponential function of the time, and of the form

$$x = Ce^{-\frac{t}{\tau}},$$

where x is the strength of the current at the time t, C its strength at the beginning of the discharge, and τ a small time, which we may call the time-modulus.

In this case the whole physical nature of the discharge is determined by the values of the two constants C and τ. The intensity of the sensation produced by the discharge through our nerves is, therefore, some function of these two constants, and if we had any method of ascertaining the numerical ratio of the intensities of two sensations, we might determine the form of this function by experiments. We can hardly, however, expect much accuracy in the comparison of sensations, except in the case in which the two sensations are of the same kind, and we have to judge which is the more intense.

According to Johannes Müller, the sensation arising from a single nerve can vary only in one way, so that, of two sensations arising from the same nerve, if one remains constant, while the other is made to increase from a decidedly less to a decidedly greater value, it must, at some intermediate value, be equal in all respects to the first.

In the ordinary mode of taking shocks by passing them through the body from one hand to the other, the sensations arise from disturbances in different nerves, and these being affected in a different ratio by discharges of different kinds, it becomes difficult to determine whether, on the whole, the sensation of one discharge or the other is the more intense.

I find that when the hands are immersed in salt water the quality of the sensation depends on the value of τ.

When τ is very small, say 0·00001 second, and C is large enough to produce a shock of easily remembered intensity in the wrists and elbows, there is very little skin sensation, whereas when τ is comparatively large, say 0·01 second, but still far too small for the duration of discharge to be directly perceived, the skin sensation becomes much more intense, especially in one place where the skin may have been scratched, so that it becomes almost impossible so to concentrate attention on the sensation of the internal nerves as to determine whether this part of the sensation is more or less intense than in the discharge in which τ is small.

There are two convenient methods of producing discharges of this type.

(1) If a condenser of capacity K is charged to the potential V, and discharged through a circuit of total resistance R (including the body of the victim),

$$C = \frac{V}{R}, \qquad \tau = KR.$$

The whole quantity discharged is $Q = C\tau = VK$, and if r is the resistance of the body of the victim, the work done by the discharge in the body is

$$W = \tfrac{1}{2} QV \frac{r}{R} = \tfrac{1}{2} V^2 K \frac{r}{R}.$$

(2) If the current through the primary circuit of an induction coil is y, the coefficient of mutual induction of the primary and secondary coils M, that of the secondary circuit on itself L, and the resistance of the secondary circuit R, then for the discharge through the secondary circuit when the primary circuit is broken,

$$C = \frac{M}{L} y, \qquad \tau = \frac{L}{R},$$

$$Q = \frac{M}{R} y, \qquad W = \frac{1}{2} \frac{M^2 y^2}{L} \frac{r}{R}.$$

I first tried the comparison of shocks by means of an induction coil, in which M was about 0·78 and L about 52 earth quadrants, and in which the resistance of the secondary coil was 2710 Ohms. By adding some German silver wire to the primary coil, its resistance was made up to nearly 1 Ohm, and the primary thus lengthened, another wire of the same resistance, and a variable resistance Q, were made into a circuit. One electrode of the battery was connected to the junction of the two equal resistances, and the other was connected alternately to the two ends of the resistance Q, so that the current through the primary was varied in the ratio of the primary P to $P + Q$, while the resistance of the battery-circuit remained always the same. When the smaller primary current,

y, was interrupted, I took the secondary discharge through my body directly, but when the larger current, y', was interrupted, I made the secondary discharge pass through a capillary tube filled with salt solution as well as my body.

The resistance between my hands when both were immersed in salt water was 1245 Ohms, making with the secondary coil a resistance of 3955 in the secondary circuit, so that the time-modulus of the discharge was $\tau = 1{\cdot}3 \times 10^{-3}$ seconds.

The resistance of the first capillary tube was 370,000, so that when it was introduced $\tau = 1{\cdot}4 \times 10^{-5}$.

By a rough estimate of the comparative intensity of the shocks I supposed them to be of equal intensity when $y' = 8{\cdot}4y$, and therefore if we suppose that two shocks remain of equal intensity when C varies as τ^p, $p = 0{\cdot}468$.

By another experiment in which a tube was used whose resistance was 450,000, $p = 0{\cdot}534$.

When the shocks at breaking contact were nearly equal, that at making contact was very much more intense with the small primary current and small secondary resistance than with the large primary current and large secondary resistance.

I then compared the discharges from two condensers of 1 and 0·1 microfarads capacity respectively, charging them with a battery of 25 Leclanché cells, the electromotive force of which was about 36 Ohms.

The resistance of the discharging circuit for the microfarad was 11,200 Ohms, including my body, so that

$$\tau = 1{\cdot}12 \times 10^{-2} \text{ seconds.}$$

The resistance of the discharging circuit of the tenth of a microfarad was 3600, so that $\tau' = 3{\cdot}6 \times 10^{-4}$.

The values of C were inversely as the resistances, so that if the two shocks were, as I estimated them, nearly equal, the value of p would be 0·670.

This experiment was much more satisfactory and more easily managed than that with the induction coil, and I thought it desirable to apply the same method to the comparison of the contractions of a muscle when its nerve was acted on by the discharge. I therefore availed myself of the kindness of Mr Dew-Smith, who prepared for me the sciatic nerve and gastrocnemius muscle of a frog, and attached the preparation to his myograph. The discharge was conducted through about 0·4 cm. of the nerve by means of Du Bois Reymond's unpolarizable electrodes, the resistance of the electrodes and nerve being 35,000 Ohms. When the electrodes were in contact their resistance was 23,000, leaving about 12,000 as the resistance of the nerve itself.

I used two condensers, one 0·1 microfarad, and the other an air-condenser of 270 centimetres capacity in electrostatic measure, or about 3×10^{-4} microfarads.

The first was charged by one cell and the second by 25. The resistances were arranged so that the contractions produced in the muscle were much less than a third of a maximum contraction. The discharges were made alternately every 15 seconds, and when the resistances were 35,000 and 140,000 respectively, the alternate contractions as recorded on the myograph were as follows:

Small condenser	Large condenser
144	146
147	148
147	147
146	146
147	145

Here the time-modulus was $1{\cdot}05 \times 10^{-5}$ seconds for the small condenser and $1{\cdot}4 \times 10^{-2}$ for the large one, and the values of C were as 1 to 100, so that $p = {\cdot}640$.

If we suppose that Cavendish took the shocks through pieces of metal held in his hands, the resistance of the circuit would depend on the state of his skin. He occasionally used a piece of apparatus, which he nowhere describes, but which he names in three places* a shock-melter.

From Art. 585 it would appear that it was filled with salt water, even when fresh water was the subject of the experiment, and from Art. 637 Cavendish seems to have considered it his last resource as a method of receiving shocks. I therefore think that it must have been an apparatus by which his hands were well wetted with salt water, so that the resistance of his body would be between 1000 and 2000 Ohms.

The capacity of his battery of 49 jars was 321,000 glob. inc., which comes to rather less than half a microfarad.

The discharges of this through 2000 Ohms would have a time-modulus of about one-thousandth of a second.

The following table gives the different results obtained by Cavendish and by myself, with the time-modulus of the discharges compared. The quantity p is such that the ratio of the initial strength of the two discharges is inversely as the p power of the ratio of the time-moduli when the shocks are equal in intensity, or

$$\frac{C_1}{C_2} = \left(\frac{\tau_1}{\tau_2}\right)^{-p}, \quad \frac{Q_1}{Q_2} = \left(\frac{\tau_1}{\tau_2}\right)^{1-p}, \quad \frac{W_1}{W_2} = \left(\frac{\tau_1}{\tau_2}\right)^{1-2p}.$$

The number of jars among which a quantity of electricity must be divided in order to give a shock of a given intensity through a given resistance, varies as the $\frac{1}{1-p}$ power of the quantity of electricity.

Cavendish's experiments.

	τ_1	τ_2	p
Art. 573	0·0000065	0·000026	0·5 +
... 610	0·0000065	0·001	0·652
... do.	0·00014	0·001	0·767
... 613	0·00014	0·00042	0·697

* Arts. 585, 622, 637. See facsimile at p. 317.

Experiments by the Editor.

Induction coil	0·000014	0·0013	0·468
do.	0·000011	0·0013	0·534
Condensers	0·00036	0·0112	0·670

Experiments on the prepared nerve and muscle of a frog.

	0·00001	0·014	0·640

This value of p does not differ much from 0·652, the only result which Cavendish has deduced in a numerical form from his experiments.

The most unaccountable of all the results arrived at by Cavendish is one which seems to have perplexed him so much that he has left the account of the experiments among which it occurs in a very imperfect state. He found (Arts. 639, 644) that the shock of a Leyden jar taken through a long thin copper wire produced a more intense sensation than when it was taken from the jar directly.

As in some of the experiments the wire was wound on a reel, and therefore the self-induction of the current might produce an oscillatory discharge, the physiological effects of which might be different from those of the simple discharge; I charged two Leyden jars to the same potential, using Thomson's Portable Electrometer as a gauge electrometer, and took the discharge of one through the secondary wire of an induction coil, the resistance of which was about 1000 Ohms, and that of the other through an ordinary resistance coil of 1000 Ohms.

In every trial I found that the sensation was more intense when taken through the ordinary resistance coil than when taken through the induction coil, and it is manifest that in the latter case the current begins and ends much less abruptly, so that the result is quite in accordance with the modern theory, that the sensation depends on the rapidity with which the strength of the current changes. I am, therefore, quite unable to account for the opposite result obtained by Cavendish. At the same time it is quite impossible that Cavendish could be mistaken in this comparison of the intensity of his sensations, for he had more practice than any other observer in comparing them, and he repeated this experiment many times.

The only apparent objection to the experiment is that the resistance of the copper wires was only 430 in one case and only 1000 in the other, whereas the resistance of a man's body, from one hand to the other, varies from about 1000 when the hands are thoroughly wet, to about 12,000 when they are dry, so that the resistance of the copper was small compared with the possible variations of the resistance of Cavendish's body.

The resistances of the tubes filled with solutions of salt, &c., were very much greater, being from 20,000 to 900,000.

Note 32, Arts. 398, 576, 687.

Comparison of the Resistance of Iron Wire and Salt Water.

Cavendish never published the method by which he made this comparison, but the result given in Art. 398 seems to have been accepted by men of science on Cavendish's bare word, without any question as to how it was obtained.

It appears fron Art. 576 that Cavendish made his body and the iron wire the branches of a divided circuit, and then tried how many inches of salt water must be put in the place of the iron wire, so that the shock might appear of the same strength.

By Matthiessen's experiments on the resistance of metals, the resistance of an iron wire of the dimensions given by Cavendish would be about 196 Ohms. As this is much less than that of a man's body from hand to hand, it would have made hardly any difference to the shock whether Cavendish took it through his body alone, or through his body and the iron wire in series.

By using the iron wire as a shunt and increasing the discharge so as to obtain a shock of easily remembered intensity, Cavendish was enabled to compare the wire with a column 5·1 inches long of saturated solution of salt.

By this experiment the resistance of saturated solution of salt is 355,400 times that of iron.

By the statements in Art. 398, that the resistance of rain-water is 400,000,000 times that of iron wire, and 720 times that of a saturated solution of sea-salt, the resistance of saturated solution would be 555,555 times that of iron wire.

It is true that this result given by Cavendish does not agree with the only experiment he has recorded, but we must remember that it is the only result which he published, and therefore he must have thought it the best he had.

By Kohlrausch's experiments on salt solutions combined with Matthiessen's on metals, the resistance of saturated solution of salt is 451,390 times that of annealed iron, when both are at 18° C. The ratio of the resistances would agree with that given by Cavendish at a temperature of about 11° C.

The coincidence with the best modern measurements is remarkable.

Note 33, Art. 619.

Conductivity of Solutions of Salt.

According to the measurements of Kohlrausch* the electric conductivity k, of saturated solution of sodium chloride, the conductivity of mercury at 0° C. being taken as unity, is given by the equation

$$10^8 k = 1259\,(1 + 0{\cdot}0308t + 0{\cdot}000146t^2).$$

* Wiedemann's *Annalen*, Bd. VI. (1879), p. 51.

When the temperature is near 18° C., we may use the equation

$$10^8 k = 2015 + 45{\cdot}1\,(t - 18).$$

Saturated solution at 18° contains according to Kohlrausch 26·4 per cent. of salt. Cavendish's saturated solution contained $\frac{1}{3{\cdot}78}$ of salt, which is equivalent to 26·45 per cent.

Kohlrausch finds that saturated solution of salt is one of the best standard substances for the comparison of the resistance of other electrolytes. Its conductivity seems to be sensibly the same, whether it is made with chemically pure salt or with the ordinary salt of commerce. The temperature coefficient is also smaller than that of many other electrolytes.

For other solutions of sodium chloride he finds that at 18°

$$10^8 k = 13650p - 22700p^2,$$

where p is the proportion, by weight, of the salt to the whole solution.

For the particular solutions examined by Cavendish we have

p	$10^8 k$	resistance in terms of sat. sol.	resistance found by Cavendish	
$\frac{1}{3{\cdot}78}$	2015	1	1	sat. sol.
$\frac{1}{12}$	980	2·56	1·91	salt in 11
$\frac{1}{30}$	430	4·69	3·97	salt in 29
$\frac{1}{70}$	190	10·58	8·8	salt in 69
$\frac{1}{143}$	94	21·44	15·75	salt in 142
$\frac{1}{150}$	90	22·39	20·05	salt in 149
$\frac{1}{1000}$	13·65	147·6	93·02	salt in 999
$\frac{1}{3000}$	4·55	442·9	340·85	salt in 2999

Note 34, Art. 626.

Conductivity of other Solutions.

The substances mentioned by Cavendish are easily identified, with the exception of "calc. S. S. A." and "f. alk. d." The weights of the quantities furnish no indication, for they are so large as to show that a dilute solution was used. The letters A and D probably indicate the bottles in which the solutions were kept.

The expression f. alk. or fixed alkali occurs in several parts of Cavendish's writings, especially in the manuscripts lithographed by Mr Vernon Harcourt in the *Report of the British Association* for 1839. It certainly means pearl ashes or carbonate of potash. The full title seems to have been *alkali fixum vegetabile*, as distinguished from *alkali fixum fossile*, which is sodic carbonate, and other writers seem to have used the expression fixed alkali for either of these, but Cavendish always uses the expression as a synonym for pearl ashes, and distinguishes potassic hydrate by the name of "sope leys."

The conductivity as determined by Cavendish agrees much better with potassic carbonate than with potassic hydrate, the conductivity of which is much greater.

It seems likely that calc. S. S. was sodic carbonate, and the conductivity would agree very well with this explanation, only it is difficult to find among the names in use at the time any which could be written in this form. Mr [P. T.] Main has suggested *Calcined Salsola Soda.* The burnt seaweed from the shores of the Mediterranean, from which soda was often extracted, was, I believe, called salsola, but I doubt whether the word soda was then in use.

The weights of the other substances are, when reduced to pennyweights, not very far from the equivalent numbers now received, hydrogen being taken as the unit.

The most remarkable exception is common salt itself, the solution of which was one in 29, and therefore in 1116 there were 37·2 parts of salt. Now the equivalent of NaCl is 58·5, which is very much greater.

Besides this the conductivity of a solution of salt in 29 of water would be much less in comparison with that of the other solutions than would appear from Cavendish's results, whereas if we assume that the molecular strength of the salt solution was really the same as that of the other solutions, the numbers do not differ much from those given by Kohlrausch.

The following table shows the results obtained by Cavendish and by Kohlrausch.

Name given by Cavendish	Modern symbol	Weight used by Cavendish	Modern equivalent	Conductivity found by Cavendish (Sea Salt = 1)	Conductivity found by Cavendish (NaCl = 1)
Sea Salt	NaCl	37·2?	58·5	1·00	1·00
Sal Sylvii	KCl	74	74·5	1·08	1·21
Sal Ammoniac	NH_4Cl	51	53·5	1·13	1·17
Calcined Glauber's Salt	$\frac{1}{2}Na_2SO_4$	69	71	0·696	0·95
Quadrangular Nitre	$NaNO_3$	89	85	0·887	0·91
Calc. S. S.	$\frac{1}{2}Na_2CO_3? + xH_2O$	346	$83 + 18x$	0·852	0·72
f. alk.	$\frac{1}{2}K_2CO_3 + xH_2O$	139	$99 + 18x$	0·819	0·96
Oil of Vitriol	$\frac{1}{2}H_2SO_4$	48	49	0·783	1·23
Spirit of Salt	$\frac{1}{2}HCl + xH_2O$	130	$36{\cdot}6+18x$	1·72	1·97

The theory of the electric resistance of electrolytes has been put on an entirely new footing by F. Kohlrausch, who has not only measured the resistance of a large number of solutions of different strengths and at different temperatures, but has discovered that the conductivity of a dilute solution of any electrolyte in water is the sum of two quantities, which we may call the specific conductivities of the components of the electrolyte, multiplied by the number of electro-chemical equivalents of the electrolyte in unit of volume of the solution. (Since the components of an electrolyte are not themselves electrolytes, it is manifest that they can have no actual conductivity, but the number to which we may give that name is such that when any two ions are actually combined into an electrolyte, the conductivity of the electrolyte depends on the sum of their respective numbers.)

Kohlrausch has also calculated the actual average velocity in millimetres per second with which the components are carried through the solution under an electromotive force of one volt per millimetre; and on the hypothesis that the components are charged with the electricity which travels with them, he has calculated the force in kilogrammes weight which must act on a milligramme of the component in order to make its average velocity in the solution one millimetre per second.

It appears to me that the simplest measure of the specific conductivity of an ion is the *time* during which we must suppose the electric force to act upon it so as to generate twice its actual average velocity. If we suppose that all the molecules of the ion are acted on by the electromotive force, but that each of them is brought to rest by a collision with a molecule of the opposite kind n times in a second, then the average velocity will be half that which the force can communicate to the molecule in the n^{th} part of a second.

Salts with univalent acids	$n \times 10^{-10}$	$T \times 10^{18}$	Univalent Metals with bivalent acids	$n \times 10^{-10}$	$T \times 10^{18}$
H	15941	6273	H_2	26732	3741
K	2354	42480	K_2	2844	35160
NH_4	5297	18880	$(NH_4)_2$	6719	14883
Na	6131	16310	Na_2	8730	11455
Li	30214	3310	Li_2	55430	1804
Ag	1030	97087	Ag_2	1275	78431
Cl	2551	39200	SO_4	2305	43384
Br	1030	97087	CO_3	4071	24564
I	637	156986			
F	7848	12740	Bivalent Metals		
CN	3433	29129	with SO_4		
NO_3	1569	63735	Mg	26480	3776
ClO_3	1324	75529	Zn	11281	8865
$C_2H_3O_2$	3286	30432	Cu	11772	8496
$\frac{1}{2}$Ba	2207	45310	SO_4	4218	23708
$\frac{1}{2}$Sr	3581	27925			
$\frac{1}{2}$Ca	8681	11520			
$\frac{1}{2}$Mg	16180	6180			
$\frac{1}{2}$Zn	6817	14670			
$\frac{1}{2}$Cu	4806	20807			

According to the theory of Clausius, it is only a small proportion, say $1/p$, of the molecules, which, at any given instant, are dissociated from molecules of the other kind, so as to be free to move under the action of the electromotive force, so that we must suppose each of the free molecules to continue free for a time pT; but since the proportion of free molecules to combined ones is quite unknown, the only definite result we can obtain from Kohlrausch's data is a certain very small time T, such that if the electromotive force acted on the molecules of the component during the time T, it would impress on them a velocity twice their actual average velocity.

Since the time T is very small, it is more convenient to speak of the molecule being brought to rest n times in a second, and to calculate n*.

Note 35, Art. 654.

On the Ratio of the Charge of a Globe to that of a Circle of the same Diameter.

The true value† of this ratio is $\frac{1}{2}\pi = 1\cdot570796...$ [: it had not been discovered until much later].

Cavendish has given several different values as the results of his experiments.

In the account of his experiments, which represents his most matured conclusions, he states this ratio is 1·57 (Art. 237).

All the other values, however, either as stated by Cavendish or as deducible from his experiments, are lower than this.

* [It is to be remembered that these remarks were published by Maxwell in 1879. The hypothesis of free time T has been developed by J. J. Thomson, and later has found application for the case of electrons, in a theory of electric conduction in metals by Drude and others.]

† [According to Lord Kelvin, writing in 1869 (*Papers on Electrostatics and Magnetism*, p. 179) the expression for the distribution of electricity on a circular disk, involving this value of its electric capacity, was first given by Green near the conclusion of his paper "On the Laws of the Equilibrium of Fluids analogous to the Electric Fluid" (*Cambridge Transactions*), so late as 1832, and comparison made with the experiments of Coulomb on a copper plate 10 inches in diameter.

It is however the determination of capacity, a conception rendered possible by his virtual introduction of the idea of potential, that is here original with Cavendish. The law of density of the charge on a disk, here referred to, is an immediate inference from Newton's theorem (*Principia*, Lib. 1, Prop. xci, cor. 3) that a gravitating shell bounded by similar ellipsoidal surfaces exerts no attraction throughout its interior. Cf. Maxwell's Introduction, *supra*, pp. 10, 18.

In a footnote Lord Kelvin records 'an entry which I find written in pencil in an old memorandum book.'

Plymouth, Mon. July 2, 1849.

"Sir William Snow Harris has been showing me Cavendish's unpublished MSS., put in his hands by Lord Burlington, and his work upon them: a most valuable mine of results. I find already the capacity of a disc (circular) was determined experimentally by Cavendish as 1/1·57 of that of a sphere of the same radius...."

After due expression of surprise he proceeds "It is much to be desired that these manuscripts of Cavendish should be published complete; or, at all events, that their safe keeping and accessibility should be secured to the world."]

In Art. 281 the charge of the globe of 12·1 inches diameter being 1, that of a circle 18·5 inches diameter is given as ·992. The ratio of the charge of a globe to that of a circle of equal diameter as deduced from this is 1·542.

In Art. 445 the charge of the globe is compared with that of a pasteboard circle of 19·4 inches diameter. Cavendish gives the actual observations but does not deduce any numerical result from them, which shows that he did not attach much weight to them. As they seem to be the earliest measurements of the kind, I have endeavoured to interpret the observations by assuming that the positive and negative separations were equal when the observations are qualified in the same words by Cavendish.

I thus find 14·2 or 14·3 for the charge of the globe, and 15·2 for that of the circle, and from these we deduce for the ratio of the charge of a globe to that of a circle of equal diameter 1·5054.

In Art. 456 the ratio as deduced by Cavendish from the observations on the globe and the tin circle of 18·5 inches diameter is 1·56.

From the numerical data given in the same article, the ratio would be 1·554.

Cavendish evidently thought the result given here of some value, for he quotes it in the footnote to Art. 473.

Another set of observations is recorded in Art. 478, from which we deduce the ratio 1·561.

It appears by a comparison of Arts. 506 and 581 that Cavendish, at the date of the latter article (which is doubtful), supposed the ratio to be 1·5. (See footnote to Art. 581.)

At Art. 648 the ratio is stated as 1·54.

At Art. 654 measures are given from which we deduce 1·542 and 1·37.

The numbers in Art. 682 are the same as those in Art. 281.

LIFE OF CAVENDISH

By THOMAS YOUNG, M.D., F.R.S.

(From the Supplement to the *Encyclopaedia Britannica*, 1816–1824.)

HENRY CAVENDISH, a great and justly celebrated Chemist, Natural Philosopher, and Astronomer; son of Lord Charles Cavendish, and grandson of William, second Duke of Devonshire; born the 10th of October, 1731, at Nice, where his mother, Lady Anne Grey, daughter of Henry, Duke of Kent, had gone, though ineffectually, for the recovery of her health.

Of a man, whose rank, among the benefactors of science and of mankind, is so elevated as that of Mr Cavendish, we are anxious to learn all the details both of intellectual cultivation and of moral character that the labours of a biographer can discover and record. Little, however, is known respecting his earliest education: he was for some time at Newcombe's school, an establishment of considerable reputation at Hackney; and he afterwards went to Cambridge: but it is probable that he acquired his taste for experimental investigation in great measure from his father, who was in the habit of amusing himself with meteorological observations and apparatus, and to whom we are indebted for a very accurate determination of the depression of mercury in barometrical tubes, which has been made the basis of some of the most refined investigations of modern times. "It has been observed," says M. Cuvier, "that more persons of rank enter seriously into science and literature in Great Britain than in other countries: and this circumstance may naturally be explained from the constitution of the British Government, which renders it impossible for birth and fortune alone to attain to distinction in the state, without high cultivation of the mind; so that amidst the universal diffusion of solid learning, which is thus rendered indispensable, some individuals are always found who are more disposed to occupy themselves in the pursuit of the eternal truths of nature, and in the contemplation of the finished productions of talent and genius, than in the transitory interests of the politics of the day." Mr Cavendish was neither influenced by the ordinary ambition of becoming a distinguished statesman, nor by a taste for expensive luxuries or sensual gratifications: so that, enjoying a moderate competence during his father's life, and being elevated by his birth above all danger of being despised for want of greater influence, he felt himself exempted from the necessity of applying to any professional studies, of courting the approbation of the public either by the parade of literature or by the habits of conviviality, or of ingratiating himself with mixed society by the display of superficial accomplishments. It is difficult to refrain from imagining that his mind had received some slight impression from the habitual recurrence to the motto of his family: the words *cavendo tutus* must have occurred perpetually to his eye; and all the operations of his intellectual powers exhibit a degree of *caution* almost unparalleled in the annals of science, for there is scarcely a single instance in which he had occasion to retrace his steps or to recall his opinions. In 1760 he became a Fellow of the Royal Society, and continued for almost fifty years to contribute to the *Philosophical Transactions* some of the most interesting and important papers that have ever appeared in that collection, expressed in language which affords a model of concise simplicity and unaffected modesty, and exhibiting a precision of experimental demonstration commensurate to the judicious selection of the methods of research and to the accuracy of the argumentative induction; and which have been considered, by some of the most enlightened historians, as having been no less instrumental in promoting the further progress of chemical discovery, by banishing the vague manner of observing and reasoning that had too long prevailed, than by immediately extending the bounds of human knowledge with respect to the very important facts which are first made public in these communications.

1. *Three Papers containing Experiments on Factitious Air.* (*Phil. Trans.* 1766, p. 141.) It had been observed by Boyle, that some kinds of air were unfit for respiration; and Hooke and Mayow had looked still further forwards into futurity with prophetic glances, which seem to have been soon lost and forgotten by the inattention or want of candour of their successors. Hales had made many experiments on gases, but without sufficiently distinguishing their different kinds, or even being fully aware that fixed air was essentially different from the common atmosphere. Sir James Lowther, in 1733, had sent to the Royal Society some bladders filled with coal-damp, which remained inflammable for many weeks—little imagining the extent of the advantages which were one day to result to his posterity from the labours of that society by the prevention of the fatal mischiefs which this substance so frequently occasioned. Dr Seip had soon after suggested that the gas which stagnated in some caverns near Pyrmont was the cause of the briskness of the water; Dr Brownrigg of Whitehaven had confirmed this opinion by experiments in 1741; and Dr Black, in 1755, had explained the operation of this fluid in rendering the earths and alkalis mild. Such was the state of pneumatic chemistry when Mr Cavendish began these experimental researches. He first describes the apparatus now commonly used in processes of this kind, a part of which had been before employed by Hales and others, but which he had rendered far more perfect by the occasional employment of mercury. He next relates the experiments by which he found the specific gravity of inflammable air to be about $\frac{1}{11}$ of that of common air, whether it was produced from zinc or otherwise: first weighing a bladder filled with a known bulk of the gas, and then in a state of collapse; and also examining the loss of weight during the solution of zinc in an acid, having taken care to absorb all the superfluous moisture of the gas by means of dry potass. He also observed that the gas obtained during the solution of copper in muriatic acid was rapidly absorbed by water, but he did not inquire further into its nature. The second paper relates to fixed air, which was found to undergo no alteration in its elasticity when kept a year over mercury;

to be absorbed by an equal bulk of water or of olive-oil, and by less than half its bulk of spirit of wine; to exceed the atmospheric air in specific gravity by more than one-half, and to render this fluid unfit for supporting combustion even when added to it in the proportion of 1 to 9 only. Mr Cavendish ascertained the quantity of this gas contained in marble and in the alkalis, but his numbers fell somewhat short of those which have been determined by later experiments; he also observed the solubility of the supercarbonate of magnesia. In the third part, the air produced by fermentation and putrefaction is examined. Macbride had shown that a part of it was fixed air; and our author finds that sugar and water, thrown into fermentation by yeast, emit this gas without altering the quantity or quality of the common air previously contained in the vessel, which retains its power of exploding with hydrogen, exactly like common air: he also shows that the gas thus emitted is identical with the fixed air obtained from marble; and that the inflammable air, extricated during putrefaction, resembles that which is procured from zinc, although it appeared to be a little heavier.

2. *Experiments on Rathbone Place Water.* (*Phil. Trans.* 1767, p. 92.) In this paper Mr Cavendish shows the solubility of the supercarbonate of lime, which is found in several waters about London, and is decomposed by the process of boiling, the simple carbonate being deposited in the form of a crust: the addition of pure lime-water also causes a precipitation of a greater quantity of lime than it contains. These conclusions are confirmed by synthetical experiments, in which the supercarbonate is formed and remains in solution.

3. *An Attempt to explain some of the principal Phenomena of Electricity by means of an Elastic Fluid.* (*Phil. Trans.* 1771, p. 584.) Our author's theory of electricity agrees with that which had been published a few years before by Æpinus, but he has entered more minutely into the details of calculation, showing the manner in which the supposed fluid must be distributed in a variety of cases, and explaining the phenomena of electrified and charged substances as they are actually observed. There is some degree of unnecessary complication from the great generality of the determinations: the law of electric attraction and repulsion not having been at that time fully ascertained, although Mr Cavendish inclines to the true supposition, of forces varying inversely as the square of the distance: this deficiency he proposes to supply by future experiments, and leaves it to more skilful mathematicians to render some other parts of the theory still more complete. He probably found that the necessity of the experiments, which he intended to pursue, was afterwards superseded by those of Lord Stanhope and M. Coulomb; but he had carried the mathematical investigation somewhat further at a later period of his life, though he did not publish his papers*: an omission, however, which is the less to be regretted, as M. Poisson, assisted by all the improvements of modern

* It is generally understood that Sir William Snow Harris, the eminent electrician, is engaged in the publication of these important papers.—*Note by the Editor* [Dean Peacock, in Young's *Miscellaneous Works*, vol. II, 1855. As regards the text, see Maxwell's Introduction to this volume: also footnote, p. 433.]

analysis, has lately treated the same subject in a very masterly manner. The acknowledged imperfections, in some parts of Mr Cavendish's demonstrative reasoning, have served to display the strength of a judgment and sagacity still more admirable than the plodding labours of an automatical calculator. One of the corollaries seems at first sight to lead to a mode of distinguishing positive from negative electricity, which is not justified by experiment; but the fallacy appears to be referable to the very comprehensive character of the author's hypothesis, which requires some little modification to accommodate it to the actual circumstances of the electric fluid, as it must be supposed to exist in nature.

4. *A Report of the Committee appointed by the Royal Society, to consider of a Method for securing the Powder Magazine at Purfleet.* (*Phil. Trans.* 1773, p. 42. *Additional Letter*, p. 66.) Mr Cavendish, and most of his colleagues on the committee, recommended the adoption of pointed conductors. Mr Wilson protested, and preferred blunt conductors; but the committee persisted in their opinion. Later experiments, however, have shown that the point in dispute between them was of little moment.

5. *An Account of some Attempts to imitate the Effects of the Torpedo by Electricity.* (*Phil. Trans.* 1776, p. 196.) The peculiarity of these effects is shown to depend in some measure on the proportional conducting powers of the substances concerned, and on the quantity of electricity, as distinguished from its intensity. Iron is found to conduct 400 million times as well as pure water, and sea water 720 times as well; and the path chosen by the electric fluid, depending on the nature of all the substances within its reach, an animal, not immediately situated in the circuit, will often be affected on account of the facility with which animal substances in general conduct the fluid. The shock of a torpedo, producing a strong sensation, but incapable of being conveyed by a chain, was imitated by the effect of a weak charge of a very large battery: and an artificial torpedo of wood being made a part of the circuit, the shock diffused itself very perceptibly through the water in which it was placed; but the experiment succeeded better when the instrument was made of wet leather, which conducts rather better than wood, the battery being more highly charged in proportion to the increase of conducting power.

6. *An Account of the Meteorological Instruments used at the Royal Society's House.* (*Phil. Trans.* 1776, p. 375.) Of the thermometers it is observed, that they are adjusted by surrounding the tubes with wet cloths or with steam, and barely immersing the bulbs in the water, since a variation of two or three degrees will often occur if these precautions are neglected. For the correction of the heights of barometers we have Lord Charles Cavendish's table of the depression arising from capillary action. The variation-compass was found to exhibit a deviation from the meridian 15′ greater in the house of the Royal Society than in an open garden in Marlborough Street; there was also a mean error of about 7′ in the indications of the dipping-needle, but it was difficult to ascertain the dip without being liable to an irregularity, which often amounted to twice as much.

7. *Report of the Committee appointed to consider of the best Method of adjusting Thermometers.* (*Phil. Trans.* 1777, p. 816.) This paper is signed by Mr Cavendish and six other members, but it is principally a continuation of the preceding. It contains very accurate rules for the determination of the boiling point, and tables for the correction of unavoidable deviations from them: establishing 29·8 inches as the proper height of the barometer for making the experiment, if only steam be employed, and 29·5 if the ball be dipped in the water; but with all precautions, occasional variations of half a degree were found in the results.

8. *An Account of a New Eudiometer.* (*Phil. Trans.* 1783, p. 106.) Mr Cavendish was aware of the great difference in the results of eudiometrical experiments with nitrous gas, or nitric oxyd, according to the different modes of mixing the elastic fluids; and he justly attributes them to the different degrees of oxygenization of the acid that is formed. But he found that when the method employed was the same, the results were perfectly uniform; and he ascertained in this manner that there was no sensible difference in the constituent parts of the atmosphere under circumstances the most dissimilar: the air of London, with all its fires burning in the winter, appearing equally pure with the freshest breezes of the country. He also observed the utility of the sulphurets of potass and of iron for procuring phlogisticated air; but he does not seem to have employed them as tests of the quantity of this gas contained in a given mixture.

9. *Observations on Mr Hutchins's Experiments for determining the degree of Cold at which Quicksilver freezes.* (*Phil. Trans.* 1783, p. 303.) In experiments of this kind, many precautions are necessary, principally on account of the contraction of the metal at the time of its congelation, which was found to amount to about $\frac{1}{23}$ of its bulk; and the results which had been obtained were also found to require some corrections for the errors of the scales, which reduce the degree of cold observed to 39° below the zero of Fahrenheit, or 71° below the freezing point, answering to $-$ 39·4° of the centesimal scale. In speaking of the evolution of heat during congelation, he calls it "generated" by the substances, and observes, in a note, that Dr Black's hypothesis of capacities depends "on the supposition that the heat of bodies is owing to their containing more or less of a substance called the matter of heat; and as" he thinks "Sir Isaac Newton's opinion, that heat consists in the internal motion of the particles of bodies, much the most probable," he chooses "to use the expression heat is generated," in order to avoid the appearance of adopting the more modern hypothesis; and this persuasion, of the non-existence of elementary heat, he repeats in his next paper*. It is remarkable that one of the first of Sir Humphry Davy's objects, at the very beginning of his singularly brilliant career of refined investigation and fortunate discovery, was the confirmation of this almost forgotten opinion of Mr Cavendish; and for this purpose he devised the very ingenious experiment of melting two pieces of ice by their mutual friction in a room below the freezing temperature, which is certainly incompatible with

* [See the account of Cavendish's dynamical manuscripts, at the end of the second volume of this edition.]

the common doctrine of caloric, unless we admit that caloric could have existed in the neighbouring bodies in the form of cold, or of something else that could be converted into caloric by the operation; and this transmutation would still be nearly synonymous with generation, in the sense here intended. However this may be, it is certain that, notwithstanding all the experiments of Count Rumford, Dr Haldalt, and others, Sir Humphry has been less successful in persuading his contemporaries of the truth of Mr Cavendish's doctrine of heat, than in establishing the probability of his opinions respecting the muriatic acid.

10. *Experiments on Air.* (*Phil. Trans.* 1784, p. 119.) This paper contains an account of two of the greatest discoveries in chemistry that have ever yet been made public—the composition of water, and that of the nitric acid. The author first establishes the radical difference of hydrogen from nitrogen or azote; he then proceeds to relate his experiments on the combustion of hydrogen with oxygen, which had partly been suggested by a cursory observation of Mr Warltire, a Lecturer on Natural Philosophy, and which prove that pure water is the result of the process, provided that no nitrogen be present*. These experiments were first made in 1781, and were then mentioned to Dr Priestley; and when they were first communicated to Lavoisier, he found some difficulty in believing them to be accurate. The second series of experiments demonstrates that when phlogisticated air, or nitrogen, is present in the process, some nitric acid is produced; and that this acid may be obtained from atmospheric air by the repeated operation of the electrical spark.

It has been supposed by one of Mr Cavendish's biographers, that if Mr Kirwan, instead of opposing, had adopted his chemical opinions, "he would never have been obliged to yield to his French antagonists, and the antiphlogistic theory would never have gained ground." But in this supposition there seems to be a little of national prejudice. Mr Cavendish by no means dissented from the whole of the antiphlogistic theory; and in this paper he has quoted Lavoisier and Scheele in terms of approbation, as having suggested the opinion "that dephlogisticated and phlogisticated air are quite distinct substances, and not differing only in their degree of phlogistication, and that common air is a mixture of the two." He afterwards mentions several memoirs of Lavoisier in which phlogiston is entirely discarded; and says that "not only the foregoing experiments, but most other phenomena of nature, seem explicable as well, or nearly as well, upon this as upon the commonly believed principle of phlogiston"; and after stating a slight conjectural objection, derived from the chemical constitution of vegetables, he proceeds finally to observe, that "Lavoisier endeavours to prove that dephlogisticated air is the acidifying principle": this is no more than saying, that acids lose their acidity by uniting to phlogiston, which, with regard to the nitrous, vitriolic, phosphoric, and arsenical acids, is certainly true, and probably with regard to the acid of sugar;

* M. Arago, in his Éloge of Watt, attempted to transfer to that philosopher the merit of this great discovery, and thus gave rise to a vehement controversy, which has been finally and conclusively settled in favour of Cavendish by Dr Wilson, of Edinburgh. See his Life of Cavendish, 1851.—*Note by the Editor* [Dean Peacock, 1855.]

"but as to the marine acid, and acid of tartar, it does not appear that they are capable of losing their acidity by any union with phlogiston" and the acids of sugar and tartar become even less acid by a further dephlogistication. It is obvious that this argument amounts only to an exception, and not to a total denial of the truth of the theory: M. Cuvier has even asserted that the antiphlogistic theory derived its first origin from one great discovery of Mr Cavendish, that of the nature of hydrogen gas, and owed its complete establishment to another, that of the composition of water: but it would be unjust to deny to Lavoisier the merit of considerable originality in his doctrines respecting the combinations of oxygen; and however he may have been partly anticipated by Hooke and Mayow, it was certainly from him that the modern English chemists immediately derived the true knowledge of the constitution of the atmosphere, which they did not admit without some hesitation, but which they did ultimately admit when they found the evidence irresistible. On the other hand, it has been sufficiently established, since Mr Cavendish's death, by the enlightened researches of the most original of all chemists, that Lavoisier had carried his generalization too far; and it must ever be remembered, to the honour of Mr Cavendish, and to the credit of this country, that we had not all been seduced, by the dazzling semblance of universal laws, to admit facts as demonstrated which were only made plausible by a slight and imperfect analogy.

11. *Answer to Mr Kirwan's Remarks upon the Experiments on Air.* (*Phil. Trans.* 1784, p. 170.) Mr Kirwan, relying on the results of some inaccurate experiments, had objected to those conclusions which form the principal basis of the antiphlogistic theory. Mr Cavendish repeated such of these experiments as seemed to be the most ambiguous, and repelled the objections; showing, in particular, that when fixed air was derived from the combustion of iron, it was only to be referred to the plumbago, shown by Bergmann to exist in it, which was well known to be capable, in common with other carbonaceous substances, of affording fixed air.

12. *Experiments on Air.* (*Phil. Trans.* 1785, p. 372.) The discovery of the composition of the nitric acid is here further established; and it is shown that the whole, or very nearly* the whole of the irrespirable part of the atmosphere is convertible into this acid, when mixed with oxygen, and subjected to the operation of the electric spark: the fixed air, sometimes obtained during the process, being wholly dependent on the presence of some organic substances.

13. *An Account of Experiments made by Mr John Macnab, at Henley House, Hudson's Bay, relating to Freezing Mixtures.* (*Phil. Trans.* 1786, p. 241.) From these experiments Mr Cavendish infers the existence of two distinct species of congelation in mixed liquids, which he calls the Aqueous and Spirituous Congelations, and of several alternations of easy and difficult congelation when the strength is varied, both in the case of the mineral acids and of spirit of wine. The greatest degree of cold obtained was $-78\frac{1}{2}°$.

[* That is, except the argon and other inert gases contained in Cavendish's residue.]

14. *An Account of Experiments made by Mr John Macnab, at Albany Fort, Hudson's Bay.* (*Phil. Trans.* 1788, p. 166.) The points of easy congelation are still further investigated, and illustrated by comparison with Mr Keir's experiments on the sulphuric acid. It was found that the nitric acid was only liable to the aqueous congelation, when it was strong enough to dissolve $\frac{1}{4}$th of its weight of marble; and that it had a point of easy congelation, when it was capable of dissolving $\frac{415}{1000}$, the frozen part exhibiting, in other cases, a tendency to approach to this standard. Mr Keir had found that sulphuric acid, of the specific gravity 1·78, froze at 46°, and that it had another maximum when it was very highly concentrated*.

15. *On the Conversion of a Mixture of Dephlogisticated and Phlogisticated Air into Nitric Acid, by the Electric Shock.* (*Phil. Trans.* 1788, p. 261.) Some difficulties having occurred to the Continental chemists in the repetition of this experiment, it was exhibited with perfect success, by Mr Gilpin, to a number of witnesses. This was an instance of condescension, which could scarcely have been expected from the complete conviction, which the author of the discovery must have felt, of his own accuracy, and of the necessity of the establishment of his discovery, when time should have been afforded for its examination.

16. *On the Height of the Luminous Arch, which was seen on Feb.* 23, 1784. (*Phil. Trans.* 1790, p. 101.) Mr Cavendish conjectures that the appearance of such arches depends on a diffused light, resembling the aurora borealis, spread into a flattened space, contained between two planes nearly vertical, and only visible in the direction of its breadth: so that they are never seen at places far remote from the direction of the surface; and hence it is difficult to procure observations sufficiently accurate for determining their height, upon so short a base: but in the present instance there is reason to believe that the height must have been between 52 and 71 miles.

17. *On the Civil Year of the Hindoos, and its Divisions, with an Account of three Almanacs belonging to Charles Wilkins, Esq.* (*Phil. Trans.* 1792, p. 383.) The subject of this paper is more intricate than generally interesting; but it may serve as a specimen of the diligence which the author employed in the investigation of every point more or less immediately connected with his favourite objects. The month of the Hindoos is lunar in its duration, but solar in its commencement; and its periods are extremely complicated, and often different for different geographical situations: the day is divided and subdivided sexagesimally. The date of the year, in the epoch of the Kalee Yug, expresses the ordinal number of years elapsed, as it is usual with our astronomers to reckon their days: so that the year 100 would be the beginning of the second century, and not the 100th year, or the end of the first century, as in the European calendar: in the same manner as, in astronomical language, 1817 December 31d. 18h. means six o'clock in the morning of the 1st of January 1818.

* [Compare with these two papers the modern investigations of coexistent chemical phases, and their applications to metallurgy and other sciences.]

18. *Experiments to determine the Density of the Earth.* (*Phil. Trans.* 1798, p. 469.) The apparatus, with which this highly important investigation was conducted, had been invented and constructed many years before by the Reverend John Michell, who did not live to perform the experiments for which he intended it. Mr Cavendish, however, by the accuracy and perseverance with which he carried on a course of observations of so delicate a nature, as well as by the skill and judgment with which he obviated the many unforeseen difficulties that occurred in its progress, and determined the corrections of various kinds which it was necessary to apply to the results, has deserved no less gratitude from the cultivators of astronomy and geography, than if the idea had originally been his own. The method employed was to suspend, by a vertical wire, a horizontal bar, having a leaden ball at each end; to determine the magnitude of the force of torsion by the time occupied in the lateral vibrations of the bar; and to measure the extent of the change produced in its situation by the attraction of two large masses of lead, placed on opposite sides of the case containing the apparatus, so that this attraction might be compared with the weight of the balls, or, in other words, with the attraction of the earth. In this manner the mean density of the earth was found to be $5\frac{1}{2}$ times as great as that of water; and although this is considerably more than had been inferred from Dr Maskelyne's observations on the attraction of Schehallion, yet the experiments agree so well with each other, that we can scarcely suppose any material error to have affected them. Mr Michell's apparatus resembled that which M. Coulomb had employed in his experiments on magnetism, but he appears to have invented it before the publication of M. Coulomb's Memoirs.

19. *On an Improved Method of Dividing Astronomical Instruments.* (*Phil. Trans.* 1809, p. 221.) The merits of this improvement have not been very highly appreciated by those who are in the habit of executing the divisions of circular arcs. It consists in a mode of employing a microscope, with its cross wires, as a substitute for one of the points of a beam compass, while another point draws a faint line on the face of the instrument in the usual manner. The Duke de Chaulnes had before used microscopical sights for dividing circles; but his method more nearly resembled that which has been brought forwards in an improved form by Captain Kater; and Mr Cavendish, by using a single microscope only, seems to have sacrificed some advantages which the other methods appear to possess: but none of them has been very fairly tried; and our artists have hitherto continued to adhere to the modes which they had previously adopted, and which it would perhaps have been difficult for them to abandon, even if they had been convinced of the advantages to be gained by some partial improvements.

Such were the diversified labours of a philosopher, who possessed a clearness of comprehension and an acuteness of reasoning which had been the lot of very few of his predecessors since the days of Newton*. Maclaurin and Waring, perhaps also Stirling and Landen, were incomparably greater mathematicians;

* [As regards Cavendish's dynamical manuscripts, see the end of the second volume of this edition.]

but none of them attempted to employ their powers of investigation in the pursuit of physical discovery: Euler and Lagrange, on the Continent, had carried the improvements of analytical reasoning to an unparalleled extent, and they both, as well as Daniel Bernoulli and d'Alembert, applied these powers with marked success to the solution of a great variety of problems in mechanics and in astronomy; but they made no experimental discoveries of importance: and the splendid career of chemical investigation, which has since been pursued with a degree of success so unprecedented in history, may be said to have been first laid open to mankind by the labours of Mr Cavendish; although the further discoveries of Priestley, Scheele, and Lavoisier, soon furnished, in rapid succession, a superstructure commensurate to the extent of the foundations so happily laid. "Whatever the sciences revealed to Mr Cavendish," says Cuvier, "appeared always to exhibit something of the sublime and the marvellous; he weighed the earth; he rendered the air navigable; he deprived water of the quality of an element"; and he denied to fire the character of a substance. "The clearness of the evidence on which he established his discoveries, so new and so unexpected as they were, is still more astonishing than the facts themselves which he detected; and the works, in which he has made them public, are so many master-pieces of sagacity and of methodical reasoning; each perfect as a whole and in its parts, and leaving nothing for any other hand to correct, but rising in splendour with each successive year that passes over them, and promising to carry down his name to a posterity far more remote than his rank and connections could ever have enabled him to attain without them."

In his manners Mr Cavendish had the appearance of a quickness and sensibility almost morbid, united to a slight hesitation in his speech, which seems to have depended more on the constitution of his mind than on any deficiency of his organic powers, and to an air of timidity and reserve, which sometimes afforded a contrast, almost ludicrous, to the sentiments of profound respect which were professed by those with whom he conversed. It is not impossible that he may have been indebted to his love of severe study, not only for the decided superiority of his faculties to those of the generality of mankind, but even for his exemption from absolute eccentricity of character. His person was tall, and rather thin: his dress was singularly uniform, although sometimes a little neglected. His pursuits were seldom interrupted by indisposition; but he suffered occasionally from calculous complaints. His retired habits of life, and his disregard of popular opinion, appear to have lessened the notoriety which might otherwise have attached to his multiplied successes in science; but his merits were more generally understood on the Continent than in this country; although it was not till he had passed the age of seventy, that he was made one of the eight Foreign Associates of the Institute of France.

Mr Cavendish was no less remarkable in the latter part of his life, for the immense accumulation of his pecuniary property, than for his intellectual and scientific treasures. His father died in 1783, being at that time eighty years old, and the senior member of the Royal Society: but he is said to have succeeded at an earlier period to a considerable inheritance left him by one of his uncles. He principally resided at Clapham Common; but his library was latterly at his house in Bedford Square; and his books were at the command of all men of letters, either personally known to him, or recommended by his friends: indeed the whole arrangement was so impartially methodical, that he never took down a book for his own use, without entering it in the loan book; and after the death of a German gentleman, who had been his librarian, he appointed a day on which he attended in person every week for the accommodation of the few, who thought themselves justified in applying to him for such books as they wished to consult. He was constantly present at the meetings of the Royal Society, as well as at the conversations held at the house of the President; and he dined every Thursday with the club composed of its members. He had little intercourse with general society, or even with his own family, and saw only once a year the person whom he had made his principal heir. He is said to have assisted several young men, whose talents recommended them to his notice, in obtaining establishments in life; but in his later years, such instances were certainly very rare. His tastes and his pleasures do not seem to have been in unison with those which are best adapted to the generality of mankind; and amidst the abundance of all the means of acquiring every earthly enjoyment, he must have wanted that sympathy, which alone is capable of redoubling our delights, by the consciousness that we share them in common with a multitude of our friends, and of enhancing the beauties of all the bright prospects that surround us, when they are still more highly embellished by reflection "from looks that we love." He could have had no limitation either of comfort or of luxury to stimulate him to exertion; even his riches must have deprived him of the gratification of believing, that each new triumph in science might promote the attainment of some great object in life that he earnestly desired; a gratification generally indeed illusory, but which does not cease to beguile us till we become callous as well to the pleasures as to the sorrows of existence. But in the midst of this "painful pre-eminence," he must still have been capable of extending his sensibility over a still wider field of time and space, and of looking forwards to the approbation of the wise and the good of all countries and of all ages: and he must have enjoyed the highest and purest of all intellectual pleasures, arising from the consciousness of his own excellence, and from the certainty that, sooner or later, all mankind must acknowledge his claim to their profoundest respect and highest veneration.

"It was probably either the reserve of his manners," says Cuvier, "or the modest tone of his writings, that procured him the uncommon distinction of never having his repose disturbed either by jealousy or by criticism. Like his great countryman Newton, whom he resembled in so many other respects, he died full of years and honours, beloved even by his rivals, respected by the age which he had enlightened, celebrated throughout the scientific world, and exhibiting to mankind a perfect model of what a man of science ought to be, and a splendid example of that success, which is so eagerly sought, but so seldom obtained." The last words that he uttered were characteristic of his unalterable love of method and subordination: he had ordered his servant to leave him, and not to return till a certain hour, intending to pass his latest moments in the tranquillity of perfect solitude: but the servant's impatience to watch his master diligently having induced him to infringe the order, he was severely reproved for his indiscretion, and took care not to repeat the offence until the scene was finally closed. Mr Cavendish died on the 24th of February, 1810; and was buried in the family vault at Derby. He left a property in the funds of about £700,000, which he divided into six equal parts, giving two to Lord George Cavendish, the son of his first cousin, one to each of his sons, and one to the Earl of Besborough, whose mother was also his first cousin. Some other personal property devolved to Lord George as residuary legatee; and a landed estate of £6,000 a year descended to his only brother, Mr Frederic Cavendish, of Market Street, Herts, a single man, and of habits of life so peculiarly retired, that any further increase of income would have been still more useless to him than it had been to the testator.

Much as Mr Cavendish effected for the promotion of physical science throughout his life, it has not been unusual, even for his warmest admirers, to express some regret that he did not attempt to do still more after his death, by the appropriation of a small share of his immense and neglected wealth, to the perpetual encouragement of those objects, which he had himself pursued with so much ardour. But however we might be disposed to lament such an omission, we have surely no reason to complain of his determination to follow more nearly the ordinary course of distribution of his property, among those whose relationship would have given them a legal claim to the succession, if he had not concerned himself in directing it. We may observe on many other occasions, that the most successful cultivators of science are not always the most strenuous promoters of it in others; as we often see the most ignorant persons, having been rendered sensible by experience of their own deficiencies, somewhat disposed to overrate the value of education, and to bestow more on the improvement of their children than men of profounder learning, who may possibly have felt the insufficiency of their own accomplishments for insuring success in the world. But even if Mr Cavendish had been inclined to devote a large share of his property to the establishment of fellowships or professorships, for the incitement of men of talents to a more complete devotion of their lives to the pursuit of science, it is very doubtful whether he could have entertained a reasonable hope of benefiting his country by such an institution: for the highest motives that stimulate men to exertion are not those which are immediately connected with their pecuniary interests: the senators and the statesmen of Great Britain are only paid in glory; and where we seek to obtain the cooperation of the best educated and the most enlightened individuals in any pursuit or profession, we must hold out as incentives the possession of high celebrity and public respect; assured that they will be incomparably more effectual than any mercenary considerations, which are generally found to determine a crowd of commercial speculators to enter into competition for the proposed rewards, and to abandon all further concern with the objects intended to be pursued, as soon as their avarice is gratified. To raise the rank of science in civil life is therefore most essentially to promote its progress: and when we compare the state, not only of the scientific associations, but also of the learned professions in this country and among our neighbours, we shall feel little reason to regret the total want of pecuniary patronage that is remarkable in Great Britain, with respect to every independent department of letters, while it is so amply compensated by the greater degree of credit and respectability attached to the possession of successful talent. It must not however be denied, that even in this point of view there might be some improvement in the public spirit of the country: Mr Cavendish was indeed neither fond of giving nor of receiving praise; and he was little disposed to enliven the intervals of his serious studies by the promotion of social or convivial cheerfulness: but it would at all times be very easy for an individual, possessed of high rank and ample fortune, of correct taste and elegant manners, to confer so much dignity on science and literature by showing personal testimonies of respect to acknowledged merit, as greatly to excite the laborious student to the unremitting exertions of patient application, and to rouse the man of brilliant talent to the noblest flights of genius.

PAPERS COMMUNICATED BY CAVENDISH TO THE ROYAL SOCIETY AND PUBLISHED IN THE *PHILOSOPHICAL TRANSACTIONS*

ARRANGED IN CHRONOLOGICAL ORDER

[NOTE. Cavendish's published scientific communications are here reproduced exactly as originally printed, except that certain numerical tables have had to be placed somewhat differently in the text owing to the difference in the size of the page of this volume and that of the *Philosophical Transactions*. Obvious errors and necessary typographical corrections have also been indicated.

The responsibility for editing the *Philosophical Transactions* rests with the Secretaries of the Society and their practice, or that of the Printers, has varied from time to time. Thus Cavendish's earlier papers are printed without head-lines to the pages. Accordingly, for the sake of uniformity, in the present reproduction, page head-lihes have been introduced in the case of the papers on *Factitious Air* and on the *Rathbone Place Water*. These are of the same general character as the head-lines appearing on the papers subsequently published.]

PHILOSOPHICAL TRANSACTIONS. VOL. 56, 1766, p. 141

Received May 12, 1766

XIX. *Three Papers, containing Experiments on factitious Air, by the Hon.* Henry Cavendish, *F.R.S.*

Read May 29, Nov. 6 and Nov. 13, 1766

By factitious air, I mean in general any kind of air which is contained in other bodies in an unelastic state, and is produced from thence by art.

By fixed air, I mean that particular species of factitious air, which is separated from alcaline substances by solution in acids or by calcination; and to which Dr. Black has given that name in his treatise on quicklime.

As fixed air makes a considerable part of the subject of the following papers; and as the name might incline one to think, that it signified any sort of air which is contained in other bodies in an unelastic form; I thought it best to give this explanation before I went any farther.

Before I proceed to the experiments themselves, it will be proper to mention the principal methods used in making them.

In order to fill a bottle with the air discharged from metals or alcaline substances by solution in acids, or from animal or vegetable substances by fermentation, I make use of the contrivance represented in TAB. VII. Fig. 1. where A represents the bottle, in which the materials for producing air are placed; having a bent glass tube C ground into it, in the manner of a stopper. E represents a vessel of water. D the bottle to receive the air, which is first filled with water, and then inverted into the vessel of water, over the end of the bent tube. F*f* represents the string, by which the bottle is suspended. When I would measure the quantity of air, which is produced by any of these substances, I commonly do it by receiving the air in a bottle, which has divisions marked on its sides with a diamond, shewing the weight of water, which it requires to fill the bottle up to those divisions: but sometimes I do it by making a mark on the side of the bottle in which I have received the air, answering to the surface of the water therein; and then, setting the [bottle] upright, find how much water it requires to fill it up to that mark.

In order to transfer the air out of one bottle into another, the simplest way, and that which I have oftenest made use of, is that represented in

Fig. 2. where A is the bottle, into which the air is to be transferred: it is supposed to be filled with water and inverted into the vessel of water DEFG, and suspended there by a string: the line DG is the surface of the water: B represents a tin funnel held under the mouth of the bottle: C represents the inverted bottle, out of which the air is to be transferred; the mouth of which is lifted up till the air runs out of it into the funnel, and from thence into the bottle A.

In order to transfer air out of a bottle into a bladder, the contrivance Fig. 3. is made use of. A is the bottle out of which the air is to be transferred, inverted into the vessel of water FGHK: B is a bladder whose neck is tied fast over the hollow piece of wood C*c*, so as to be air-tight. Into the piece of wood is run a bent pewter pipe D, and secured with lute[1]. The air is then pressed out of the bladder as well as possible, and a bit of wax E stuck upon the other end of the pipe, so as to stop up the orifice. The pipe, with the wax upon it, is then run up into the inverted bottle, and the wax torn off by rubbing it against the sides. By this means, the end of the pipe is introduced within the bottle, without suffering any water to get within it. Then, by letting the bottle descend, so as to be totally immersed in the water, the air is forced into the bladder.

The weights used in the following experiments, are troy weights, 1 ounce containing 480 grains. By an ounce or grain measure, I mean such a measure as contains one ounce or grain Troy of water.

EXPERIMENTS ON FACTITIOUS AIR

PART I

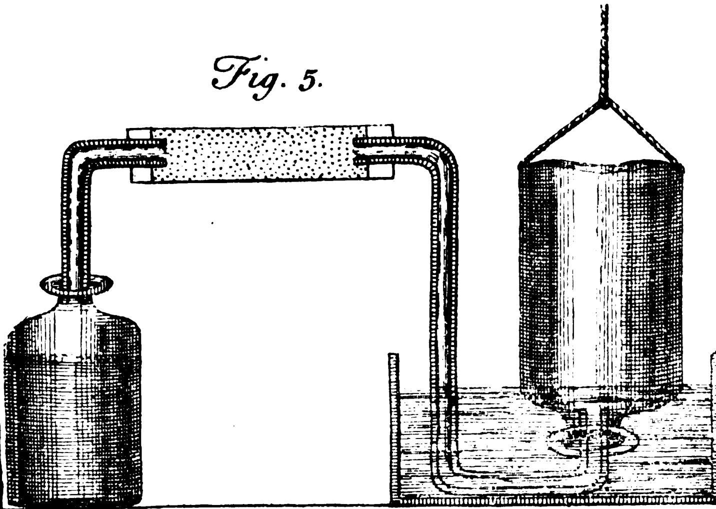
Containing Experiments on Inflammable Air.

I know of only three metallic substances, namely, zinc, iron and tin, that generate inflammable air by solution in acids; and those only by solution in the diluted vitriolic acid, or spirit of salt.

Zinc dissolves with great rapidity in both these acids; and, unless they are very much diluted, generates a considerable heat. One ounce of zinc produces about 356 ounce measures of air: the quantity seems just the same whichsoever of these acids it is dissolved in. Iron dissolves readily

[1] The lute used for this purpose, as well as in all the following experiments, is composed of almond powder, made into a paste with glue, and beat a good deal with a heavy hammer. This is the strongest and most convenient lute I know of. A tube may be cemented with it to the mouth of a bottle, so as not to suffer any air to escape at the joint; though the air within is compressed by the weight of several inches of water.

Fig. 5.



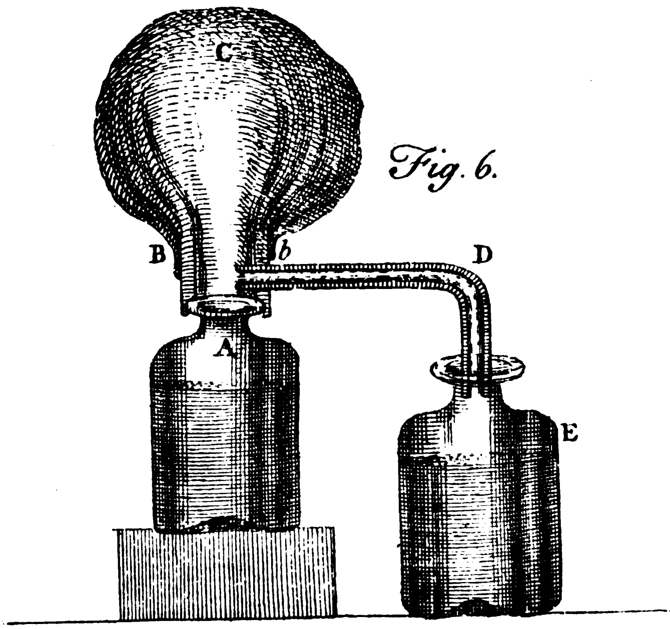


Fig. 6.

in the diluted vitriolic acid, but not near so readily as zinc. One ounce of iron wire produces about 412 ounce measures of air: the quantity was just the same, whether the oil of vitriol was diluted with 1½, or 7 times its weight of water: so that the quantity of air produced seems not at all to depend on the strength of the acid.

Iron dissolves but slowly in spirit of salt while cold: with the assistance of heat it dissolves moderately fast. The air produced thereby is inflammable; but I have not tried how much it produces.

Tin was found to dissolve scarce at all in oil of vitriol diluted with an equal weight of water, while cold: with the assistance of a moderate heat it dissolved slowly, and generated air, which was inflammable: the quantity was not ascertained.

Tin dissolves slowly in strong spirit of salt while cold: with the assistance of heat it dissolves moderately fast. One ounce of tinfoil yields 202 ounce measures of inflammable air.

These experiments were made, when the thermometer was at 50° and the barometer at 30 inches.

All these three metallic substances dissolve readily in the nitrous acid, and generate air; but the air is not at all inflammable. They also unite readily, with the assistance of heat, to the undiluted acid of vitriol; but very little of the salt, formed by their union with the acid, dissolves in the fluid. They all unite to the acid with a considerable effervescence, and discharge plenty of vapours, which smell strongly of the volatile sulphureous acid, and which are not at all inflammable. Iron is not sensibly acted on by this acid, without the assistance of heat; but zinc and tin are in some measure acted on by it, while cold.

It seems likely from hence, that, when either of the above-mentioned metallic substances are dissolved in spirit of salt, or the diluted vitriolic acid, their phlogiston flies off, without having its nature changed by the acid, and forms the inflammable air; but that, when they are dissolved in the nitrous acid, or united by heat to the vitriolic acid, their phlogiston unites to part of the acid used for their solution, and flies off with it in fumes, the phlogiston losing its inflammable property by the union. The volatile sulphureous fumes, produced by uniting these metallic substances by heat to the undiluted vitriolic acid, shew plainly, that in this case their phlogiston unites to the acid; for it is well known, that the vitriolic sulphureous acid consists of the plain vitriolic acid united to phlogiston[1]. It is highly probable too, that the same thing happens in dissolving these metallic substances in the nitrous acid; as the fumes produced during the

[1] Sulphur is allowed by chymists, to consist of the plain vitriolic acid united to phlogiston. The volatile sulphureous acid appears to consist of the same acid united to a less proportion of phlogiston than what is required to form sulphur. A circumstance which I think shews the truth of this, is that if oil of vitriol be distilled, from sulphur, the liquor, which comes over, will be the volatile sulphureous acid.

solution appear plainly to consist in great measure of the nitrous acid, and yet it appears, from their more penetrating smell and other reasons, that the acid must have undergone some change in its nature, which can hardly be attributed to anything else than its union with the phlogiston. As to the inflammable air, produced by dissolving these substances in spirit of salt or the diluted vitriolic acid, there is great reason to think, that it does not contain any of the acid in its composition; not only because it seems to be just the same whichsoever of these acids it is produced by; but also because there is an inflammable air, seemingly much of the same kind as this, produced from animal substances in putrefaction, and from vegetable substances in distillation, as will be shewen hereafter; though there can be no reason to suppose, that this kind of inflammable air owes its production to any acid. I now proceed to the experiments made on inflammable air.

I cannot find that this air has any tendency to lose its elasticity by keeping, or that it is at all absorbed, either by water, or by fixed or volatile alcalies; as I have kept some by me for several weeks in a bottle inverted into a vessel of water, without any sensible decrease of bulk; and as I have also kept some for a few days, in bottles inverted into vessels of sope leys and spirit of sal ammoniac, without perceiving their bulk to be at all diminished.

It has been observed by others, that, when a piece of lighted paper is applied to the mouth of a bottle, containing a mixture of inflammable and common air, the air takes fire, and goes off with an explosion. In order to observe in what manner the effect varies according to the different proportions in which they are mixed, the following experiment was made.

Some of the inflammable air, produced by dissolving zinc in diluted oil of vitriol, was mixed with common air in several different proportions, and the inflammability of these mixtures tried one after the other in this manner. A quart bottle was filled with one of these mixtures, in the manner represented in Fig. 2. The bottle was then taken out of the water, set upright on a table, and the flame of a lamp or piece of lighted paper applied to its mouth. But, in order to prevent the included air from mixing with the outward air, before the flame could be applied, the mouth of the bottle was covered, while under water, with a cap made of a piece of wood covered with a few folds of linnen; which cap was not removed till the instant that the flame was applied. The mixtures were all tried in the same bottle; and, as they were all ready prepared, before the inflammability of any of them was tried, the time elapsed between each trial was but small: by which means I was better able to compare the loudness of the sound in each trial. The result of the experiment is as follows.

With one part of inflammable air to 9 of common air, the mixture would not take fire, on applying the lighted paper to the mouth of the bottle; but, on putting it down into the belly of the bottle, the air took fire, but made very little sound.

With 2 parts of inflammable to 8 of common air, it took fire immediately, on applying the flame to the mouth of the bottle, and went off with a moderately loud noise.

With 3 parts of inflammable air to 7 of common air, there was a very loud noise.

With 4 parts of inflammable to 6 of common air, the sound seemed very little louder.

With equal quantities of inflammable and common air, the sound seemed much the same. In the first of these trials, namely, that with one part of inflammable to 9 of common air, the mixture did not take fire all at once, on putting the lighted paper into the bottle; but one might perceive the flame to spread gradually through the bottle. In the three next trials, though they made an explosion, yet I could not perceive any light within the bottle. In all probability, the flame spread so instantly through the bottle, and was so soon over, that it had not time to make any impression on my eye. In the last mentioned trial, namely, that with equal quantities of inflammable and common air, a light was seen in the bottle, but which quickly ceased.

With 6 parts of inflammable to 4 of common air, the sound was not very loud: the mixture continued burning a short time in the bottle, after the sound was over.

With 7 parts of inflammable to 3 of common air, there was a very gentle bounce or rather puff: it continued burning for some seconds in the belly of the bottle.

A mixture of 8 parts of inflammable to 2 of common air caught fire on applying the flame, but without any noise: it continued burning for some time in the neck of the bottle, and then went out, without the flame ever extending into the belly of the bottle.

It appears from these experiments, that this air, like other inflammable substances, cannot burn without the assistance of common air. It seems too, that, unless the mixture contains more common than inflammable air, the common air therein is not sufficient to consume the whole of the inflammable air; whereby part of the inflammable air remains, and burns by means of the common air, which rushes into the bottle after the explosion.

In order to find whether there was any difference in point of inflammability between the air produced from different metals by different acids, five different sorts of air, namely, 1. Some produced from zinc by diluted oil of vitriol, and which had been kept about a fortnight; 2. Some of the same kind of air fresh made; 3. Air produced from zinc by spirit of salt; 4. Air from iron by the vitriolic acid; 5. Air from tin by spirit of salt; were each mixed separately with common air in the proportion of 2 parts of inflammable air to $7\frac{7}{10}$ of common air, and their inflammability tried in the same bottle, that was used for the former experiment, and with the same precautions. They each went off with a pretty loud noise, and without any difference in the sound that I could be sure of. Some more of each of the above parcels of air were then mixed with common air, in the proportion of 7 parts of inflammable air to $3\frac{1}{5}$ of common air, and tried in the same way as before. They each of them went off with a gentle bounce, and burnt some time in the bottle, without my being able to perceive any difference between them.

In order to avoid being hurt, in case the bottle should burst by the explosion, I have commonly, in making these sort of experiments, made use of an apparatus contrived in such manner, that, by pulling a string, I drew the flame of a lamp over the mouth of the bottle, and at the same time pulled off the cap, while I stood out of the reach of danger. I believe, however, that this precaution is not very necessary; as I have never known a bottle to burst in any of the trials I have made.

The specific gravity of each of the above-mentioned sorts of inflammable air, except the first, was tried in the following manner. A bladder holding about 100 ounce measures was filled with inflammable air, in the manner represented in Fig. 3. and the air pressed out again as perfectly as possible. By this means the small quantity of air remaining in the bladder was almost intirely of the inflammable kind. 80 ounce measures of the inflammable air, produced from zinc by the vitriolic acid, were then forced into the bladder in the same manner: after which, the pewter pipe was taken out of the wooden cap of the bladder, the orifice of the cap stopt up with a bit of lute, and the bladder weighed. A hole was then made in the lute, the air pressed out as perfectly as possible, and the bladder weighed again. It was found to have increased in weight $40\frac{3}{4}$ grains. Therefore the air pressed out of the bladder weighs $40\frac{3}{4}$ grains less than an equal quantity of common air: but the quantity of air pressed out of the bladder must be nearly the same as that which was forced into it, i.e. 80 ounce measures: consequently 80 ounce measures of this sort of inflammable air weigh $40\frac{3}{4}$ grains less than an equal bulk of common air. The three other sorts of inflammable air were then tried in the same way, in the same bladder, immediately one after the other. In the trial with the air from zinc by spirit of salt, the bladder increased $40\frac{1}{2}$ grains on forcing out the air. In the trial with the air from iron, it increased $41\frac{1}{2}$ grains, and in that with the air from tin, it increased 41 grains. The heat of the air, when this experiment was made, was 50°; the barometer stood at $29\frac{3}{4}$ inches.

There seems no reason to imagine, from these experiments, that there is any difference in point of specific gravity between these four sorts of inflammable air; as the small difference observed in these trials is in all probability less than what may arise from the unavoidable errors of the experiment. Taking a medium therefore of the different trials, 80 ounce measures of inflammable air weigh 41 grains less than an equal bulk of common air. Therefore, if the density of common air, at the time when this experiment was tried, was 800 times less than that of water, which, I imagine, must be near the truth[1], inflammable air must be 5490 times lighter than water, or near 7 times lighter than common air. But if the density of common air was 850 times less than that of water, then would inflammable air be 9200 times lighter than water, or $10\frac{8}{10}$ lighter than common air.

This method of finding the density of factitious air is very convenient and sufficiently accurate, where the density of the air to be tried is not much less than that of common air, but cannot be much depended on in the present case, both on account of the uncertainty in the density of common air, and because we cannot be certain but what some common air might be mixed with the inflammable air in the bladder, notwithstanding the precautions used to prevent it; both which causes may produce a considerable error, where the density of the air to be tried is many times less than that of common air. For this reason, I made the following experiments.

I endeavoured to find the weight of the air discharged from a given

[1] Mr. Hawksbee, whose determination is usually followed as the most exact, makes air to be more than 850 times lighter than water; vid. Hawksbee's experiments, p. 94, or Cotes's Hydrostatics, p. 159. But his method of trying the experiment must in all probability make it appear lighter than it really is. For having weighed his bottle under water, both when full of air and when exhausted, he supposes the difference of weight to be equal to the weight of the air exhausted; whereas in reality it is not so much: for the bottle, when exhausted, must necessarily be compressed, and on that account weigh heavier in water than it would otherwise do. Suppose, for example, that air is really 800 times lighter than water, and that the bottle is compressed $\frac{1}{12000}$ part of its bulk; which seems no improbable supposition: the weight of the bottle in water will thereby be increased by $\frac{1}{12000}$ of the weight of a quantity of water of the same bulk, or more than $\frac{1}{15}$ of the weight of the air exhausted: whence the difference of weight will not be so much as $\frac{14}{15}$ of the weight of the air exhausted: and therefore the air will appear lighter than it really is in the proportion of more than 15 to 14, i.e. more than 857 times lighter than water: whereas, if the ball had been weighed in air in both circumstances, the error arising from the compression would have been very trifling.

It appears, from some experiments that have been made by weighing a ball in air, while exhausted, and also after the air was let in, that air, when the thermometer is at 50°, and the barometer at $29\frac{3}{4}$, is about 800 times lighter than water. Though the weight of the air exhausted was little more than 50 grains, no error could well arise near sufficient to make it agree with Hawksbee's experiment. Air seems to expand about $\frac{1}{500}$ part by 1° of heat, whence its density in any other state of the atmosphere is easily determined. The density here assumed agrees very well with the rule given by the gentlemen, who measured the length of a degree in Peru, for finding the height of mountains barometrically, and which is given in the Connoissance des mouvemens celestes, année 1762. To make that rule agree accurately with observation, the density of air, whose heat is the same as that of the places where these observations were made, and which I imagine we may estimate at about 45°, should be 798 times less than that of water, when the barometer stands at $29\frac{3}{4}$.

quantity of zinc by solution in the vitriolic acid, in the manner represented in Fig. 4. A is a bottle filled near full with oil of vitriol diluted with about six times its weight of water: B is a glass tube fitted into its mouth, and secured with lute: C is a glass cylinder fastened on the end of the tube, and secured also with lute. The cylinder has a small hole at its upper end to let the inflammable air escape, and is filled with dry pearl-ashes in coarse powder. The whole apparatus, together with the zinc, which was intended to be put in, and the lute which was to be used in securing the tube to the neck of the bottle, were first weighed carefully; its weight was 11930 grains. The zinc was then put in, and the tube put in its place. By this means, the inflammable air was made to pass through the dry pearl-ashes; whereby it must have been pretty effectually deprived of any acid or watery vapours that could have ascended along with it. The use of the glass tube B was to collect the minute jets of liquor, that were thrown up by the effervescence, and to prevent their touching the pearl-ashes; for which reason, a small space was left between the glass-tube and the pearl-ashes in the cylinder. When the zinc was dissolved, the whole apparatus was weighed again, and was found to have lost $11\frac{3}{4}$ grains in weight[1]; which loss is principally owing to the weight of the inflammable air discharged. But it must be observed, that, before the effervescence, that part of the bottle and cylinder, which was not occupied by other more solid matter, was filled with common air; whereas, after the effervescence, it was filled with inflammable air; so that, upon that account alone, supposing no more inflammable air to be discharged than what was sufficient to fill that space, the weight of the apparatus would have been diminished by the difference of the weight of that quantity of common air and inflammable air. The whole empty space in the bottle and cylinder was about 980 grain measures, there is no need of exactness; and the difference of the weight of that quantity of common and inflammable air is about one grain: therefore the true weight of the inflammable air discharged, is $10\frac{3}{4}$ grains. The quantity of zinc used was 254 grains, and consequently the weight of the air discharged is $\frac{1}{23}$ or $\frac{1}{24}$ of the weight of the zinc.

It was before said, that one grain of zinc yielded 356 grain measures of air: therefore 254 grains of zinc yield 90427 grain measures of air; which we have just found to weigh $10\frac{3}{4}$ grains; therefore inflammable air is about 8410 times lighter than water, or $10\frac{1}{2}$ times lighter than common air.

The quantity of moisture condensed in the pearl-ashes was found to be about $1\frac{1}{4}$ grains.

By another experiment, tried exactly in the same way, the density of inflammable air came out 8300 times less than that of water.

[1] As the quantity of lute used was but small, and as this kind of lute does not lose a great deal of its weight by being kept in a moderately dry room, no sensible error could arise from the drying of the lute during the experiment.

The specific gravity of the air, produced by dissolving zinc in spirit of salt, was tried exactly in the same manner. 244 grains of zinc being dissolved in spirit of salt diluted with about four times its weight of water, the loss in effervescence was $10\frac{3}{4}$ grains; the empty space in the bottle and cylinder was 914 grain measures; whence the weight of the inflammable air was $9\frac{3}{4}$ grains, and consequently its density was 8910 times less than that of water.

By another experiment, its specific gravity came out 9030 times lighter than water.

A like experiment was tried with iron. $250\frac{1}{2}$ grains of iron being dissolved in oil of vitriol diluted with four times its weight of water, the loss in effervescence was 13 grains, the empty space 1420 grain measures. Therefore the weight of the inflammable air was $11\frac{3}{8}$ grains, i.e. about $\frac{1}{22}$ of the weight of the iron, and its density was 8973 times less than that of water. The moisture condensed was $1\frac{1}{4}$ grains.

A like experiment was tried with tin. 607 grains of tinfoil being dissolved in strong spirit of salt, the loss in effervescence was $14\frac{3}{4}$ grains, the empty space 873 grain measures: therefore the weight of the inflammable air was $13\frac{3}{4}$ grains, i.e. $\frac{1}{44}$ of the weight of the tin, and its density 8918 times less than that of water. The quantity of moisture condensed was about three grains.

It is evident, that the truth of these determinations depend[s] on a supposition, that none of the inflammable air is absorbed by the pearl-ashes. In order to see whether this was the case or no, I dissolved 86 grains of zinc in diluted acid of vitriol, and received the air in a measuring bottle in the common way. Immediately after, I dissolved the same quantity of zinc in the same kind of acid, and made the air to pass into the same measuring bottle, through a cylinder filled with dry pearl-ashes, in the manner represented in Fig. 5. I could not perceive any difference in their bulks.

It appears from these experiments, that there is but little, if any, difference in point of density between the different sorts of inflammable air. Whether the difference of density observed between the air procured from zinc, by the vitriolic and that by the marine acid is real, or whether it is only owing to the error of the experiment, I cannot pretend to say. By a medium of the experiments, inflammable air comes out 8760 times lighter than water, or eleven times lighter than common air.

In order to see whether inflammable air, in the state in which it is, when contained in the inverted bottles, where it is in contact with water, contains any considerable quantity of moisture dissolved in it, I forced 192 ounce measures of inflammable air, through a cylinder filled with dry pearl-ashes, by means of the same apparatus, which I used for filling the bladders with inflammable air, and which is represented in Fig. 3. The cylinder was weighed carefully before and after the air was forced through;

whereby it was found to have increased 1 grain in weight. The empty space in the cylinder was 248 grains, the difference of weight of which quantity of common and inflammable air is $\frac{1}{4}$ of a grain. Therefore the real quantity of moisture condensed in the pearl-ashes is $1\frac{1}{4}$ grain. The weight of 192 ounce measures of inflammable air deprived of its moisture appears from the former experiments to be $10\frac{1}{2}$ grains; therefore its weight when saturated with moisture would be $11\frac{3}{4}$ grains. Therefore inflammable air, in that state in which it is in, when kept under the inverted bottles, contains near $\frac{1}{9}$ its weight of moisture; and its specific gravity in that state is 7840 times less than that of water.

I made an experiment with design to see, whether copper produced any inflammable air by solution in spirit of salt. I could not procure any inflammable air thereby: but the phenomena attending it seem remarkable enough to deserve mentioning. The apparatus used for this experiment was of the same kind as that represented in Fig. 1. The bottle A was filled almost full of strong spirit of salt, with some fine copper wire in it. The wire seemed not at all acted on by the acid, while cold; but, with the assistance of a heat almost sufficient to make the acid boil, it made a considerable effervescence, and the air passed through the bent tube, into the bottle D, pretty fast, till the air forced into it by this means seemed almost equal to the empty space in the bent tube and the bottle A: when, on a sudden, without any sensible alteration of the heat, the water rushed violently through the bent tube into the bottle A, and filled it almost intirely full.

The experiment was repeated again in the same manner, except that I took away the bottle D, and let out some of the water of the cistern: so that the end of the bent tube was out of water. As soon as the effervescence began, the vapours issued visibly out of the bent tube; but they were not at all inflammable, as appeared by applying a piece of lighted paper to the end of the tube. A small empty phial was then inverted over the end of the bent tube, so that the mouth of the phial was immersed in the water, the end of the tube being within the body of the phial and out of water. The common air was by degrees expelled out of the phial, and its room occupied by the vapours; after which, having chanced to shake the inverted phial a little, the water suddenly rushed in, and filled it almost full; from thence it passed through the bent tube into the bottle A, and filled it quite full. It appears likely from hence that copper, by solution in the marine acid, produces an elastic fluid, which retains its elasticity as long as there is a barrier of common air between it and the water, but which immediately loses its elasticity, as soon as it comes in contact with the water. In the first experiment, as long as any considerable quantity of common air was left in the bottle containing the copper and acid, the vapours, which passed through the bent tube, must have contained a good deal of common air. As soon therefore as any part of these vapours came to the farther end of the bent tube, where they were in contact with the water, that part of them, which consisted of the air from copper, would be immediately condensed, leaving the common air unchanged; whereby the end of the tube would be filled with common air only; by which means the vapours, contained in the rest of the tube and bottle A, seem to have been defended from the action of the water. But when almost all the common air was driven out of the bottle, then the proportion of common air contained in the vapours, which passed through the tube, seems to have been too small to defend them from the action of the water. In the second experiment, the narrow space left between the neck of the inverted phial and the tube would answer much the same end, in defending the vapours within the inverted phial from the action of the water, as the bent tube in the first experiment did in defending the vapours within the bottle from the action of the water.

EXPERIMENTS ON FACTITIOUS AIR

PART II

Containing Experiments on Fixed Air, or that Species of Factitious Air, which is produced from Alcaline Substances, by Solution in Acids or by Calcination.

EXPERIMENT I

The air produced, by dissolving marble in spirit of salt, was caught in an inverted bottle of water, in the usual manner. In less than a day's time, much the greatest part of the air was found to be absorbed. The water contained in the inverted bottle was found to precipitate the earth from lime-water; a sure sign that it had absorbed fixed air[1].

EXPERIMENT II

I filled a Florence flask in the same way with the same kind of fixed air. When full, I stopt up the mouth of the flask with my finger, while under water, and removed it into a vessel of quicksilver, so that the mouth of the flask was intirely immersed therein. It was kept in this situation upwards of a week. The quicksilver rose and fell in the neck of the flask, according to the alterations of heat and cold, and of the height

[1] Lime, as Dr. Black has shewn, is no more than a calcareous earth rendered soluble in water by being deprived of its fixed air. Lime water is a solution of lime in water: therefore, on mixing lime water with any liquor containing fixed air, the lime absorbs the air, becomes insoluble in water, and is precipitated. This property of water, of absorbing fixed air, and then making a precipitate with lime water, has been taken notice of by Mr. M'Bride.

of the barometer; as it would have done if it had been filled with common air. But it appeared, by comparing together the heights of the quicksilver at the same temper of the atmosphere, that no part of the fixed air had been absorbed or lost its elasticity. The flask was then removed, in the same manner as before, into a vessel of sope leys. The fixed air, by this means, coming in contact with the sope leys, was quickly absorbed.

I also filled another Florence flask with fixed air, and kept it with its mouth immersed in a vessel of quicksilver in the same manner as the other, for upwards of a year, without being able to perceive any air to be absorbed. On removing it into a vessel of sope leys, the air was quickly absorbed like the former.

It appears from this experiment, that fixed air has no disposition to lose its elasticity, unless it meets with water or some other substance proper to absorb it, and that its nature is not altered by keeping.

EXPERIMENT III

In order to find how much fixed air water would absorb, the following experiment was made. A cylindrical glass, with divisions marked on its sides with a diamond, shewing the quantity of water which it required to fill it up to those marks, was filled with quicksilver, and inverted into a glass filled with the same fluid. Some fixed air was then forced into this cylindrical glass, in the same manner that it was into the inverted bottles of water, in the former experiments; except that, to prevent any common air from being forced into the glass along with the fixed, I took care not to introduce the end of the bent tube within the cylindrical glass, till I was well assured that no common air to signify could remain within the bottle. This was done by first introducing the end of the bent tube within an inverted bottle of water, and letting it remain there, till the air driven into this bottle was at least 10 times as much as would fill the empty space in the bent tube, and the bottle containing the marble and acid. By this means one might be well assured, that the quantity of common air remaining within the bent tube and bottle must be very trifling. The end of the bent tube was then introduced within the cylindrical glass, and kept there till a sufficient quantity of fixed air was let up. After letting it stand for a few hours, the division answering to the surface of the quicksilver in the cylinder was observed and wrote down, by which it was known how much fixed air had been let up. A little rain water was then introduced into the cylindrical glass, by pouring some rain water into the vessel of quicksilver, and then lifting up the cylindrical glass so as to raise the bottom of it a little way out of the quicksilver. After having suffered it to stand a day or two, in which time the water seemed to have absorbed as much fixed air as it was able to do, the division answering to the upper surface of the water, and also that answering to the surface of the quicksilver, were observed: by which it was known how much air remained not absorbed, and also how much water had been introduced: the division answering to the surface of the water telling how much air remained not absorbed, and the difference of the two divisions telling how much water had been let up. More water was then let up in the same manner, at different times, till almost the whole of the fixed air was absorbed. As all water contains a little air, the water used in this experiment was first well purged of it by boiling, and then introduced into the cylinder while hot. The result of the experiment is given in the following table; in which the first column shews the bulk of the water let up each time; the second shews the bulk of air absorbed each time; the third the whole bulk of water let up; the fourth the whole bulk of air absorbed; and the fifth column shews the bulk of air remaining not absorbed. In order to set the result in a clearer light, the whole bulk of air introduced into the cylinder is called 1, and the other quantities set down in decimals thereof.

Bulk of air let up = 1.

Bulk of water let up each time	Bulk of air absorbed each time	Whole bulk of water let up	Whole bulk of air absorbed	Whole bulk of air remaining
·322	·374	·322	·374	·626
·481	·485	·803	·859	·141
·082	·048	·885	·907	·093
·145	·079	1·030	·986	·014

I imagine that the quantities of water let up and of the air absorbed could be estimated to about three or four 1000th parts of the whole bulk of air introduced. The height of the thermometer, during the trial of this experiment, was at a medium 55°.

This experiment was tried once before. The result agreed pretty nearly with this; but, as it was not tried so carefully, the result is not set down.

It appears from hence, that the fixed air contained in marble consists of substances of different natures, part of it being more soluble in water than the rest: it appears too, that water, when the thermometer is about 55°, will absorb rather more than an equal bulk of the more soluble part of this air.

It appears, from an experiment which will be mentioned hereafter, that water absorbs more fixed air in cold weather than warm; and, from the following experiment, it appears, that water heated to the boiling point is so far from absorbing air, that it parts with what it has already absorbed.

Experiment IV

Some water, which had absorbed a good deal of fixed air, and which made a considerable precipitate with lime water, was put into a phial, and kept about ¼ of an hour in boiling water. It was found when cold not to make any precipitate, or to become in the least cloudy on mixing it with lime water.

EXPERIMENT V

Water also parts with the fixed air, which it has absorbed by being exposed to the open air. Some of the same parcel of water, that was used for the last experiment, being exposed to the air in a saucer for a few days, was found at the end of that time to make no clouds with lime water.

EXPERIMENT VI

In like manner it was tried how much of the same sort of fixed air was absorbed by spirits of wine. The result is as follows.

Bulk of air introduced = 1.

Spirit let up each time	Air absorbed each time	Whole bulk of spirit let up	Whole bulk of air absorbed	Bulk of air remaining
·207	·453	·207	·453	·547
·146	·274	·353	·727	·273
·074	·103	·427	·830	·170
·046	·030	·473	·860	·140

The mean height of the thermometer, during the trial of the experiment, was 46°. Therefore spirits of wine, at the heat of 46°, absorbs near $2\frac{1}{4}$ times its bulk of the more soluble part of this air.

EXPERIMENT VII

After the same manner it was tried how much fixed air is absorbed by oil. Some olive oil, equal in bulk to $\frac{1}{3}$ part of the fixed air in the cylindrical glass, was let up. It absorbed rather more than an equal bulk of air; the thermometer being between 40 and 50°. The experiment was not carried any farther. The oil was found to absorb the air very slowly.

EXPERIMENT VIII

The specific gravity of fixed air was tried by means of a bladder, in the same manner which was made use of for finding the specific gravity of inflammable air; except that the air, instead of being caught in an inverted bottle of water, and thence transferred into the bladder, was thrown into the bladder immediately from the bottle which contained the marble and spirit of salt, by fastening a glass tube to the wooden cap of the bladder, and luting that to the mouth of the bottle containing the effervescing mixture, in such manner as to be air-tight. The bladder was kept on till it was quite full of fixed air: being then taken off and weighed, it was found to lose 34 grains, by forcing out the air. The bladder was previously found to hold 100 ounce measures. Whence if the outward air, at the time when this experiment was tried, is supposed to have been 800 times lighter than water, fixed air is 511 times lighter than water, or $1\frac{57}{100}$ times heavier than common air. The heat of the air during the trial of this experiment was 45°.

By another experiment of the same kind, made when the thermometer was at 65°, fixed air seemed to be about 563 times lighter than water.

Experiment IX

Fixed air has no power of keeping fire alive, as common air has; but, on the contrary, that property of common air is very much diminished by the mixture of a small quantity of fixed air; as appears from hence.

A small wax candle burnt 80″ in a receiver, which held 190 ounce measures, when filled with common air only.

The same candle burnt 51″ in the same receiver, when filled with a mixture of one part of fixed air to 19 of common air, i.e. when the fixed air was $\frac{1}{20}$ of the whole mixture.

When the fixed air was $\frac{3}{40}$ of the whole mixture, the candle burnt 23″.

When the fixed air was $\frac{1}{10}$ of the whole, it burnt 11″.

When the fixed air was $\frac{6}{55}$ or $\frac{1}{9\frac{1}{6}}$ of the whole mixture, the candle went out immediately.

Hence it should seem, that, when the air contains near $\frac{1}{9}$ its bulk of fixed air, it is unfit for small candles to burn in. Perhaps indeed, if I had used a larger candle and a larger receiver, it might have burnt in a mixture containing a larger proportion of fixed air than this; as I believe that large flaming bodies will burn in a fouler air than small ones. But this is sufficient to shew, that the power, which common air has of keeping fire alive, is very much diminished by a small mixture of fixed air.

This experiment was tried, by setting the candle in a large cistern of water, in such manner that the flame was raised but a little way above the surface; the receiver being inverted full of water into the same cistern. The proper quantity of fixed air was then let up, and the remaining space filled with common air, by raising the receiver gradually out of water; after which, it was immediately whelmed gently over the burning candle.

Experiments on the Quantity of Fixed Air, contained in Alcaline Substances.

Experiment X

The quantity of fixed air contained in marble was found by dissolving some marble in spirit of salt, and finding the loss of weight, which it suffered in effervescence, in the same manner as I found the weight of the inflammable air discharged from metals by solution in acids, except that the cylinder was filled with shreds of filtering paper instead of dry pearl ashes; for pearl ashes would have absorbed the fixed air that passed through them. The weight of the marble dissolved was 311½ grains. The loss of weight in effervescence was 125½ grains. The whole empty space in the bottle and cylinder was about 2700 grain measures: the excess of weight of that quantity of fixed, above an equal quantity of common, air is 1¾ grains. Therefore the weight of the fixed air discharged is 127¼ grains. The cylinder with the filtering paper was found to have increased 1¾ grains in weight during the effervescence. The empty space in the cylinder was about 1160 grain measures: the excess of weight of which quantity of fixed air above an equal bulk of common air is ¾ grains. Therefore the quantity of moisture condensed in the filtering paper is one grain, or about $\frac{1}{125}$ part of the weight of the air discharged.

As water has been already shewn to absorb fixed air, it seemed not improbable, but what there might be some fixed air contained in the solution of marble in spirit of salt; in which case the air discharged, during the effervescence, would not be the whole of the fixed air in the marble. In order to see whether this was the case, I poured some of the solution into lime water. It made scarce any precipitate; which, as the acid was intirely saturated with marble, it would certainly have done if the solution had contained any fixed air. It appears therefore from this experiment, first, that marble contains $\frac{127\frac{1}{4}}{311\frac{1}{2}} = \frac{407}{1000}$ of its weight of fixed air; and secondly, that the quantity of moisture, which flies off along with the fixed air in effervescence, is but trifling; as I imagine that the greatest part of what did fly off must have been condensed in the filtering paper.

By another experiment tried much in the same way, marble was found to contain $\frac{408}{1000}$ of its weight of fixed air.

EXPERIMENT XI

Volatile sal ammoniac dissolves with too great rapidity in acids, and makes too violent an effervescence, to allow one to try what quantity of fixed air it contains in the foregoing manner: I therefore made use of the following method.

Three small phials were weighed together in the same scale. The first contained some weak spirit of salt, the second contained some volatile sal ammoniac in moderate sized lumps without powder, corked up to prevent evaporation, and the third, intended for mixing the acid and alcali in, contained only a little water, and was covered with a paper cap, to prevent the small jets of liquor, which are thrown up during the effervescence, from escaping out of the bottle. In order to prevent too violent an effervescence, the acid and alcali were both added by a little at a time, care being taken that the acid should always predominate in the mixture. Care was also taken always to cover the bottle with the paper cap, as soon as any of the acid or alcali were added. As soon as the mixture was finished, the three phials were weighed again; whereby the loss in effervescence was found to be 134 grains. The weight of the volatile salt made use of was 254 grains, and was pretty exactly sufficient to saturate the acid. The solution appeared, by pouring some of it into lime water, to contain scarce any fixed air. Therefore 254 grains of the volatile sal ammoniac contain 134 grains of fixed air, i.e. $\frac{528}{1000}$ of their weight. It appeared from the same experiment, that 1680 grains of the volatile salt saturate as much acid as 1000 grains of marble.

By another experiment, tried with some of the same parcel of volatile salt, it was found to contain $\frac{538}{1000}$ of its weight of fixed air, and 1643 grains of it saturated as much acid as 1000 grains of marble. By a medium, the salt contained $\frac{533}{1000}$ of its weight of fixed air; and 1661 grains of it saturated as much acid as 1000 grains of marble.

One thousand grains of marble were found to contain 407½ grains of air, and 1661 grains of volatile sal ammoniac contain 885 grains. Therefore this parcel of volatile sal ammoniac contains more fixed air, in proportion to the quantity of acid that it can saturate, than marble does, in the proportion of 885 to 407½, or of 217 to 100.

N.B. It is not unlikely, that the quantity of fixed air may be found to differ considerably in different parcels of volatile sal ammoniac; so that any one, who was to repeat these experiments, ought not to be surprized if he was to find the result to differ considerably from that here laid down. The same thing may be said of pearl ashes.

Experiment XII

This serves to account for a remarkable phenomenon, which I formerly met with, on putting a solution of volatile sal ammoniac in water into a solution of chalk in spirit of salt. The earth was precipitated hereby, as might naturally be expected: but what surprized me, was, that it was attended with a considerable effervescence; though I was well assured, that the acid in the solution of chalk was perfectly neutralized. This is very easily accounted for, from the above-mentioned circumstance of volatile sal ammoniac containing more fixed air in proportion to the quantity of acid that it can saturate, than calcareous earths do. For the volatile alcali, by uniting to the acid, was necessarily deprived of its fixed air. Part of this air united to the calcareous earth, which was at the same time separated from the acid; but, as the earth was not able to absorb the whole of the fixed air, the remainder flew off in an elastic form, and thereby produced an effervescence.

Experiment XIII

The same solution of volatile sal ammoniac made no precipitate, when mixed with a solution of Epsom salt; though a mixture thereof with a little spirit of sal ammoniac, made with lime, immediately precipitated the magnesia from the same solution of Epsom salt; as it ought to do according to Dr. Black's account of the affinity of magnesia and volatile alcalies to acids. This experiment is not so easily accounted for as the last; but I imagine, that the magnesia is really separated from the acid by the volatile alcali; but that it is soluble in water, when united to so great a proportion of fixed air, as is contained in a portion of volatile sal ammoniac, sufficient to saturate the same quantity of acid. The reason, why the mixture of the solution of volatile sal ammoniac, with the spirits of sal ammoniac made with lime, precipitates the magnesia from the Epsom salt, is that, as the spirits made with lime contain no fixed air, the mixture of these spirits with the solution of volatile sal ammoniac contains less air in proportion to the quantity of acid which it can saturate, than the solution of volatile sal ammoniac by itself does.

Volatile sal ammoniac requires a great deal of water to dissolve it, and the solution has not near so strong a smell as the spirits of sal ammoniac made with fixed alcali; the reason of which is, that the latter contain much less fixed air. But volatile sal ammoniac dissolves in considerable quantity in weak spirits of sal ammoniac made with lime, and the solution differs in no respect from the spirits made with fixed alcali. This is a convenient way of procuring the mild spirits of sal ammoniac, as those made with fixed alcali are seldom to be met with in the shops.

Experiment XIV

The quantity of fixed air contained in pearl ashes was tried, by mixing a solution of pearl ashes with diluted oil of vitriol, in the same manner as was used for volatile sal ammoniac. As much of the solution was used as contained $328\frac{1}{4}$ grains of dry pearl ashes. The loss of effervescence was 90 grains. The mixture, which was perfectly neutralized, being then added to a sufficient quantity of lime water, in order to see whether it contained any fixed air, a precipitate was made, which being dried weighed $8\frac{1}{2}$ grains. Therefore, if we suppose this precipitate to contain as much fixed air as an equal weight of marble, which I am well assured cannot differ very considerably from the truth, the fixed air therein is $3\frac{1}{2}$ grains, and consequently the air in $328\frac{1}{4}$ grains of the pearl ashes, is $93\frac{1}{2}$ grains, i.e. $\frac{284}{1000}$ of their weight.

By another experiment tried in the same way, they appeared to contain $\frac{287}{1000}$ of their weight of fixed air.

1558 grains of the pearl ashes were found to saturate as much acid as 1000 grains of marble. Therefore this parcel of pearl ashes contains more air in proportion to the quantity of acid that it can saturate, than marble does, in the proportion of 109 to 100.

Experiment XV

Dr. Black says, that, by exposing a solution of salt of tartar for a long time to the open air, some crystals were formed in it, which seemed to be nothing else than the vegetable alcali united to more than its usual proportion of fixed air. This induced me to try, whether I could not perform the same thing more expeditiously, by furnishing the alcali with fixed air artificially; which I did in the manner represented in Fig. 6: where A represents a wide-mouthed bottle, containing a solution of pearl ashes; B*b* represents a round wooden ring fastened over the mouth of the bottle, and secured with luting; C is a bladder bound tight over the wooden ring. This bladder, being first pressed close together, so as to drive out as much of the included air as possible, was filled with fixed air, by means of the bent tube D; one end of which is fixed into the wooden ring, and the other fastened into the mouth of the bottle E, containing marble and spirit of salt. By this means the fixed air thrown into the bladder mixed with the air in the bottle, and came in contact with the fixed alcali. The fixed air was by degrees absorbed, and crystals were formed on the surface of the fixed alcali, which were thrown to the bottom by shaking the bottle. When the alcali had absorbed as much fixed air as it would readily do, the crystals were taken out and dryed on filtered paper, and the remaining solution evaporated; by which means some more crystals were procured.

N.B. It seemed, as, if not all the air discharged from the marble was of a nature proper to be absorbed by the alcali, but only part of it; for when the alcali had absorbed somewhat more than ½ of the air first thrown into the bladder, it would not absorb any more: but, on pressing the remaining air out of the bladder, and supplying its place with fresh fixed air, a good deal of this new air was absorbed. I cannot, however, speak positively as to this point; as I am not certain whether the apparatus was perfectly airtight[1].

These crystals do not in the least attract the moisture of the air; as I have kept some, during a whole winter, exposed to the air in a room without a fire, without their growing at all moist or increasing in weight.

Being held over the fire in a glass vessel, they did not melt as many salts do, but rather grew white and calcined.

They dissolve in about four times their weight of water when the weather is temperate, and dissolve in greater quantity in hot water than cold.

[1] Pearl ashes deprived of their fixed air, i.e. sope leys, will absorb the whole of the air discharged from marble; as I know by experience. But yet it is not improbable, but that the same alcali, when near saturated with fixed air, may be able to absorb only some particular part of it. For as it has been already shewn, that part of the air discharged from marble is more soluble in water than the rest; so it is not unlikely, but that part of it may have a greater affinity to fixed alcali, and be absorbed by it in greater quantity than the rest.

It was found, by the same method, that was made use of for the volatile sal ammoniac, that these crystals contain $\frac{423}{1000}$ of their weight of fixed air, and that 2035 grains of them saturate as much acid as 1000 grains of marble. Therefore these crystals contain more air in proportion to the quantity of acid they saturate, than marble does, in the ratio of 211 to 100.

EXPERIMENT XVI

As these crystals contain about as much fixed air in proportion to the quantity of acid, that they can saturate, as volatile sal ammoniac does, it was natural to expect, that they should produce the same effects with a solution of Epsom salt, or a solution of chalk in spirit of salt; as those effects seemed owing only to the great quantity of fixed air contained in volatile sal ammoniac. This was found to be the real case: for a solution of these crystals in five times their weight of water, being dropt into a solution of chalk in spirit of salt, the earth was precipitated, and an effervescence was produced. No precipitate was made on dropping some of the same solution into a solution of Epsom salt, though the mixture was kept upwards of twelve hours. But, upon heating this mixture over the fire, a great deal of air was discharged, and the magnesia was precipitated.

EXPERIMENTS ON FACTITIOUS AIR

PART III

Containing Experiments on the Air, produced by Fermentation and Putrefaction.

Mr. M'Bride has already shewn, that vegetable and animal substances yield fixed air by fermentation and putrefaction. The following experiments were made chiefly with a view of seeing, whether they yield any other sort of air besides that.

EXPERIMENT I

The air produced from brown sugar and water, by fermentation, was caught in an inverted bottle of sope leys in the usual manner, and which is represented in Fig. 1. As the weather was too cold to suffer the sugar and water to ferment freely, the bottle containing it was immersed in water, which, by means of a lamp, was kept constantly at about 80° of heat. The quantity of sugar put into the bottle was 931 grains: it was dissolved in about 6½ times its weight of water, and mixed with 100 grains of yeast, by way of ferment. The empty space left in the fermenting bottle and tube together measured 1920 grains. The mixture fermented freely, and generated a great deal of air, which was forced up in bubbles into the inverted bottle, but was absorbed by the sope leys, as fast as it rose up. It frothed greatly; but none of the froth or liquor ran over. In about ten days, the fermentation seeming almost over, the vessels were separated. The bottle with the fermented liquor was found to weigh 412 grains less than it did, before the fermentation began. As none of the liquor ran over, and as little or no moisture condensed within the bent tube, I think one may be well assured, that the loss of weight was owing intirely to the air forced into the inverted bottle; for the matter discharged, during the fermentation, must have consisted either of air, or of some other substance, changed into vapour: if this last was the case, I think it could hardly have failed, but that great part of those vapours must have condensed in the tube. The air remaining unabsorbed in the inverted bottle of sope leys was measured, and was found to be exactly equal to the empty space left in the bent tube and fermenting bottle. It appears therefore, that there is not the least air of any kind discharged from the sugar and water by fermentation, but what is absorbed by the sope leys, and which may therefore be reasonably supposed to be fixed air. It seems also, that no part of the common air left in the fermenting bottle was absorbed by the fermenting mixture, or suffered any change in its nature from thence: for a small phial being filled with one part of this air, and two of inflammable air; the mixture went off with a bounce, on applying a piece of lighted paper to the mouth, with exactly the same appearances, as far as I could perceive, as when the phial was filled with the same quantities of common and inflammable air.

The sugar used in this experiment was moist, and was found to lose $\frac{228}{1000}$ parts of its weight by drying gently before a fire. Therefore the quantity of dry sugar used was 715 grains; and the weight of the air discharged by fermentation appears to be near 412 grains, i.e. near $\frac{57}{100}$ parts of the weight of the dry sugar in the mixture.

The fermented liquor was found to have intirely lost its sweetness; so that the vinous fermentation seemed to be compleated; but it was not grown at all sour.

Experiment II

The air, discharged from apple-juice by fermentation, was tried exactly in the same manner. The quantity set to ferment was 7060 grains, and was mixed with 100 grains of yeast. Some of the same parcel of apple-juice, being evaporated gently to the consistence of a moderately hard extract, was reduced to $\frac{1}{7}$ of its weight; so that the quantity of extract, in the 7060 grains of juice employed, was 1009 grains. The liquor fermented much faster than the sugar and water. The loss of weight during the fermentation was 384 grains. The air remaining unabsorbed in the inverted bottle of sope leys was lost by accident, so that it could not be measured; but, from the space it took up in the inverted bottle, I think I may be certain that it could not much exceed the empty space in the bent tube and fermenting bottle, if it did at all. Therefore there is no reason to think that the apple-juice, any more than the sugar and water, produced any kind of air during the fermentation, except fixed air. It appears too, that the fixed air was near $\frac{381}{1000}$ of the weight of the extract contained in the apple-juice. The fermented liquor was very sour; so that it had gone beyond the vinous fermentation, and made some progress in the acetous fermentation.

In order to compare more exactly the nature of the air produced from sugar by fermentation, with that produced from marble by solution in acids, I made the three following experiments.

EXPERIMENT III

I first tried in what quantity the air from sugar was absorbed by water, and at the same time made a like experiment on the air discharged from marble, by solution in spirit of salt. This was done exactly in the same way as the former experiments of this kind. The result is as follows, beginning with the air from sugar and water.

Air from sugar and water let up = 1000.

Bulk of water let up each time	Bulk of air absorbed each time	Whole bulk of water let up	Whole bulk of air absorbed	Bulk of air remaining	Height of thermometer when observ. was made
375	517	375	517	483	40°
143	164	518	681	319	45
153	164	673	845	154	45
82	103	755	948	52	46
Air from marble let up = 1000.					
391	473	391	473	527	40
143	133	534	606	394	45
284	115	818	811	189	45
194	80	1012	891	109	46

The apparatus used in this experiment was suffered to remain in the same situation till summer, when the thermometer stood at 65°. The bulk of the air from sugar, not absorbed by the water, was then found to be 287; so that the matter had remitted 235 parts of air. The bulk of the air from marble not absorbed, was 194; so that 85 parts were remitted; which is therefore a proof, that water absorbs less fixed air in warm weather than cold.

It appears from this experiment, that the air produced from sugar by fermentation, as well as that discharged from marble by solution in acids, consists of substances of different nature: part being absorbed by water in greater quantity than the rest. But, in general, the air from sugar is absorbed in greater quantity than that from marble.

In forcing the air from sugar into the cylindrical glass, no sensible quantity of moisture was found to condense on the surface of the quicksilver, or sides of the glass; which is a proof that no considerable quantity of any thing except air could fly off from the sugar and water in fermentation.

Experiment IV

The specific gravity of the air produced from sugar was found in the same way as that produced from marble. A bladder holding 102 ounce measures, being filled with this kind of air, lost $29\frac{1}{8}$ grains on forcing out the air, the thermometer standing at 62°, and the barometer at $29\frac{1}{2}$ inches. Whence, supposing the outward air during the trial of this experiment to be 826 times lighter than water, as it should be, according to the supposition made use of in the former parts of this paper, the air from sugar should be 554 times lighter than water. Its density therefore appears to be much the same as that of the air contained in marble; as that air appeared to be 511 times lighter than water, by a trial made when the thermometer was at 45°; and 563 times lighter, by another trial when the thermometer was at 65°.

This air seems also to possess the property of extinguishing flame, in much the same degree as that produced from marble; as appears from the following experiment.

Experiment V

A small wax candle burnt 15″ in a receiver filled with $\frac{1}{10}$ of air from sugar, the rest common air.

In a mixture containing $\frac{6}{55}$ or $\frac{1}{9\frac{1}{6}}$, of air from sugar, the rest common air, the candle went out immediately. When the receiver was filled with common air only, the same candle burnt 72″.

The receiver was the same as that used in the former experiment of this kind, and the experiment tried in the same way, except that the air from sugar was first received in an empty bladder, and thence transferred into the inverted bottles of water, in which it was measured: for the air is produced from the sugar so slowly, that, if it had been received in the inverted bottles immediately, it would have been absorbed almost as fast as it was generated.

It appears from these experiments, that the air produced from sugar by fermentation, and in all probability that from all the other sweet juices of vegetables, is of the same kind as that produced from marble by solution in acids, or at least does not differ more from it than the different parts of that air do from each other, and may therefore justly be called fixed air. I now proceed to the air generated by putrefying animal substances.

EXPERIMENT VI

The air produced from gravy broth by putrefaction, was forced into an inverted bottle of sope leys, in the same way as in the former experiment. The quantity of broth used, was 7640 grains, and was found, by evaporating some of the same to the consistence of a dry extract, to contain 163 grains of solid matter. The fermenting bottle was immersed in water kept constantly to the heat of about 96°. In about two days the fermentation seemed intirely over. The liquor smelt very putrid, and was found to have lost $11\frac{1}{2}$ grains of its weight. The sope leys had acquired a brownish colour from the putrid vapours, and a musty smell. The air forced into the inverted bottle, and not absorbed by the sope leys, measured 6280 grains; the air left in the bent tube and fermenting bottle was 1100 grains; almost all of which must have been forced into the inverted bottles: so that this unabsorbed air is a mixture of about one part of common air and $4\frac{7}{10}$ of factitious air.

This air was found to be inflammable; for a small phial being filled with 109 grain measures of it, and 301 of common air, which comes to the same thing as 90 grains of pure factitious air, and 320 of common air, it took fire on applying a piece of lighted paper, and went off with a gentle bounce, of much the same degree of loudness as when the phial was filled with the last mentioned quantities of inflammable air from zinc and common air. When the phial was filled with 297 grains of this air, and 113 of common air, i.e. with 245 of pure factitious air, and 165 of common air, it went off with a gentle bounce on applying the lighted paper; but I think not so loud as when the phial was filled with the last-mentioned quantities of air from zinc and common air.

5500 grain measures of this air, i.e. 4540 of pure factitious air, and 960 of common air, were forced into a piece of ox-gut furnished with a small brass cock, which I find more convenient for trying the specific gravity of small quantities of air, than a bladder: the gut increased $4\frac{1}{2}$ grains in weight on forcing out the air. A mixture of 4540 grains of air from zinc and 960 of common air being then forced into the same gut, it increased $4\frac{3}{4}$ grains on forcing out the air. So that this factitious air should seem to be rather heavier than air from zinc; but the quantity tried was too small to afford any great degree of certainty.

N.B. The weight of 4540 grain measures of inflammable air, is $\frac{58}{100}$ grains, and the weight of the same quantity of common air is $5\frac{7}{10}$ grains.

On the whole it seems that this sort of inflammable air is nearly of the same kind as that produced from metals. It should seem, however, either to be not exactly the same, or else to be mixed with some air heavier than it, and which has in some degree the property of extinguishing flame, like fixed air.

The weight of the inflammable air discharged from the gravy appears to be about one grain, which is but a small part of the loss of weight which it suffered in putrefaction. Part of the remainder, according to Mr. M'Bride's experiments, must have been fixed air. But the colour and smell, communicated to the sope leys, shew, that it must have discharged some other substance besides fixed and inflammable air.

Raw meat also yields inflammable air by putrefaction, but not in near so great a quantity, in proportion to the loss of weight which it suffers, as gravy does. Four ounces of raw meat mixed with water, and treated in the same manner as the gravy, lost about 100 grains in putrefaction; but it yielded hardly more inflammable air than the gravy. This air seemed of the same kind as the former; but, as the experiments were not tried so exactly, they are not set down.

I endeavoured to collect in the same manner the air discharged from bread and water by fermentation, but I could not get it to ferment, or yield any sensible quantity of air; though I added a little putrid gravy by way of ferment.

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XI. *Experiments on* Rathbone-Place *Water: By the Hon.* Henry Cavendish, *F.R.S.*

Read Feb. 19, 1767

DR. LUCAS has given a short examination of this water in the first part of his treatise of waters. It is the produce of a large spring at the end of Rathbone-Place, and used a few years ago to be raised by an engine for supplying part of the town. The engine is now destroyed; but there is a pump, nearly in the same situation, which yields the same kind of water. It is the water of this pump, which was used in these experiments.

Most waters, though ever so transparent, contain some calcareous earth, which is separated from them by boiling, and which seems to be dissolved in them without being neutralized by any acid, and may therefore not improperly be called their unneutralized earth. The following experiments were made chiefly with a view of enquiring into the cause of the suspension of this earth, for which purpose this water seemed well adapted; as it contains more unneutralized earth than most others.

These experiments were made towards the latter end of September 1765, after a very dry summer; whereby the water was most likely more impregnated with saline and other matters than it usually is.

The water, at the time I used it, looked rather foul to the eye. On exposing some of it for a few days to the open air, a scurf was formed on its surface, which was nothing else but some of the unneutralized earth separated from the water. On dropping into it a solution of corrosive sublimate, it grew cloudy in a few seconds; it quickly became opake, and let fall a sediment. This is a property, which I believe does not take place, in any considerable degree, in most of the London waters.

EXPERIMENT I

494 ounces of this water were distilled in a copper still, till about 150 oz. were drawn off. A good deal of earth was precipitated during the distillation, which being collected and dried, weighed 271 grains. It proved to be entirely a calcareous earth, except a small part, which was

magnesia. This I found in the following manner. A little of this earth, being mixed with spirit of salt, dissolved entirely; which shews it to consist solely of an absorbent earth, but does not shew whether it is a calcareous earth or magnesia. The remainder was saturated with oil of vitriol: a great deal of matter remained undissolved, which, as the earth was shewn to be entirely of the absorbent kind, must have been selenite, or a calcareous earth saturated with the oil of vitriol. The clear liquor strained from off the selenite yielded on evaporation only eighteen grains of solid matter, which proved to be Epsom salt; so that all the earth, except that contained in the eighteen grains of Epsom salt, must have been of the calcareous kind. That contained in the Epsom salt is well known to be magnesia.

The water remaining after distillation, and from which the earth was separated, was evaporated, first in a silver pan, and afterwards in a glass cup, till it was reduced to about three ounces. Not the least earth was precipitated during the evaporation, till it was reduced to a small quantity; there then fell 39 grains, which were entirely selenite: so that all the unneutralized earth in the water was separated during the distillation. The liquor thus evaporated was of a reddish colour, like an infusion of soot.

Many waters contain a good deal of neutral salt composed of the nitrous acid united to a calcareous earth; the most convenient way of ascertaining the quantity of which, is to drop a solution of fixed alcali into the evaporated water, till all the earth is precipitated; whereby this salt is changed into true nitre, and is capable of being crystallized. For this reason, some fixed alcali was dropt into the evaporated water till it made no farther precipitation. The earth precipitated thereby weighed thirty-six grains, and was entirely magnesia. The liquor was then farther evaporated, but no nitre could be made to shoot: being then evaporated to dryness, it weighed 256 grains. It gave not the least signs of containing any nitrous salt, either by putting some of it upon lighted charcoal, or by making a match with a solution of it, but appeared to be a mixture of sea salt and vitriolated tartar, or some other salt composed of the vitriolic acid. As I have heard of no other London water, that has been examined with this view, but what has been found to contain a considerable proportion of nitrous salt, it seems very remarkable that this should be intirely destitute of it. I now proceed to the experiments made on the distilled water.

The distilled water, especially that part of it which came over first, became opake, and let fall a precipitate, on drop[p]ing into it a solution of sugar of lead. It also became opake by the addition of corrosive sublimate, much in the same manner that the plain water did before distillation.

It was found, by dropping into it a little acid of vitriol and committing it to evaporation, to contain a small quantity of volatile alcali; as it left four grains of a brownish salt, which being re-dissolved in water, yielded a smell of volatile alcali on the addition of lime. It is doubtless this volatile alcali, which is the cause of the precipitate, which the distilled water makes with sugar of lead and corrosive sublimate.

What first suggested to me that the distilled water contained a volatile alcali, was the distilling some of it over again in a retort; whereby the first runnings were so much impregnated with volatile alcali, as to turn paper died with the juice of blue flowers, to a green colour, and in some measure to yield a smell of volatile alcali.

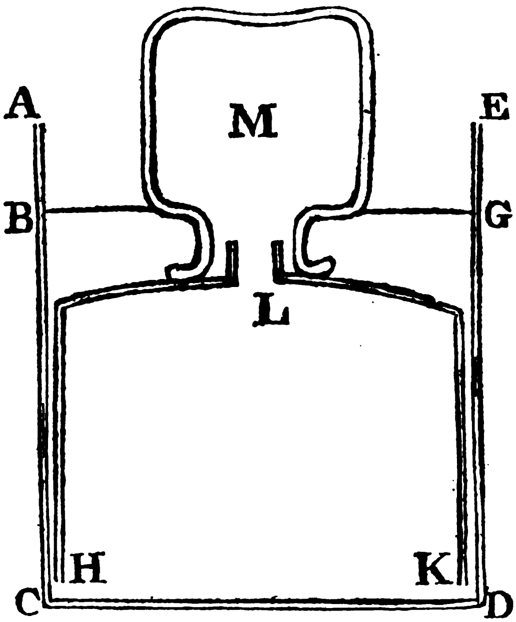
In the foregoing experiment, the salt procured from the distilled water was perfectly neutral; so that the quantity of acid employed was certainly not more than sufficient to saturate the alcali, but it may very likely have been less; as in that case the superfluous volatile alcali would have flown off in the evaporation. The following experiment shews pretty nearly the quantity of volatile alcali in the distilled water.

Experiment II

1128 ounces of Rathbone-place water were distilled in the same manner as the former. The distilled water was divided into two parcels, that parcel which came over first weighing 121 ounces, the other 146. A preparatory experiment was first made, in order to form a judgement of the comparative strength of each parcel, and also of the quantity of acid which it would require to saturate them. This was done by dropping sugar of lead into each parcel till it ceased to make a precipitate. It was judged from hence that the first parcel contained about 2½ times as much volatile alcali as an equal quantity of the second. Into 30 ounces of the first parcel, mixed with as much of the second, was then put 43 grains of oil of vitriol, which was supposed to be about ½ more than sufficient to saturate the alcali therein. The mixture was then evaporated. When reduced to a small quantity, it was found to be rather acid: sixteen grains of volatile sal ammoniac were therefore added, which seemed nearly sufficient to neutralize it. Being then evaporated to dryness, it left sixty-six grains of a brownish salt, which dissolved readily in water, leaving only a trifling quantity of brown sediment. A little of this salt was found to make no precipitate on the addition of fixed alcali, and the remainder, being boiled with lime, was converted into selenite; a sure sign that the salt was merely vitriolic ammoniacal salt. The volatile alcaline salt contained in sixty-six grains of vitriolic ammoniacal salt is 58½ grains; from whence deducting sixteen grains, the weight of the volatile sal ammoniac added, it appears that the distilled water used in this experiment contains 42½ grains of volatile salt; and therefore the whole quantity of volatile salt driven over by distillation seems to be about sixty-eight grains, which, as the second parcel was so much weaker than the first, is probably nearly the whole volatile alkali contained in the water.

EXPERIMENT III

Dr. Brownrigg, in a paper printed in the Philosophical Transactions, for the year 1765, shews that a great deal of fixed air is contained in Spa water. This induced me to try whether I could not find any in that of Rathbone-place; which I did by means of the contrivance represented in the drawing.



ACDE represents a tin pan, filled with Rathbone-place water as high as BG. HKL is another tin pan, within the first, in the manner of an inverted funnel, and made in such a manner as to leave as little room as possible between that and the sides of the outward vessel. M represents a bottle, full of the same water, inverted over the mouth of the funnel. By this means, as fast as the air is disengaged by heat from the water within the funnel, it must necessarily rise up into the bottle. The Rathbone-place water, put into the vessel, weighed 411 ounces, the funnel held 353 ounces. A bottle full of water being inverted over the mouth of the funnel, as in the figure, the water was heated, and kept boiling about ¼ of an hour. As soon as one bottle was filled with air, it was removed by putting a small ladle under its mouth, while under water, and set with its mouth immersed in the same manner in another vessel of water, taking care not to suffer any communication between the included air and the outward air during the removal. At the same time, another bottle full of water was inverted over the mouth of the funnel, in the same manner as the former. It was not easy telling how much air was discharged from the water; as the air in the bottles, when first removed, was hot and expanded; and, before I could be sure it was cold, there was some of it absorbed by the water: but there seemed to be above 75 ounce measures discharged, scarce twenty of which arose before the water began to boil. The water continued discharging air after the experiment was discontinued. In about a day's time, much the greatest part of the air was absorbed, scarce sixteen ounce measures remaining. That which was absorbed appeared to be fixed air, as the water which had absorbed it made a precipitate with lime-water. But, in order to absorb all the fixed air more perfectly, the air which remained not absorbed was transferred into another bottle of water, in the manner described in my first paper on factitious air, page 142 of the preceding volume[1]. This bottle was then set with its mouth immersed in a bottle of sope-leys; after which, by shaking the bottle, the sope-leys was mixed with the included water; whereby the air in the bottle was brought in contact with the sope-leys, which is well-known to absorb fixed air very readily. By this means the air was reduced to 8¾ ounce measures. A small vial being filled with equal quantities of this and inflammable air, and a piece of lighted paper applied to its mouth, it went off with as loud a bounce, as when the same vial was filled with equal quantities of common air and inflammable air. The specific gravity of the remainder was tried by a bladder, in the manner described in the above-mentioned paper; as well as could be judged from so small a quantity, it was just the same as that of common air. From these two circumstances, I think we may fairly conclude that this unabsorbed part was intirely common air; consequently the air discharged from the Rathbone-place water consisted of 8¾ ounces of common air and about 66 of fixed air. The air which was discharged before the water began to boil contained much more common air, than that which was discharged afterwards; that which was discharged towards the latter end seeming to containing scarce any but fixed air.

As so much fixed air is discharged from this water by boiling, it seemed reasonable to suppose, that the distilled water should contain fixed air. I accordingly found it to make a precipitate with lime-water.

EXPERIMENT IV

The following experiment shews that the fixed air was not generated during the boiling, but was contained in the water before. Into 30 ounces of Rathbone-place water was poured some lime-water, which immediately made a precipitate. More lime-water was added, till it ceased to make any farther precipitate. It required 20¼ ounces. The precipitated earth being dried weighed 39 grains.

The unneutralized earth contained in 30 ounces of Rathbone-place water is 16½ grains, and the earth contained in 20¼ ounces of lime-water

[1] See p. 78 of this volume.

(as was found by precipitating the earth by volatile sal ammoniac) is 21 grains. Therefore the earth precipitated from the mixture of Rathbone-place water, and lime-water, is about equal to the sum of the weights, of the earth contained in the lime-water, and of the unneutralized earth in the Rathbone-place water; and consequently all the unneutralized earth seems to be precipitated from Rathbone-place water by the addition of a proper quantity of lime-water. But a more convincing proof that this is the case, is that the clear liquor, after the precipitate had subsided, did not deposit any earth on boiling, or become in the least cloudy on the addition of fixed alkali; whereas Rathbone-place water in its natural state becomes opake thereby. It might perhaps be expected, that the clear liquor should still make a precipitate on the addition of fixed alcali, though the unneutralized earth is precipitated; as in all probability there is still a good deal of earth remaining in it in a neutralized state. The reason why it does not, seems to be, that the remaining earth is most likely intirely magnesia; and Epsom salt, when dissolved in a great quantity of water, does not make any precipitate on the addition of fixed alcali.

There is great reason to suppose that the earth precipitated on mixing the Rathbone-place water and lime-water, was very nearly saturated with fixed air, i.e. that it contained very near as much fixed air, as is naturally contained in the same quantity of calcareous earth. If so, 30 ounces of Rathbone-place water contain as much fixed air as 39 grains of calcareous earth; whereas the unneutralized earth, in that quantity of water, is only 16½ grains; so that Rathbone-place water contains near 2⅓ times as much fixed air as is sufficient to saturate the unneutralized earth in it.

It seems likely from hence, that the suspension of the earth in the Rathbone-place water, is owing merely to its being united to more than its natural proportion of fixed air; as we have shewn that this earth is actually united to more than double its natural proportion of fixed air, and also that it is immediately precipitated, either by driving off the superfluous fixed air by heat, or absorbing it by the addition of a proper quantity of lime water.

Calcareous earths, in their natural state, i.e. saturated with fixed air, are totally insoluble in water; but the same earths, entirely deprived of their fixed air, i.e. converted into lime, are in some measure soluble in it; for lime-water is nothing more than a solution of a small quantity of lime in water. It is very remarkable, therefore, that calcareous earths should also be rendered soluble in water, by furnishing them with more than their natural proportion of fixed air, i.e. that they should be rendered soluble, both by depriving them of their fixed air, and by furnishing them with more than their natural quantity of it. Yet, strange as this may appear, the following experiments, I think, shew plainly that it is the real case.

EXPERIMENT V

In order to see whether I could suspend a calcareous earth in water, by furnishing it with more than its natural proportion of fixed air, I took 30 ounces of rain water, and divided it into two parts: into one part I put as much spirit of salt, as would dissolve $30\frac{3}{10}$ grains of calcareous earth, and as much of a saturated solution of chalk, in spirit of salt, as contained 20 grains of calcareous earth: into the other part I put as much fixed alcali, as was equivalent to $46\frac{8}{10}$ grains of calcareous earth, i.e. which would saturate as much acid. This alcali was known to contain as much fixed air as 39 grains of calcareous earth. The whole was then mixed together and the bottle immediately stopped. The alcali was before said to be equivalent to $46\frac{8}{10}$ grains of calcareous earth, and was, therefore, sufficient to saturate all the spirit of salt, and also to decompound as much of the solution of chalk as contains $16\frac{1}{2}$ grains of earth. This mixture, therefore, supposing I made no mistake in my calculation, contained $16\frac{1}{2}$ grains of unneutralized earth, with as much fixed air as is contained in 39 grains of calcareous earth; which is the quantity which was found to be in the same quantity of Rathbone-place water. The mixture became turbid on first mixing, but the earth was quickly re-dissolved on shaking, so that the liquor became almost transparent. After standing some time, a slight sediment fell to the bottom, leaving the liquor perfectly transparent. The mixture was kept three or four days stopped up, during which time it remained perfectly clear, without depositing any more sediment. The clear liquor was then poured off from the sediment, and boiled for a few minutes, in a Florence flask; it grew turbid before it began to boil, and discharged a good deal of air; some earth was precipitated during boiling, which being dried weighed 13 grains.

This shews that there was really, at least 13 grains of earth suspended in this mixture, without being neutralized by any acid; the suspension of which could be owing only to its being united to more than its natural proportion of fixed air. But, as a further proof of this, I made the following experiment.

EXPERIMENT VI

I took the same quantities of rain water, solution of chalk, spirit of salt, and fixed alcali, as in the last experiment, but mixed them in a different order. The fixed alcali was first dropped into the spirit of salt, and when the effervescence was over, was diluted with $\frac{1}{2}$ the rain water. The solution of chalk was then diluted with the remainder of the rain water, the whole mixed together, and the bottle immediately stoppered, and shook vehemently. A precipitate was immediately formed on mixing, which could not be re-dissolved on shaking.

It must be observed, that, in the first of the two foregoing experiments, all the fixed air contained in the alcali was retained in the mixture, none being lost by effervescence; whereas, in the last experiment, the greatest part of the fixed air was dissipated in the effervescence; no more being retained than what was contained in that portion of the fixed alcali, which was not neutralized by the acid; and consequently the unneutralized earth, in the mixture, contained not much more fixed air than what was sufficient to saturate it. As the latter of these mixtures differed no otherwise from the former, than that it contained less fixed air; the suspension of the earth in the former must necessarily be owing to the fixed air.

In the two foregoing experiments the water contained, besides the unneutralized earth, and fixed air, some sal sylvii, and a little solution of chalk in the marine acid; which, it may be supposed, contributed to the suspension of the earth: but the following experiment shews that a calcareous earth may be suspended in water, without the addition of any other substance than fixed air.

Experiment VII

A bottle full of rain water was inverted into a vessel of rain water, and some fixed air forced up into the bottle, at different times, till the water had absorbed as much fixed air as it would readily do; 11 ounces of this water were mixed with 6½ of lime water. The mixture became turbid on first mixing, but quickly recovered its transparency, on shaking, and has remained so for upwards for a year.

This mixture contains 7 grains of calcareous earth; and, from a subsequent experiment, I guess it to contain as much fixed air, as there is in 14 grains of calcareous earth.

Experiment VIII

Least it should be supposed, that the reason why the earth was not precipitated in the foregoing experiment, was, that it was not furnished with a sufficient quantity of fixed air, the following mixture was made, which contains the same proportion of earth as the former, but a less proportion of fixed air: 4¾ ounces of the above-mentioned water, containing fixed air, were diluted with 6¼ of rain water, and then mixed with 6½ ounces of limewater. A precipitate was immediately made on mixing, which could not be re-dissolved on shaking.

Experiment IX

I made some experiments to find whether the unneutralized earth could be precipitated from other London waters, by the addition of lime water, as well as from Rathbone-place water. It is necessary for this purpose, that the quantity of lime water should be adjusted very exactly; for, if it is too little, it does not precipitate all the unneutralized earth; if it is too great, some of the earth in the lime water remains suspended. For this reason, as I found it almost impossible to adjust the quantity with sufficient exactness, I added such a quantity of lime water, as I was well assured, was more than sufficient to precipitate the whole of the unneutralized earth; and when the precipitate was subsided, decanted off the clear liquor, and exposed it to the open air, till all the lime remaining in the water was precipitated, by attracting fixed air from the atmosphere. The clear liquor was then decanted and evaporated, which is much the most exact way I know of seeing whether any unneutralized earth remains suspended in the water. The result of the experiments was as follows:

200 ounces of water, from a pump in Marlborough-street, were mixed with 38 ounces of lime water. The earth precipitated thereby weighed 38 grains. The clear liquor, exposed to the air, and evaporated in a silver pan till it was reduced to 6 or 7 ounces, deposited no more than 2 or 3 grains of unneutralized earth.

A like quantity of the same pump water, evaporated by itself without the addition of lime water, deposited about 19 grains of unneutralized earth.

200 ounces of water, from a pump in Hanover-square, being mixed with 67 ounces of lime water, the precipitate weighed 93 grains. The clear liquor, treated in the same way as the former, deposited about 2 grains of earth. 200 ounces of the same water, evaporated by itself, deposited 28 grains of earth.

The same quantity of water from a pump in St. Martin's church-yard, being mixed with 82 ounces of lime water, the precipitate weighed 108 grains. The clear liquor deposited scarce any unneutralized earth on evaporation.

The same quantity of water, evaporated by itself, yielded 45 grains of unneutralized earth.

The way, by which I found the quantity of unneutralized earth deposited on evaporation, was, after having decanted the clear liquor, and washed the residuum with rain water, to pour a little spirit of salt into the silver pan, which dissolves all the calcareous earth, but does not corrode the silver. Then, having separated the solution from the insoluble matter, the earth was precipitated by fixed alcali.

In this way of finding the quantity of unneutralized earth, care must be taken to add very little more acid than is necessary to dissolve the unneutralized earth, and to use as little water in washing out the solution as possible; for otherwise a good deal of the selenite, which is deposited in the evaporation of most water[s], will be dissolved; the earth of which will be precipitated by the fixed alcali, and by that means make the quantity of unneutralized earth appear greater than it really is.

It appears from these experiments, that the unneutralized earth is intirely precipitated from these three waters, by the addition of a proper quantity of lime water; as the trifling quantity found to be deposited, on the evaporation of two of them, most likely proceeded only from not exposing the water to the air, long enough for all the lime to be precipitated. So that I think it seems reasonable to conclude, that the unneutralized earth, in all waters, is suspended merely by being united to more than its natural proportion of fixed air.

To return to Rathbone-place water; it appears from the foregoing experiments, that one pint of it, or 7315 grains, contains, first, as much volatile alcali as is equivalent to about $\frac{9}{10}$ grains of volatile sal ammoniac: secondly, $8\frac{4}{10}$ grains of unneutralized earth, a very small part of which is magnesia, the rest a calcareous earth: thirdly, as much fixed air, including that in the unneutralized earth, as is contained in $19\frac{8}{10}$ grains of calcareous earth: fourthly, $1\frac{2}{10}$ of selenite: fifthly, $7\frac{9}{10}$ of a mixture of sea salt, and Epsom salt; and the whole solid contents of 1 pint of the water is $17\frac{1}{2}$ grains.

One pint of water,. from the pump in Marlborough-street, contains $1\frac{4}{10}$ grains of unneutralized earth, and as much fixed air as is contained in $2\frac{9}{10}$ grains of calcareous earth.

The same quantity of water, from the pump in Hanover-square, contains $2\frac{1}{10}$ grains of unneutralized earth, with as much fixed air as is contained in $7\frac{2}{10}$ of earth.

The same quantity of water, from St. Martin's Church-yard, contains $3\frac{4}{10}$ grains of unneutralized earth, with as much fixed air as is contained in $8\frac{2}{10}$ of earth.

PHILOSOPHICAL TRANSACTIONS. VOL. 66, 1776, p. 375

XXI. *An Account of the Meteorological Instruments used at the* Royal Society's *House. By the Hon.* Henry Cavendish, *F.R.S.*

Read March 14th, 1776

Of the thermometers, with reflections concerning some precautions necessary to be used in making experiments with those instruments, and in adjusting their fixed points.

THE thermometers are both adjusted to Fahrenheit's scale: that without doors is placed out of a two-pair-of-stairs window, looking to the North, and stands about two or three inches from the wall, that it may be the more exposed to the air, and the less affected by the heat and cold of the house. The situation is tolerably airy, as neither the buildings opposite to it, nor those on each side, are elevated above it in an angle of more than 12°; but as the opposite building is only twenty-five feet distant, perhaps the heat may be a little increased at the time of the afternoon observation by the reflection from thence. In the middle of summer the Sun shines on the wall of the house, against which the thermometer is fixed, for an hour or two before the morning observation, but never shines on the thermometer itself, or that part of the wall close to it, except in the afternoon, long after the time of observing. On the whole, the situation is not altogether such as could be wished, but is the best the house afforded.

The thermometer within doors is intended chiefly for correcting the heights of the barometer, and is therefore placed close to it. The room in which it is kept looks to the North, and has sometimes a fire in it, but not often.

It has been too common a custom, both in making experiments with thermometers and in adjusting their fixed points, to pay no regard to the heat of that part of the quicksilver which is contained in the tube, though this is a circumstance which ought by no means to be disregarded; for a thermometer, dipped into a liquor of the heat of boiling water, will stand at least 2° higher, if it is immersed to such a depth that the quicksilver in the tube is heated to the same degree as that in the ball, than if it is immersed no lower than the freezing point, and the rest of the tube is not much warmer than the air. The only accurate method is, to take care that all parts of the quicksilver should be heated equally. For this reason, in trying the heat of liquors much hotter or colder than the air, the thermometer ought, if possible, to be immersed almost as far as to the top of the column of quicksilver in the tube. As this, however, would frequently be attended with great inconvenience, the observer will often be obliged to content himself with immersing it to a much less depth; but then, as the quicksilver in a great part of the tube will be of a different heat from that in the ball, it will be necessary to apply a correction on that account to the heat shewn by the thermometer; to facilitate which the following table is given, in which the upper horizontal line is the

Diff. of Heat	Degrees not immersed in the liquors							
	50	100	150	200	250	300	350	400
50	,2	,4	,7	,9	1,1	1,3	1,5	1,7
100	,4	,9	1,3	1,8	2,2	2,6	3,0	3,5
150	,7	1,3	2,0	2,6	3,3	3,8	4,6	5,2
200	,9	1,8	2,6	3,5	4,4	5,1	6,1	7,0
250	1,1	2,2	3,3	4,4	5,5	6,4	7,6	8,7
300	1,3	2,6	3,8	5,1	6,4	7,7	9,1	10
350	1,5	3,0	4,6	6,1	7,6	9,1	11	12
400	1,7	3,5	5,2	7,0	8,7	10	12	14
450	2	3,9	5,9	7,8	9,8	12	14	16
500	2,2	4,4	6,5	8,7	11	13	15	17
550	2,4	4,8	7,2	9,6	12	14	17	19

	450	500	550	600	650	700	750
50	2	2,2	2,4	2,6	2,8	3,1	3,3
100	3,9	4,4	4,8	5,2	5,7	6,1	6,6
150	5,9	6,5	7,2	7,9	8,4	9,2	9,8
200	7,8	8,7	9,6	10	11	12	13
250	9,8	11	12	13	14	15	16
300	12	13	14	16	17	18	20
350	14	15	17	18	20	21	23
400	16	17	19	21	23	24	26
450	18	20	22	24	25	27	29
500	20	22	24	26	28	31	33
550	22	24	26	29	31	34	36

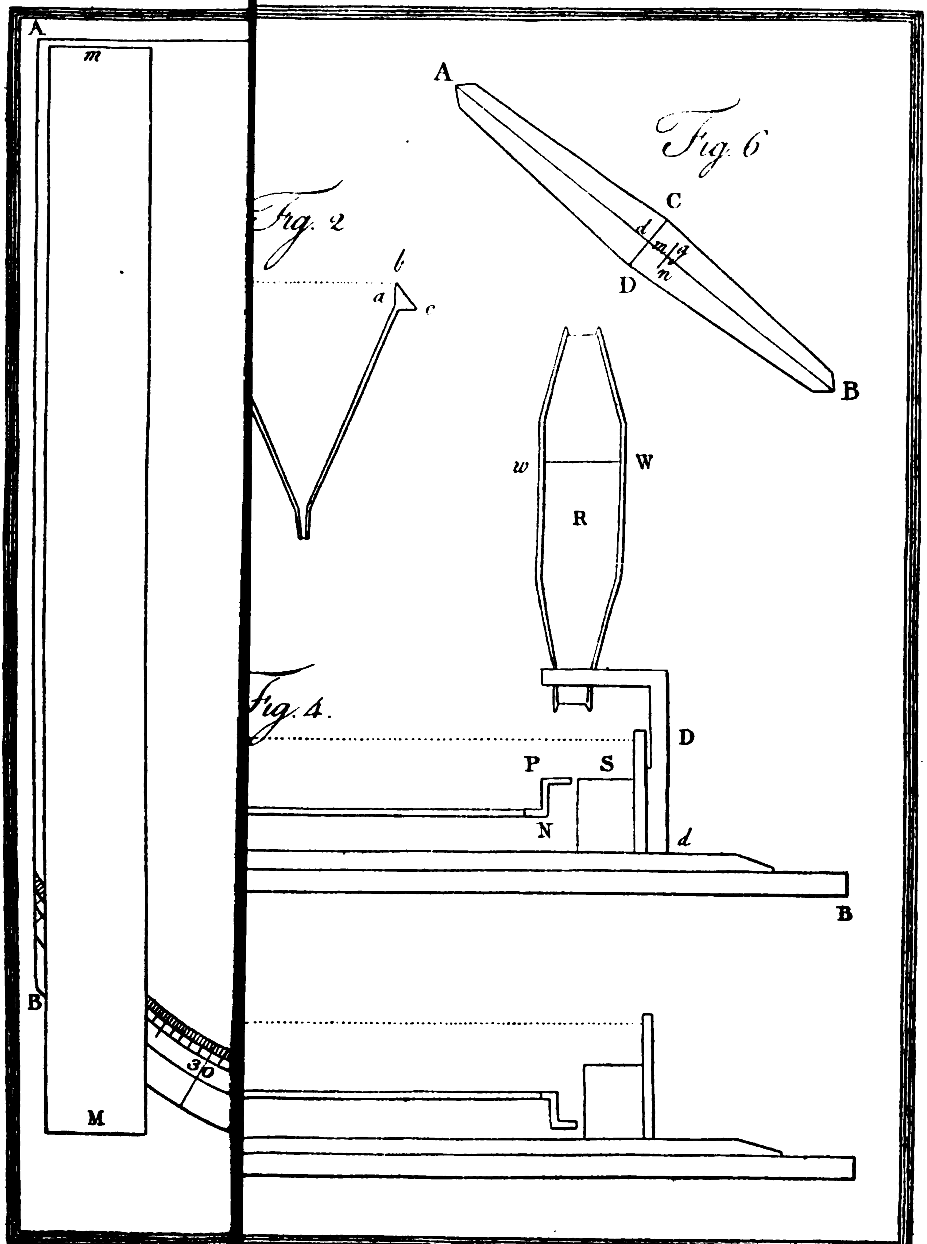
length of the column of quicksilver contained in that part of the tube which is not immersed in the liquor expressed in degrees; the first perpendicular column is the supposed difference of heat of the quicksilver in that part of the tube and in the ball; and the corresponding numbers in the table shew how much higher or lower the thermometer stands than it ought to do. The foundation on which the table is computed is, that quicksilver expands one 11500th part of its bulk by each degree of heat.

But as the generality of observers will be apt to neglect this correction, it would be proper to form two sets of divisions on such thermometers as are intended for trying the heat of liquors; one of which should be used when the tube is immersed almost to the top of the column of quicksilver; and the other, when not much more than the ball is immersed; in which last case the observer should be careful, that the tube should be as little heated by the steam of the liquor as possible. It must be observed, however, that the heat of the liquor may be estimated with much more accuracy by the first set of divisions, with the help of the correction, than it can by the second set, as the latter method is just only in one particular heat of the atmosphere, namely, that to which the divisions are adapted; but, if they are adapted to the mean heat of the climate for which the thermometer is intended, the error can never be very great, and, when the liquor is much hotter or colder than the air of that climate ever is, will be much less than if the first set of divisions were used without any correction; but, when the liquor is within the limits of the heat of the atmosphere, greater accuracy will sometimes be obtained by using the first set of divisions than the second, for which reason the latter set should not be continued within those limits. I would willingly have given rules for the construction of this second set of divisions, but am obliged to omit it, as it cannot be done properly without first determining, by experiment, how much the quicksilver in the tube is heated by immersing the ball in hot liquors.

In a spirit thermometer, the error proceeding from the fluid in the tube being not of the same heat as that in the ball, is much greater; as spirits of wine expand much more by heat than quicksilver: for which reason spirit thermometers are not so proper for trying the heat of liquors as those of quicksilver.

Another circumstance which ought to be attended to in adjusting the boiling point of a thermometer is, that the ball should not be immersed deep in the water; for, if it is, the fluid which surrounds it will be compressed by considerably more than the weight of the atmosphere, and will therefore acquire a sensibly greater heat than it would otherwise do. The most convenient vessel I know for adjusting the boiling point is represented in Fig. 1. *ABCD* is the vessel; *AB* the cover, made to take on and off readily; *E* a chimney to carry off the steam; *FG* the thermometer, passed through a hole *Mm* in the cover, and resting in a little bag fastened to the wire *HK*, intended to prevent the ball from being broken by accidentally falling to the bottom. This wire is made so as to be raised higher or lower at pleasure, and must be placed at such a height that the boiling point shall rise very little above the cover. The hole *Mm* is stopped with bits of cork or tow. By this means, as the tube is inclosed in a

PLATE II



vessel intirely filled with the steam of boiling water, the quicksilver in it is heated to the same degree as that in the ball; and besides, that part of the tube, on which the boiling point is to be placed, is defended from the vapour, so that it is easy making a mark on the glass with ink. If such a vessel as this is used, the thermometer will be found to stand not sensibly higher when the water boils vehemently than when it boils gently; and if the mouth of the chimney is covered by any light body, in such manner as to leave no more passage for the steam than what is necessary to prevent the body from being blown off by the pressure of the included vapour, the thermometer will stand only half or three quarters of a degree higher, if the ball is immersed a little way in the water, than if it is exposed only to the steam. But if the covering of the chimney is removed, the thermometer will immediately sink several degrees, when the ball is exposed only to the steam, at least if the cover does not fit close; whereas when the ball is immersed in the water, the removal of the covering has scarce any effect upon it. Whence it appears, that the steam of water boiling in a vessel, from which the air is perfectly excluded, is a little but not much cooler than the water itself, but is considerably so if the air has the least admission to the vessel. Perhaps a still more convenient method of adjusting the boiling point would be not to immerse the ball in the water at all, but to expose it only to the steam, as thereby the trouble of keeping the water in the vessel to the right depth would be avoided; and besides, several thermometers might be adjusted at the same time, which cannot be done with proper accuracy when they are immersed in the water, unless the distance of the boiling point from the ball is nearly the same in all of them. At present there is so little uniformity observed in the manner of adjusting thermometers, that the boiling points, in instruments made by our best artists, differ from one another by not less than $2\frac{1}{2}°$; owing partly to a difference in the height of the barometer at which they were adjusted, and partly to the quicksilver in the tube being more heated in the method used by some persons than in that used by others. It is very much to be wished, therefore, that some means were used to establish an uniform method of proceeding; and there are none which seem more proper, or more likely to be effectual, than that the Royal Society should take it into consideration, and recommend that method of proceeding which shall appear to them to be most expedient.

Of the barometer, rain-gage, wind, and hygrometer.

The barometer is of the cistern kind, and the height of the quicksilver is estimated by the top of its convex surface, and not by the edge where it touches the glass, the index being properly adapted for that purpose. This manner of observing appears to me more accurate than the other; because if the quicksilver should adhere less to the tube, or be less convex at one time than another, the edge will, in all probability, be more affected by this inequality than the surface. I prefer the cistern to the syphon barometer, because both the trouble of observing and error of observation are less; as in the latter we are liable to an error in observing both legs. Moreover, the quicksilver can hardly fail of settling truer in the former than in the latter; for the error in the settling of the quicksilver can proceed only from the adhesion of its edge to the sides of the tube; now the latter is affected by the adhesion in two legs, and the former by that in only one: and, besides, as the air has necessarily access to the lower leg of the syphon barometer, the adhesion of the quicksilver in it to the tube will most likely be different, according to the degree of dryness or cleanness of the glass. It is true, as Mr. De Luc observes, that the cistern barometer does not give the true pressure of the atmosphere; the quicksilver in it being a little depressed on the same principle as in capillary tubes. But this does not appear to me a sufficient reason for rejecting the use of them. It is better, I think, where so much nicety is required, to determine, by experiment, how much the quicksilver is depressed in tubes of a given bore, and to allow accordingly.

By some experiments which have been made on this subject by my father Lord Charles Cavendish, the depression appears to be as in the following table:

Inside diameter of tube	Grains of quicksilver in one inch of tube	Depress. of surface of quicksilver	Inside diameter	Grains of quicksilver	Depress. of surface	Inside diameter	Grains of quicksilver	Depress. of surface
,6	972	,005	,35	331	,025	,20	108	,067
,5	675	,007	,30	243	,036	,15	61	,092
,4	432	,015	,25	169	,050	,10	27	1,40

The first column is the inside diameter of the tube, expressed in decimals of an inch; the second is the weight of a quantity of quicksilver sufficient to fill one inch in length of it; and the third is the corresponding depression of the convex surface of the quicksilver in a cistern barometer, whose tube is of that size. The reason of giving the second column is, because the easiest way of ascertaining the inside diameter of the tube is, by finding the quantity of quicksilver sufficient to fill a given length of it. It is needless saying, that the part of the tube, whose diameter is to be measured, is that answering to the upper part of the column of quicksilver; and that the table can be of no use but to those only who observe by the convex surface.

In this barometer, the inside diameter of the tube is about ,25 of an inch, and consequently the depression is ,05; the area of the cistern is near 120 times as great as that of the bore of the tube; so that as the quantity of quicksilver was adjusted when the barometer stood at $29\frac{3}{4}$, the error arising from the alteration of the height of the quicksilver in the cistern can scarce ever amount to so much as $\frac{1}{100}$th of an inch. As the tube appeared to be well filled, it was thought unnecessary to have the quicksilver boiled in it; but that is certainly the surest way of filling a barometer well.

The principal reason of setting down the mean heat of the thermometer within doors, during each month, in the journal of the weather, is this: suppose that any one desires to find the mean height of the barometer in any month, corrected on account of the heat of the quicksilver in the tube; that is, to find what would have been the mean height, if the quicksilver in the tube had been constantly of a certain given heat. To do this it is sufficient to take the mean height of the barometer, and correct that according to the mean heat of the thermometer; the result will be exactly the same as if each observation had been corrected separately, and a mean of the corrected observations taken. For example, suppose it is desired to find what would have been the mean height of the barometer in the month of August 1775, if the quicksilver during that time had been always at 50 degrees of heat: the mean of the observed heights is 29,86 inches, and the mean heat of the thermometer is 65° or 50 + 15. The alteration of the height of the barometer by 15° of heat, according to M. De Luc's rule, is ,047 inch; consequently, the corrected mean height is 29,813.

The vessel which receives the rain is a conical funnel, strengthened at the top by a brass ring, twelve inches in diameter. The sides of the funnel and inner lip of the brass ring are inclined to the horizon, in an angle of above 65°; and the outer lip in an angle of above 50°[1]; which are such degrees of steepness, that there seems no probability either that any rain which falls within the funnel, or on the inner lip of the ring, should dash out, or that any which falls on the outer lip should dash into the funnel. This vessel is placed on some flat leads on the top of the Society's House. It can hardly be screened from any rain by the chimnies, as none of them are elevated above it in an angle of more than 25°; and as it is raised 3½ feet above the roof, there seems no danger of any rain dashing into it by rebounding from the lead.

The strength of the wind is divided in the journal into three degrees; namely, gentle, brisk, and violent or stormy, which are distinguished by the figures 1, 2, and 3. When there is no sensible wind it is distinguished by a cypher.

In the future journals of the weather will be given observations of the hygrometer. The instrument intended to be used is of Mr. Smeaton's construction, and is described in *Phil. Trans.* vol. LXI. p. 198. It is kept in a wooden case, made so as to exclude the rain, but to leave a free passage for the wind, and placed in the open air, where the Sun scarce ever shines on it. The instrument and case are both a present to the

[1] To make what is here said the more intelligible, there is, in Fig. 2, given a vertical section of the funnel, *ABC* and *abc* being the brass ring, *BA* and *ba* the inner lip, and *BC* and *bc* the outer.

Society from Mr. Smeaton. The hygrometer was last adjusted in Dec. 1775, and as the string has now been in use upwards of five years, it is not likely to want re-adjusting soon.

Of the Variation Compass.

In this instrument, the box which holds the needle is not fixed, but turns horizontally on a center, and has an index fastened to it, pointing to a divided arch on the brass frame on which it turns; and the method of observing is to move the box, till a line drawn on it points exactly to the end of the needle; which being done, the angle that the needle makes with the side of the frame is shewn by the index. Fig. 3. is the plan of the instrument; *ABba* is the brass frame, the sides *AB* and *ab* being parallel; *Ee* is a circular plate fastened thereto, on which *CDdc*, the box which holds the needle, turns as on a center; *Nn* is the needle, the pin on which it vibrates, being fixed in the center of the plate *Ee*; *Bb* is the division on the brass frame; and *G* the index fastened to the box *CDdc*, furnished with a vernier division; the division and vernier being constructed so as to shew the angle which the line *Ff* makes with *AB* or *ab*. The instrument is placed in the meridian by the telescope *Mm*, the line of collimation of which is parallel to *AB*, and is pointed to a mark fixed due North of it.

Fig. 4. is a vertical section of the instrument passing along the line *Ff*; *AB* is the brass frame; *CDdc* the box which holds the needle; *Ee* the circular plate on which it turns; *Nn* is the needle; *P* and *p* are small plates of brass fixed to the ends of it, on each of which is drawn a line serving by way of index. These pieces of brass are raised to such a height that their tops are on a level with the point of the pin on which the needle turns. The use of them is, that it is much easier observing this way than when the lines, serving by way of index, are drawn on the needle itself, as by this means the inconvenience proceeding from one kind of vibration in the needle is avoided. *S* and *s* are two brass plates, on each of which is drawn a line to which the index at the end of the needle is to point; there is also a line parallel to these drawn on the bottom of the box; these three lines form the line *Ff* in Fig. 3. *R* is a double microscope intended to assist us in judging when the index *P* points exactly to the line *F*, that is, to the line drawn on the plate *S*. It is placed so, that a wire *Ww* in its *focus* appears to coincide with this line; and in observing, the box is moved till the wire appears also to coincide with the index *P*.

The cap in the center of the needle is made to take on and off readily, and to fit on upon either face; so that we may on occasion observe with the under face of the needle uppermost, as is represented in Fig. 5. But the regular observations are always made with the needle in its upright position, and by the help of the index *P* only; the intention of the other index and of inverting the needle is, to shew whether the line joining the indices P and p, or the line Pp as I shall call it, is parallel to the direction of magnetism in the needle, and thereby to find whether, in the usual method of observing, the index G shews the true angle which the direction of magnetism makes with the side AB. The way of doing this is as follows; having suffered the needle to settle, the observer moves the box by means of the adjusting screw T, till the index P coincides with the line F, and reads off the angle shewn by the vernier. He then moves the box till the other index p coincides with the line f, which, as the pin on which the needle is suspended is fixed to the brass frame, may be done without any danger of altering the position of the needle or making it vibrate, and reads off the angle as before. The mean of these two is the true angle which the line Pp makes with the side AB, supposing the division and vernier to be rightly constructed, even though neither the lines Pp nor Ff should pass through the center of the pin. Having done this, he takes off the cap and inverts the needle, and observes by both indices as before. It is plain, that if the line Pp is parallel to the direction of magnetism in the needle, this mean will agree with the former, supposing that the magnetic variation has not altered between the observations. On the other hand, if it is not parallel to the direction of magnetism, but makes the variation appear greater than it ought to do when the needle is upright, it will make it appear as much less when the needle is inverted; so that the mean of the two abovementioned means is the true angle which the direction of magnetism in the needle makes with the side AB; that is, the true variation of the needle at that time and place, supposing AB to be placed accurately in the meridian. Having thus found the true angle which the direction of magnetism makes with AB, he subtracts that shewn by the index P in the upright position of the needle; the difference is the error of the instrument in the usual manner of observing.

It was by this method that the error of the instrument, at the time of the observations in 1774, was found to be 10′. For example, by a mean of the observations made on Sept. 5. the variation with the needle, in its upright position, was 21·36 by the South end, and 21·27 by the North; with the needle inverted it was 21·19 by the South end, and 21·29 by the North. The mean of all four is 21·28, which is the true variation at that time and place[1], and is 8′ less than that shewn in the upright position of the needle by the South end, which is the end always used in observing; so that by this day's experiment the error of the instrument appeared to be 8′; but by a mean of the observations of this and two other days it came out 10′. Since that time the needle has been altered; and, at the time of the observations in 1775, the error was so small as to be scarcely sensible.

[1] The quantity found by taking a mean of all the four numbers is evidently the same as that got by taking a mean of the two first and of the two last, and taking a mean of those two means.

Great care was taken that the metal, of which this variation compass is composed, should be perfectly free from magnetism. There is a contrivance in it for lifting the needle from off the point, and letting it down gently, to prevent injury in carrying from one room to another. The instrument is constructed nearly on the same plan as some made by the late Dr. Knight. The principal difference is, that in his the pin which carried the needle was not fixed to the lower frame as in this, but to the box; the consequence of which was, that when the needle had settled, and the box was moved to make the index on the needle point to the proper mark, it was again put into vibration, which caused great trouble to the observer. This inconvenience is intirely removed by the present construction. There is no other material difference, except that of the needle being made to invert, and the addition of the telescope. The contrivance of fixing the pin which carries the needle to the lower frame, is taken from an instrument of Lord Charles Cavendish; that of making the needle invert I have seen in some compasses made by Sisson.

There is a very common fault in the agate-caps usually made for needles, which is, that they are not hollowed to a regular concave, but have a little projecting part in the center of the hollow; the consequence of which is, that the point of the pin will not always bear against the same part of the agate, and consequently the needle will not always stand horizontal; but sometimes one end will stand highest, and sometimes the other, which causes a difficulty in observing. There is also another inconvenience attends it when the indices of the needle are on a level with the point of the pin, which is of more consequence; namely, that it causes the two indices not to agree, and consequently makes a sensible error, when only one index is made use of, at least in nice observations: but when the lines, serving by way of index, are drawn on the needle itself, and therefore are nearly on a level with its center of gravity, it can cause very little error. The agate cap, which was first made for this instrument, was of this kind; and was so faulty, that, if no better could have been procured, it would have been necessary either to have drawn the lines serving by way of index on the needle itself, or to have observed by both ends, either of which would have been attended with a considerable increase of trouble to the observer; but Mr. Nairne, the artist who made the instrument, has since ground some himself, which are perfectly free from this fault, the concave surface being of an extremely regular shape and well polished, and also of a very small radius of curvature; which is a matter of considerable consequence, as otherwise the point of the pin will not easily slip sufficiently near to the bottom of the hollow.

Care was taken to place the variation compass in a part of the house where it is as little likely to be affected by the attraction of the iron work as in any that could be found. As it seemed, however, to be not intirely out of the reach of the influence of that metal, I took the following method to examine how much it was influenced thereby. The instrument was removed into a large garden belonging to a house in Marlborough Street, distant from the Society's House about one mile and a quarter towards the West, where there seemed no danger of its being affected by any iron-work. Here it was placed exactly in the meridian, and compared for a few days with a very exact compass, placed in an adjoining room, and kept fixed constantly in the same situation. It was then removed back to the Society's House, and compared again with the same compass. The observations were as follow:

Observations made with the Society's instrument in the garden.

Time		Variation by Society's instrument	Variation by Compass in room	Difference
1775	h ′	° ′	° ′	′
July 21	4 48 V	21 31	21 33	− 2
	5 0	32	35	− 3
	5 26	30	28	+ 2
	5 43	31	32	− 1
	5 48	30	30	0
22	10 45 M	33	33	0
	11 2	29	30	− 1
	11 18	31	29	+ 2
	11 37	31	31	0
	11 55	31	33	− 2
	4 36 V	31	32	− 1
	4 53	27	30	− 3
	5 22	24	26	− 2
	5 54	26	26	0
July 31	11 4 M	21 28	21 32	− 4
	11 20	28	30	− 2
	11 38	30	30	0
	11 57	29	32	− 3
	0 13 V	29	33	− 4
	0 32	30	31	− 1
	2 24	32	35	− 3
	2 54	32	31	+ 1
Aug. 1	10 34 M	26	28	− 2
	3 13 V	32	33	− 1
	4 33	29	29	0
	4 46	29	31	− 2
	5 12	27	29	− 2
	5 35	27	28	− 1
	5 57	28	30	− 2

The instrument being removed back to the Society's house.

Time		Variation by Society's instrument	Variation by Compass in room	Difference
1775	h ′	° ′	° ′	′
Aug. 2	1 8 V	21 45	21 32	+ 13
	1 10	44	30	+ 14
	1 20	46	29	+ 17
	1 30	47	29	+ 18
	1 40	47	32	+ 15
	1 50	47	31	+ 16
	2 0	47	31	+ 16
Aug. 4	10 50 M	21 47	21 33	+ 14
	11 0	47	34	+ 13
	11 10	47	35	+ 12
	11 20	47	35	+ 12
	11 30	46	35	+ 11
	11 40	47	34	+ 13

By a mean of the observations, the variation shewn by the compass in the room is 1′,3 greater than by the Society's instrument in the garden, and 14′,1 less than by the same instrument placed in its proper situation; so that the variation appears to be 15′,4 greater in that part of the Society's House where the compass is placed, than in the abovementioned garden; and therefore, as there is no likelihood of its being affected by any iron in the latter place, the needle seems to be drawn aside 15′½ towards the N.W. by the iron work of the house and adjacent buildings.

On comparing the observations of the two last years together, the variation appears, after allowing for the error of the instrument, to have been 27′ greater in 1775 than in 1774; though I have been informed by Dr. Heberden, who has made observations of this kind for several years past, that the annual alteration of the variation has, in general, been not more than 10′; and in particular, that the alteration in the last year appears to be only 11½′; so that the great difference observed at the Society's House seems to be owing, not solely to the real alteration in the variation, but partly to some other cause; though what that should be I cannot conceive, unless some change was made in the iron work either of this or the adjoining houses between the two periods; but I do not find that any such change has been made. During the last year, indeed, there have been two large magnets in the house, each consisting of several great bars joined together, being what the late Dr. Knight used for making artificial magnets, and at the time of the observations in 1774 there was only one; but their distance from the compass is above fifty feet: and I am well assured, that in the situation in which they are actually placed, they cannot draw the needle aside more than 3′, and not more than 15′, when the line joining their poles is placed in such a direction as to act with most force[1]. The single magnet in the year 1774 was placed nearly in the same situation and direction that the two were in 1775, so that the difference of their effect in these two years can hardly have been so much as 3′; and therefore, the great apparent alteration of the variation between the two periods cannot have been owing to them. Neither can it have been owing to the fault of the agate cap used in the year 1774, as the error proceeding from thence could hardly be more than 2 or 3′. It is intended that, for the future, the abovementioned magnets shall be kept always in the same situation and direction that they are in at present, and in which they were in 1775.

Of the Dipping-needle.

In this instrument the ends of the axis of the needle roll on horizontal agate planes, a contrivance being applied, by which the needle is at pleasure lifted off from the planes, and let down on them again, in such manner as to be supported always by the same points of the axis resting on the same parts of the agate planes; and the motion with which it is let down is very gradual and without shake. The general form of the instrument, the size and shape of the needle, and the cross used for balancing it, are the same as in the dipping-needle described in *Phil. Trans.* vol. LXII. p. 476. It is also made by the same artist Mr. Nairne.

It may be seen in the Meteorological Journal, that the dip was observed first with the front of the instrument to the West, and then to the East; after which the poles of the needle were reversed, and the dip observed both ways as before. The reason of this is, that the mean of the observed dips, in these four situations, differs very little from the truth, though the needle is not well balanced, and even though a great many other errors are committed in the construction of the instrument; provided the needle is made equally magnetical after the poles are reversed as before[2]; and that the difference of the observed dip, in these four situations, is not very great, as will appear from the following considerations.

First, let Fig. 7 [6] be a front view of the needle; AB a line parallel to the direction of magnetism therein; and CD a perpendicular thereto, meeting

[1] The principle by which this was determined is, that if a magnet is placed near a variation compass, with its poles equi-distant from it, and situated so that each shall act equally oblique to the length of the needle, it can have no tendency to alter the variation; and that the situation in which it alters it most, except when placed nearly North or South of the compass, is when the line joining its poles points almost directly towards the needle. This experiment I tried purposely on the occasion, and found it answer; but, I believe, any one skilled in magnetism would have granted the truth of the position without that precaution.

[2] It is easy to see whether the needle is made equally magnetical after the poles are reversed as before, by counting the number of vibrations which it makes in a minute.

it in the line joining the centers of the cylindrical ends of the axis, or in the axis of motion as we may call it. If the needle was truly balanced, its center of gravity would be in *d*, the intersection of *AB* and *CD*. Suppose now, that the needle is not truly balanced, but that its center of gravity is in *g*; draw *gn* perpendicular to *AB*, cutting it in *m*; and let the parts *mn* and *mg* be equal. When the instrument is turned half-way round, so that the contrary face of the needle is presented towards us, the edge *ADB*, which is now lowest, will become uppermost, and the center of gravity will be in that situation in which the point *n* now is; therefore, the mean between the forces with which the needle is drawn out of its true position in these two situations, in consequence of its not being truly balanced, is accurately the same; and the mean between the two observed dips is very nearly the same, as if the center of gravity was at *m*. But if the center of gravity is at *m*, the dip will be very nearly as much too great in the present state of the needle, as it will be too little when the poles are reversed. Therefore, the mean of the observed dips in these four situations will be very nearly the same as if the needle was truly balanced.

Secondly, if the planes on which the axis rolls are not horizontal, the dip will be very nearly as much greater than it would otherwise be, when one face is turned to the West, as it is less when the other is; for if these planes dip towards the South in one case, they will dip as much towards the North in the other, supposing the levels by which the instrument is set to remain unaltered. Consequently, the mean of the two observations will be very nearly the same as if they were placed truly horizontal.

Thirdly, by the same method of reasoning it appears, that the mean of the two abovementioned observations will be not at all altered, though the line, joining the mark on that end of the needle by which we observe, with the axis of motion, is not parallel to the direction of magnetism in the needle; that is, though the mark does not coincide with the point *A* or *B*, or though the line joining the two divisions of 90° is not perpendicular to the horizon, or though the axis of motion does not pass through the center of the divided circle, provided it is in the same horizontal plane with it. If, indeed, the axis of motion is not in the same horizontal plane with the center of the divided circle, the error proceeding from thence will not be compensated by this method of observing, unless both ends of the needle are made use of. This, however, is of no consequence as it is easy to examine whether they are in the same horizontal plane or not.

But the error which is most difficult to be avoided is, that which proceeds from the ends of the axis being not truly cylindrical. I before said, that the parts of them which rest on the agate planes are always exactly the same. The instrument is so contrived, however, that we may on occasion, by giving the axis a little liberty in the notches by which it is lifted up and down, make those planes bear against a part of the axis distant about $\frac{1}{100}$ or $\frac{1}{50}$th of an inch from their usual point of bearing.

Now, I find, that when the axis is confined so as to have none of this liberty, and when care is taken, by previously making the needle stand at nearly the right dip, that it shall vibrate in very small arches when let down on the planes; that then, if the needle is lifted up and down any number of times, it will commonly settle exactly at the same point each time, at least the difference is so small as to be scarcely sensible; but if it is not so confined, there will often be a difference of 20′ in the dip, according as different parts of the axis rest on the planes, and that though care is taken to free the axis and planes from dust as perfectly as possible, which can be owing only to some irregularity in the axis. Moreover, if the needle vibrates in arches of five or more degrees, when let down on the planes, there will frequently be as great an error in the dip. It is true, that the part of the agate planes, which the axis rests on when the vibrations are stopped, will be a little different according to the point which the needle stood at before it was let down; which will make a small difference in the dip as shewn by the divided circles, when only one end of the needle is observed, though the real dip or inclination of the needle to the horizon is not altered: but this difference is by much too small to be perceived; so that the abovementioned error cannot be owing to this cause. Neither does it seem owing to any irregularity in the surface of the agate planes, for they were ground and polished with great accuracy; but it most likely proceeds from the axis slipping in the large vibrations, so as to make the agate planes bear against a different part of it from what they would otherwise do. I have great reason to think, that this irregularity is not owing either to want of care or skill in the execution, but to the unavoidable imperfection of this kind of work. I imagine too, that this instrument is at least as exact, if not more so, than any which has been yet made.

The following table contains the result of some observations which I made, partly with a view to determine the true dip at this time in a place out of reach of the influence of any iron work, and partly to see how nearly different needles would agree. The instruments were all tried in the same garden in which the variation compass was observed, and all on the 10th, 11th, 13th and 14th days of October, 1775, except that marked *, which was tried on the 15th of the preceding April.

Each of the numbers set down in the above table [see p. 126] is the mean of two observations, the instruments being observed first with the front to the East, then to the West; then a second time to the East, and then again to the West; and in all the observations, except those with the two last instruments, which are of a different construction, care was taken that the needle should vibrate in very small arches when let down on the agate planes. By a mean of all, the true dip at London, at this time, comes out 72° 30′, the different needles all agreeing within 14′, which is a difference considerably less than I should have expected. It appears also, that the

	East	West	Poles reversed East	Poles reversed West	Poles restored to their first situation East	Poles restored to their first situation West	True dip
	° ′	° ′	° ′	° ′	° ′	° ′	° ′
The Society's needle ...	72 32	72 8	72 9	72 40	72 59	71 50	72 23
Another of the same construction, belonging to Mr. Nairne ...	72 56	72 29	71 45	73 21	72 51	72 27	72 37
One of mine on nearly the same construction	72 33	72 22	71 41	73 23	72 34	72 18	72 30
Another needle in the same frame	72 22	72 7	71 40	73 53	72 16	72 30	72 33
A needle of mine, made by Sisson, partly on the same construction as Mr. Lorimer's[1]	*73 1	71 49	71 57	73 0			72 27
Another of Mr. Nairne's on the same construction	73 8	72 0	73 15	71 57			72 35

dipping-needle, in the situation in which it is placed at the Society's House, is not much affected by any iron work, as the dip shewn by it in the garden differs only 7′ from that set down in the journal of the weather.

According to Norman, the inventor of the dipping-needle, the dip at London in the year 1576 was $71^\circ 50'$[2]; in 1676 it was $73^\circ 47'$, according to Mr. Bond[3]; Mr. Whiston in 1720 made it $75^\circ 10'$[4]; Mr. Graham in 1723 made it between 73½ or 75°[5], his different trials varying so much; and at present it appears to be $72^\circ 30'$. I do not know how much Mr. Bond's determination is to be depended on, as he does not say by what means he arrived at it; but, I believe, Mr. Whiston's is pretty accurate, for he observed the dip in many parts of the kingdom, and the observations agree well together; so that it is reasonable to suppose, that his instrument was a good one, and that he observed in places where the needle was not much influenced by iron work. The dip, therefore, seems to have been considerably greater about the year 1720, than it was in Norman's time, or is at present: it appears, however, to alter very slowly in comparison of the variation.

[1] See *Phil. Trans.* vol. LXV. p. 79.

[2] New Attractive, c. 4.

[3] Longitude found, p. 65.

[4] Longitude and Latitude found by Dipping-needle, pp. 7, 49, and 94.

[5] *Phil. Trans.* No. 389, p. 332.

PHILOSOPHICAL TRANSACTIONS. VOL. 73, 1783, p. 106

VIII. *An Account of a new Eudiometer.* *By Mr* Cavendish, *F.R.S.*

Read January 16, 1783

DR. PRIESTLEY'S discovery of the method of determining the degree of phlogistication of air by means of nitrous air, has occasioned many instruments to be contrived for the more certain and commodious performance of this experiment; but that invented by the Abbé Fontana is by much the most accurate of any hitherto published. There are many ingenious contrivances in his apparatus for obviating the smaller errors which this experiment is liable to; but the great improvement consists in this, that as the tube is long and narrow, and the orifice of the funnel not much less than the bore of the tube, and the measure is made so as to deliver its contents very quick, the air rises slowly up the tube in one continued column; so that there is time to take the tube off the funnel, and to shake it before the airs come quite in contact, by which means the diminution is much greater and much more certain than it would otherwise be. For instance, if equal measures of nitrous and common air are mixed in this manner, the bulk of the mixture will, in general, be about one measure, whereas, if the airs are suffered to remain in contact about one-fourth of a minute before they are shaken, the bulk of the mixture will be hardly less than one measure and two-tenths, and will be very different according as it is suffered to remain a little more or a little less time before it is shaken. In like manner, if through any fault in the apparatus, the air rises in bubbles, as in that case it is almost impossible to shake the tube soon enough, the diminution is less than it ought to be.

Another great advantage in this manner of mixing is, that thereby the mixture receives its full diminution in the short time during which it is shaken, and is not sensibly altered in bulk after that; whereas, if the airs are suffered to remain some time in contact before they are shaken, they will continue diminishing for many hours.

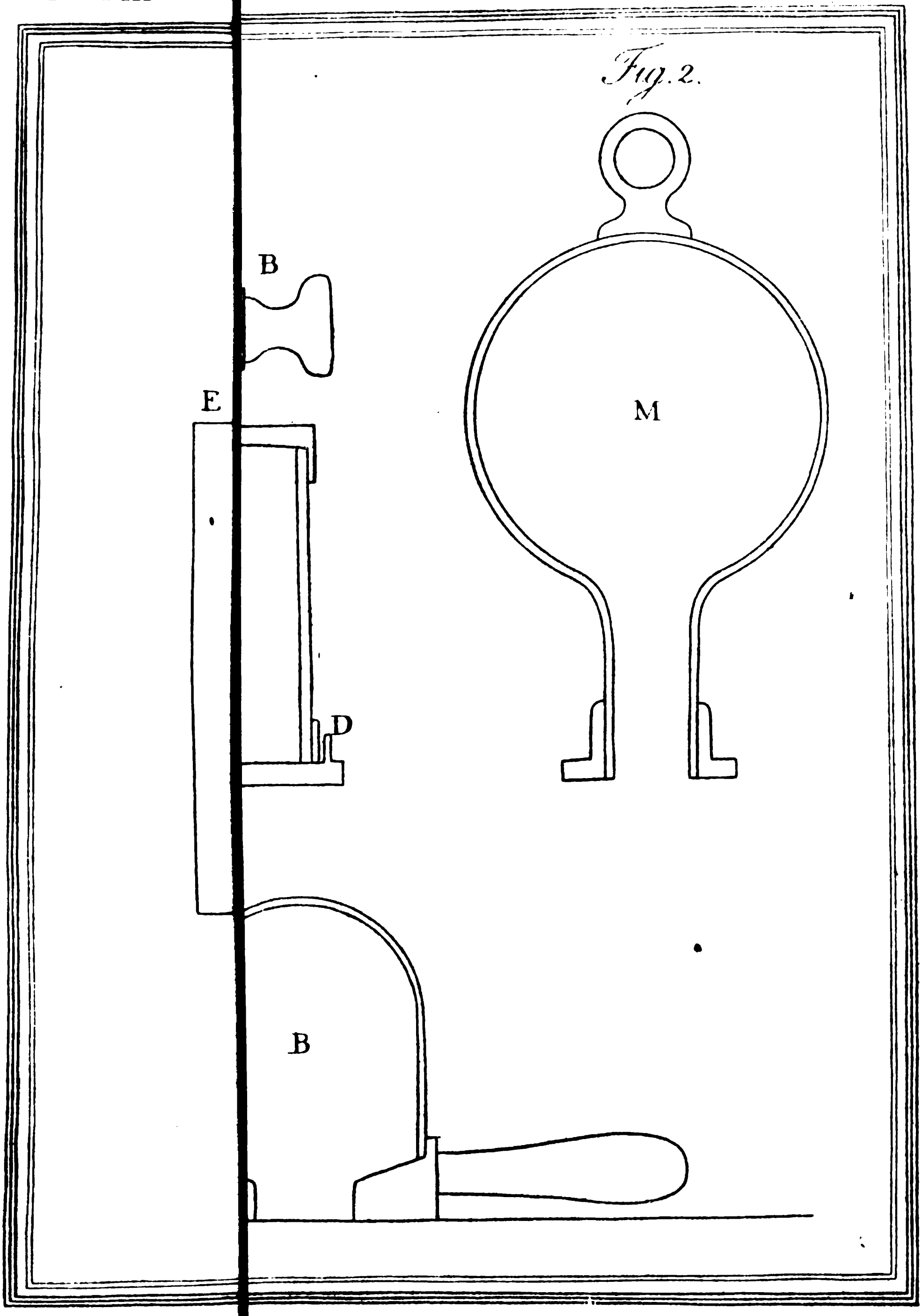
The reason of the abovementioned differences seems to be, that in the Abbé Fontana's method the water is shaken briskly up and down in the tube while the airs are mixing, whereby each small portion of the nitrous air must be in contact with water, either at the instant it mixes with the common air, or at least immediately after; and it should seem, that when the airs are in contact with water during the mixing, the diminution is much greater and more certain than when there is no water ready to absorb the nitrous acid produced by the mixture. This induced me to try whether the diminution would not be still more certain and regular if one of the two kinds of air was added slowly to the other in small bubbles, while the vessel containing the latter was kept continually shaking. I was not disappointed in my expectations, as, I think, this method is really more accurate than the Abbé Fontana's; and, moreover, in the course of my experiments I had occasion to observe a circumstance which is necessary to be attended to by those who would examine the purity of air with exactness by any kind of eudiometer, besides some others which tend very much to explain many of the phenomena attending the mixture of common and nitrous air.

The apparatus I use is as follows. *A* (Fig. 1) is a cylindrical glass vessel, with brass caps at top and bottom; to the upper cap is fitted a brass cock *B*; the bottom cap is open, but is made to fit close into the brass socket *Dd*, and is fixed in it in the same manner as a bayonet is on a musquet. The socket *Dd* has a small hole *E* in its bottom, and is fastened to the board of my tub by the bent brass *F f G*, in such manner that *b*, the top of the cock, is about half an inch under water; consequently if the vessel *A* is placed in its socket, with any quantity of air in it, and the cock is then opened, the air will run out by the cock, but will do so very slowly, as it can escape no faster than the water can enter by the small hole *E* to supply its place.

Besides this vessel, I have three glass bottles like *M* (Fig. 2) each with a flat brass cap at bottom to make it stand steady, and a ring at top to suspend it by, and also some measures of different sizes such as *B* (Fig. 3); these are of glass with a flat brass cap at bottom and a wooden handle. In using them they are filled with the air wanted to be measured, and then set upon the brass knob *C* fitted upon the board of my tub below the surface of the water, which drives out some of the air, and leaves only the proper quantity. This measure is easier made, and more expeditious in using, than the Abbé Fontana's, and, I believe, is equally accurate; but if it was not it would not signify, as I determine the exact quantity of air used by weight.

There are two different methods of proceeding which I have used; the first is to add the respirable air slowly to the nitrous; and the other, to add the nitrous air in the same manner to the respirable. The first is what I have commonly used, and which I shall first describe. In this method a proper quantity of nitrous air is put into one of the bottles *M*, by means of one of the measures above described, and a proper quantity of respirable is let into the vessel *A*, by first filling it with this air, and then setting it on the knob *C*, as was done by the measure. The vessel *A* is then fixed

Plate III



in the socket, and the bottle *M* placed with its mouth over the cock. Then on opening the cock, the air in the vessel *A* runs slowly in small bubbles into the bottle *M*, which is kept shaking all the time by moving it backwards and forwards horizontally while the mouth still remains over the cock.

Notwithstanding the precautions used by the Abbé Fontana in measuring the quantity of air used, I have sometimes found that method liable to very considerable errors, owing to more water sticking to the sides of the measure and tube at one time than at another: for this reason I determine the quantities of air used and the diminution, by weighing the vessels containing it under water in this manner. From one end of a balance, placed so as to hang over the tub of water, is suspended a forked wire, to each end of which fork is fixed a fine copper wire; and in trying the experiment the vessel *A*, with the respirable air in it, is first weighed, by suspending it from one of these copper wires, in such manner as to remain intirely under water. The bottle *M*, with the proper quantity of nitrous air in it, is then hung on in the same manner to the other wire, and the weight of both together found. The air is then let out of the vessel *A* into the bottle *M*, and the weight of both vessels together found again, by which the diminution of bulk which they suffer on mixing is known. Lastly, the bottle *M* is taken off, and the vessel *A* weighed again by itself, which gives the quantity of respirable air used. It is needless to determine the quantity of nitrous air by weight; because, as the quantity used is always sufficient to produce the full diminution, a small difference in the quantity makes no sensible difference in the diminution[1].

[1] Mr. De Saussure also determines the quantity of air which he uses by weight; but does it by weighing the vessels containing it in air. This method is liable to some inaccuracy, as the air in the vessel is apt to be compressed by putting in the stopper; though, I believe, that, if care is taken to push in the stopper slowly, the error arising from thence is but small. It is also less expeditious than weighing them under water, as some time is necessarily lost in wiping the wet off the vessels; but, on the other hand, it requires less apparatus, which makes it fitter for a portable apparatus as Mr. De Saussure's was. If any gentleman is desirous of adapting this method of determining the quantities to the above described manner of mixing the airs, nothing more is required than to have glass stoppers fitted to the vessel *A* and to the bottle *M*.

It is needless to mention, that in both these methods no sensible error can arise from any difference in the specific gravity of the air; for the thing found by weighing the vessel is the difference of weight of the included air and of an equal bulk of water, which, as no air is less than 500 times lighter than water, is very nearly equal to the weight of a quantity of water, equal in bulk to the included air.

It must be observed, that a common balance is not convenient for weighing the vessels of air under water, without some addition to it; for the lower the vessel of air sinks under the water, the more the air is compressed, which makes the vessel heavier, and thereby causes that end of the beam to preponderate. This makes it necessary either to have the index placed below the beam, as in many assay balances; or by some other means to remove the center of gravity of the beam so much below the center of suspension as to make the balance vibrate, notwithstanding the tendency which the compressibility of the air in the vessels has to prevent it.

In this manner of determining the quantities by weight, care should be taken to proportion the lengths of the copper wires in such manner that the surface of the water in *A* and *M* shall be on the same level when both have the usual quantity of air in them, as otherwise some errors will arise from the air being more compressed in one than in the other. This precaution indeed does not intirely take away the error, as the level of the water in *M* is not the same after the airs are mixed as it was before; but in vessels of the same size as mine, the error arising from thence can never amount to the 500th part of the whole, which is not worth regarding; and indeed if it were much greater, it would be of very little consequence, as it would be always the same in trying the same kind of air.

There are several contrivances which I use, in order to diminish the trouble of weighing the vessels; but I omit them, as the description would take up too much room.

The vessel *A* holds 282 grains of water, and is the quantity which I shall distinguish by the name of one measure. I have three bottles for mixing the airs in, with a measure *B* for the nitrous air adapted to each. The first bottle holds three measures, and the corresponding measure $1\frac{1}{4}$; the second bottle holds six, and the corresponding measure $2\frac{1}{2}$; and the third bottle holds 12, and the corresponding measure 5. The first bottle and measure are used in trying common air, or air not better than that; the two others in trying dephlogisticated air. The quantity of respirable air used, as was said before, is always the same, namely, one measure; consequently, in trying common air I used $1\frac{1}{4}$ measures of nitrous air to one of common; and in trying very pure dephlogisticated air I used five measures of nitrous air to one of the dephlogisticated. I believe there is no air so much dephlogisticated as to require a greater proportion of nitrous than that. The way by which I judge whether the quantity of nitrous air used is sufficient, is by the bulk of the two airs when mixed; for if that is not less than one measure, that is, than the respirable air alone, it is a sign that the quantity of nitrous air is sufficient, or that it is sufficient to produce the full diminution, unless it is very impure.

Though the quantity of respirable air used will be always nearly the same, as being put in by measure; yet it will commonly be not exactly so, for which reason the observed diminution will commonly require some correction: for example, suppose that the observed diminution was 2·353 measures, and that the quantity of respirable air was found to be ·985 of a measure; then the observed diminution must be increased by $\frac{15}{1000}$ of the whole or ·035, in order to have the true diminution, or that which would have been produced if the respirable air used had been exactly one measure; consequently, the true diminution is 2·388.

The method of weighing, described in p. [129], is that which I use in trying air much different in purity from common air; but in trying common air, I use a shorter method, namely, I do not weigh the vessel *A* at all, but only weigh the bottle *M* with the nitrous air in it; then mix the airs, and again weigh the same bottle with the mixture in it, and find the increase of weight. This, added to one measure, is very nearly the true diminution, whether the quantity of common air used was a little more or a little less than one measure. The reason of this is, that as the diminution produced on mixing common and nitrous air is only a little greater than the bulk of the common air, the bulk of the mixture will be very nearly the same, whether the bulk of the common air is a little greater or a little less than one measure: for example, let us first suppose, that the quantity of common air used is exactly one measure, and that the diminution of bulk on mixing is 1·08 of a measure, then must the increase of weight of the bottle *M*, on adding the common air, be ·08 of a measure. Let us now suppose, that the quantity of common air used is 1·02 of a measure, then will the diminution, on adding the common air, be $1{\cdot}08 \times \frac{1{\cdot}02}{1{\cdot}00}$ or 1·1016 of a measure, and consequently the increase of weight of the bottle *M* will be $1{\cdot}1016 - 1{\cdot}02$ or ·0816 of a measure, which is very nearly the same as if the common air used had been exactly one measure.

In the second method of proceeding, or that in which the nitrous air is added to the respirable, I use always the same bottle, namely, that which holds three measures, and use always one measure of respirable air; and in trying common air I use the same vessel *A* as in the first method; but for dephlogisticated air I use one that holds $3\frac{3}{4}$ measures.

In trying the experiment I first weigh the bottle *M* without any air in it, and then weigh it again with the respirable air in it, which gives the quantity of respirable air used. I next put the nitrous air into the vessel *A*, and weigh that and the bottle *M* together, and then having mixed the airs, weigh them again, which gives the diminution.

From what has been just said, it appears, that in this method of proceeding I use a less quantity of nitrous air in trying the same kind of respirable air than in the former; the reason of which is, that the same quantity of nitrous air goes further in phlogisticating a given quantity of respirable air in this than in the former method, as will be shewn further on.

In both these methods I express the test of the air by the diminution which they suffer in mixing; for example, if the diminution on mixing them is two measures and $\frac{353}{1000}$, I call its test 2·353, and so on.

In the first method of proceeding I found, that the diminution was scarce sensibly less when I used one measure of nitrous air than when I used a much greater quantity; so that one measure is sufficient to produce the full diminution. I chuse, however, to use $1\frac{1}{4}$, for fear the nitrous air may be impure; $\frac{7}{8}$ths of a measure of nitrous air produced about $\frac{15}{16}$, and $\frac{3}{4}$ths of a measure about $\frac{7}{9}$ths of the full diminution.

I found also, that there was no sensible difference in the diminution whether the orifice by which the air passed out of the vessel *A* into the bottle *M* was only $\frac{1}{25}$th of an inch in diameter, or whether it was $\frac{1}{8}$th of an inch; that is, whether the air escaped in smaller or larger bubbles. The diminution was rather less when the bottle was shook gently than when briskly; but the difference between shaking it very gently and as briskly as I could was not more than $\frac{1}{100}$dth of a measure. But if it was not shaken at all the diminution was remarkably less, being at first only ·9; in about 3′, indeed, it increased to ·93, and after being shaken for about a minute it increased to ·99; whereas, when the bottle was shaken gently, the diminution was 1·08 at first mixing, and did not increase sensibly after that time. The difference proceeding from the difference of time which the air took up in passing into the bottle was rather greater; namely, in some trials, when it took up 80″ in passing, the diminution was $\frac{5}{100}$dths greater than when it took up only 22″, and about $\frac{2}{100}$dths greater than when it took up 45′; in some other trials, however, the difference was less. It appears, therefore, that the difference arising from the difference of time which the air takes up in passing into the bottle is considerable; but, as with the same hole in the plate *Dd* it will take up always nearly the same time, and as it is easy adjusting the size of the hole, so as to make it take up nearly the time we desire, the error proceeding from thence is but small. The time which it took up in passing in my experiments was usually about 50″.

The difference proceeding from the difference of size of the bottle, and the nature of the water made use of is greater; for when I use the small bottle which holds three measures, and fill it with distilled water, the usual diminution in trying common air is 1·08; whereas, if I fill the bottle with water from my tub, the diminution is usually about ·05 less. If I use the bottle which holds twelve measures, filled with distilled water, the diminution is about 1·15; and if I use the same bottle, filled with water from my tub, about 1·08.

The reason of this difference is, that water has a power of absorbing a small quantity of nitrous air; and the more dephlogisticated the water is, the more of this air it can absorb. If the water is of such a nature also as to froth or form bubbles on letting in the common air, the diminution is remarkably less than in other waters.

The following table [p. 133] contains the diminution produced in trying common air in the bottle containing three measures, with several different kinds of water, and also the diminution which the same quantity of nitrous air suffered by being only shook in the same bottle, without the addition of any common air, tried by stopping the mouth of the bottle with my finger, and shaking it briskly for one minute, and afterwards for one minute more.

In general, the diminution was nearly as great with rain water as distilled water; but sometimes I have found rain water froth a good deal, and then the diminution was not much greater than by the water fouled with oak shavings.

Diminution in trying common air	Diminution on shaking nitrous air for: One minute	Two minutes	
1·099	·118	·122	Distilled water
1·049	·083	·088	Water from tub
1·036	·090	·098	Pump water
1·062	·090	·099	Distilled water, in which a few drops of liver of sulphur were kept for a few days
1·045	·052	·056	Distilled water impregnated with nitrous air, by keeping it with about ¼ of its bulk of nitrous air for two days, and frequently shaking it
·897	·082	·085	Water fouled by oak shavings. N.B. it frothed very much

This difference in the diminution, according to the nature of the water, is a very great inconvenience, and seems to be the chief cause of uncertainty in trying the purity of air; but it is by no means peculiar to this method, as I have found as great a difference in Fontana's method, according as I have filled the tube with different waters[1]. But it shews plainly, how little all the experiments which have hitherto been made for determining the variations in the purity of the atmosphere can be relied on, as I do not know that any one before has been attentive to the nature of the water he has used, and the difference proceeding from the difference of waters is much greater than any I have yet found in the purity of air.

The best way I know of obviating this inconvenience is to be careful always to use the same kind of water: that which I always use is distilled, as being most certain to be always alike. I should have used rain water, as being easier procured, if it had not been that this water is sometimes apt to froth, which I have never known distilled water do.

As I found that the power with which the distilled water I used absorbed nitrous air was greater at some times than others, which must necessarily make an error in the observation, I was in hopes that, by observing the quantity of nitrous air which the water absorbed in the same manner as in the preceding experiment, together with the heat of the water, as that also seems to affect the experiment, one might be able to correct the observed test, and thereby obviate the error which would otherwise arise from any little difference in the nature of the water employed. With this view I made the following experiment.

I purged some distilled water of its air by boiling, and kept one part

[1] I do not find that it makes much difference in Fontana's method whether the water is disposed to froth or not; but the advantage which it has in that respect over this method is not of much consequence, as it is easy finding water which will not froth.

of it for a week in a bottle along with some dephlogisticated air, and shook it frequently; the other part was treated in the same manner with phlogisticated air. At the end of this time I found, by a mean of three different trials, that the test of common air tried with the first of these waters was 1·139, the diminution which nitrous air suffered by being shook 2′ in it in the usual manner was ·285. The test of the same air tried with the last of these waters was only 1·054, and the diminution of nitrous air only ·090, the heat of the water in the tub and of the distilled waters being 45°. I then raised both the water of the tub and the distilled waters to the heat of 67°, and found that the test of the same air, tried by the first water, was then 1·100, and by the latter 1·044; and that the diminution of nitrous air was ·235 by the first water, and ·089 by the latter.

It should seem from hence, as if the observed test ought to be corrected by subtracting $\frac{4}{10}$ths of the diminution which nitrous air suffers by being shaken in the water, and adding ·002 for every 3° of heat above 0, as the foregoing trials will agree very well together, if they are corrected by this rule, and better than if corrected by any different rule, as will appear by the following table.

	Heat	Diminution of nitrous air	Observed test	Correction for Diminution	Correction for Heat	Corrected test
Former water	45	·285	1·139	·114	·030	1·055
	67	·235	1·100	·094	·045	1·051
Latter water	45	·090	1·054	·036	·030	1·048
	67	·089	1·044	·036	·045	1·053

Though in all probability this correction will diminish the error proceeding from a difference in the nature of the distilled water employed, yet I have reason to think, that it will by no means entirely take it away; for which reason I do not in general make use of it. In almost all the trials, indeed, in which I have applied the correction, it has come out very nearly the same; which seems to shew, that there was no other difference in the absorbing power of the distilled water I employed, than what proceeded from its difference of heat. The above experiment, however, shews plainly, that distilled water is capable of a very great difference in this respect independent of its heat.

In the second method of proceeding, or that in which the nitrous air is added to the respirable, I found nearly the same difference in the diminution, according as the bottle was shaken briskly or gently, as in the former method: I found also nearly the same difference, or perhaps rather less, according to the nature of the water employed, only it seemed to be of not much consequence whether the water frothed or not; but there seemed to be much less difference in the diminution, according to the time which the air took up in passing into the bottle. The usual diminution on trying common air with different quantities of nitrous air, when distilled water was employed, was as follows:

Common air	Nitrous air	Diminution
1·	·6	·74
	·8	·88
	1·	·89
	1·5	·90

It appears, therefore, that $\frac{8}{10}$ths of a measure of nitrous air is sufficient to produce very nearly the full diminution. I chuse, however, always to use one measure. It appears also, that the diminution is always much less in this method than when the common air is added to the nitrous; as in that method it was before said, that the usual diminution was 1·08. The reason of this is, that when nitrous and common air are mixed together, the nitrous air is robbed of part of its phlogiston, and is thereby turned into phlogisticated nitrous acid, and is absorbed by the water in that state, and besides that, the common air is phlogisticated, and thereby diminished: so that the whole diminution on mixing is equal to the bulk of nitrous air, which is turned into acid, added to the diminution which the common air suffers by being phlogisticated. Now it appears, that when a small quantity of nitrous air comes in contact with a large quantity of common air, it is more completely deprived of its phlogiston, and is absorbed by the water in a more dephlogisticated state than when a small quantity of common air comes in contact with a large quantity of nitrous; consequently, in the second method, where small portions of nitrous air come in contact with a large quantity of common air, the nitrous air is more deprived of its phlogiston, and therefore a less quantity of it is required to phlogisticate the common air than in the first method, where small portions of common air come in contact with a large quantity of nitrous air; so that a less quantity of the nitrous air is absorbed in the second method than in the first. As to the common air, as it is completely phlogisticated in both methods, it most likely suffers an equal diminution in both.

A clear proof that a less quantity of nitrous is required to phlogisticate a given quantity of common air in the second method than in the first, is, that if common air is mixed with a quantity of nitrous air not sufficient to completely phlogisticate it, the mixture will be more phlogisticated if the nitrous air is added slowly to the common, as in the second method, than if the common air is added to the nitrous; and if the nitrous air is added slowly to the common, without being in contact with water, the mixture will be found to be still more phlogisticated than in the second method, where the two airs are in contact with water at the time of mixing.

The following table contains the result of the experiments I have made on this subject.

First method			Second method			Nitrous air added slowly to common without being in contact with water		
Nitrous air	Bulk of mixture	Test	Nitrous air	Bulk of mixture	Test	Nitrous air	Bulk of mixture	Test
·716	·856	·244	·635	·849	·137			
·474	·915	·513	·430	·867	·352	·294	·836	·337
			·280	·930	·599			

The two-first sets of experiments were not tried with the apparatus above described, as that held too small a quantity, but with another upon the same principle. The last set was tried by the apparatus represented in Fig. 4 where *A* is a bottle containing nitrous air, inverted into the tub of water *DE*; *B* is a bottle with a bent glass tube *C* fitted to its mouth. This bottle is filled with common air, without any water, and is first slightly warmed by the hand; the end of the glass tube is then put into the bottle of nitrous air, as in the figure; consequently, as the bottle *B* cools, a little nitrous air runs into it, which, by the common air in it, is deprived of its elasticity, so that more nitrous air runs in to supply its place. By this means the nitrous air is added slowly to the common without coming in contact with water, till the whole of the nitrous air has run out of the bottle *A* into *B*; then, indeed, the water runs through the glass tube into *B*, to supply the vacancy formed by the diminution of the common air.

It appears from the foregoing table, that a quantity of nitrous air, used in the first method, does not phlogisticate common air more than three-fourths of that quantity used in the second way does, and not so much as half that quantity used in the third way: so that we may safely conclude, that it is this circumstance of the nitrous air going further in phlogisticating common air in some circumstances than others, which is the cause that the diminution in trying the purity of air by the nitrous test is so much greater in some methods of mixing them than in others.

From what was said in p. [135] it should seem as if the second method was more exact than the first, as the error proceeding from the air employing more or less time in passing into the bottle was found to be less, and that proceeding from a difference in the water, and from the bottle being shaken more or less strongly was not greater. I, however, have found, that the trials of the same air on the same day have commonly differed more when made in this manner than in the first; for which reason, and because in trying common air the first method takes up the least time, I have commonly used that.

It should be observed, that in trying dephlogisticated air by the first method it is convenient to use different bottles, according to the different purity of the air; and the same air will appear purer, if tried by a larger bottle than by a smaller. For example, if its test, tried by the large bottle, comes out 2·54, it will appear not more than 2·44, if tried by the middle bottle; and, in like manner, if its test by the middle bottle comes out 1·11, it will appear to be about 1·08, if tried by the least bottle; for this reason it is right always to set down which bottle it is tried by.

I think I may confidently assert, that either of the above methods are considerably more accurate than Fontana's, supposing the experiment to be made exactly in his manner, that is, determining the quantities by measure. But, in order to judge which method of mixing the airs is most exact, it was necessary to determine the quantities in his method also by weight, as otherwise it would be uncertain whether my method of mixing the airs is really better than his, or whether the apparent greater exactness proceeds only from the superiority of weighing above measuring: for this reason I made some experiments in which common and nitrous air were mixed in his manner, except that I used only one measure of each, as Dr. Ingen-Housz did, and that the nitrous air was put up first, the true diminution being determined by weight, by first weighing the tube under water with the nitrous air in it, and then adding the common air, and weighing the tube again under water. It was unnecessary, for the reasons given in [pp. 130 and 131], to determine the quantity of either the nitrous or common air by weight. My reason for this variation was, that it afforded a much easier method of determining the quantities by weight, was less trouble, and, I believe, must be at least as exact: for I have always found, that the experiments made with the Abbé Fontana's apparatus, in which I used only one measure of each air, agreed better together than those in which I used two of common, and added the nitrous air by one at a time; and I imagine it can be of no signification whether the nitrous or common air is put in first, as I cannot perceive the diminution to be sensibly greater in one of those ways than the other[1].

From the result of these experiments I am persuaded, that my method of mixing the airs is really rather more accurate than Fontana's, as in trying the same bottle of air six or seven times in my method the different trials would not often differ more than $\frac{1}{200}$dth part, and very seldom more than $\frac{1}{100}$dth; whereas in his there would commonly be a difference of $\frac{1}{100}$dth, and frequently near twice that quantity, though I endeavoured to be as regular as I could in my manner of trying the experiment. My method also certainly requires less dexterity in the operator than his.

[1] It is not extraordinary, that in this method the diminution is just the same whether the common or nitrous air is put up first, notwithstanding that in mine it is very different; since in this method the two airs mix in the same manner whichever is put up first: whereas in mine, the manner in which they mix is very different in those two cases; as in one, small portions of common air come in contact with large portions of the nitrous; and in the other, small portions of nitrous air come in contact with large portions of common air.

It is of much importance towards forming a right judgement of the degree of accuracy to be expected in the nitrous test, to know how much it is affected by a difference in the nitrous air employed. Now it must be observed, that nitrous air may differ in two respects; first, it may vary in purity, that is, in being more or less mixed with phlogisticated or other air; and, secondly, it is possible, that out of two parcels equally pure one may contain more phlogiston than the other. If it differs in the second respect, it will evidently cause an error in the test, in whatever proportion it is mixed with the respirable air; but if it differs only in the first respect, it will hardly cause any sensible error, unless it is more than usually impure, provided care is taken to use such a quantity as is sufficient to produce the full diminution. This has been observed by the Abbé Fontana, and agrees with my own experiments; for the test of common air tried in my usual method, with some nitrous air which had been debased by the mixture of common air, came out only 18 thousandths less than when tried with air of the best quality, though this air was so much debased that the diminution, on mixing two parts of this with five of common, was one-sixth part less than when good nitrous air was employed; which shews, that the error proceeding from the difference of purity of the nitrous air is much less when it is used in the full quantity than in a smaller proportion; and also shews, that if it is used in the full quantity it can hardly cause any sensible error, unless it is more impure than usual. One does not easily see, indeed, why it should cause any error; for no reason appears why the mixture of phlogisticated or other air, not absorbable by water, and not affected by respirable air, should prevent the nitrous air from diminishing and being diminished by the respirable air in just the same manner that it would otherwise be. It must be observed, however, that if the nitrous air is mixed with fixed air, it will cause an error, as part of the fixed air will be absorbed by the water while the test is trying; for which reason care should be taken that the nitrous air should not be much mixed with this substance, which it will hardly be, unless either the metal it is procured from is covered with rust; or unless the water in which it is received contains much calcareous earth suspended by fixed air, as in that case, if any of the nitrous acid comes over with the air, it will dissolve the calcareous earth, and separate some fixed air.

In order to see whether it is possible for nitrous air to differ in the second respect, I procured some from quicksilver, copper, brass, and iron, and observed the test of the same parcel of common air with them, on the same day, making four trials with each, when the difference between the tests tried with the three first kinds of air was not greater than might proceed from the error of the experiment; but those tried with the air from iron were $\frac{15}{1000}$ths greater than the rest. I then took the test of some more common air with them in the same manner, only using four parts of common to one of nitrous air, when the tests tried with the air from iron came out smaller than the rest by not less than $\frac{130}{1000}$ths. It should seem, therefore, from these experiments, that the nitrous air procured from iron, besides being much more impure than the others, differs from them also in the second respect; that is, that the pure nitrous air in it contains rather less phlogiston than that in the others: whence it happens, that a greater quantity is necessary to phlogisticate a given portion of common air, and consequently that the diminution is greater when a sufficient quantity of it is used, though with a less proportion the diminution is much less than with other nitrous air, on account of its greater impurity. As for the air procured from the three other substances, I cannot be sure that there is any difference between them. The nitrous air I always use is made from copper, as it is procured with less trouble than from quicksilver, and I have no reason to think it more likely to vary in its quality.

During the last half of the year 1781, I tried the air of near 60 different days, in order to find whether it was sensibly more phlogisticated at one time than another; but found no difference that I could be sure of, though the wind and weather on those days were very various; some of them being very fair and clear, others very wet, and others very foggy.

My way was to fill bottles with glass stoppers every now and then with air from without doors, and preserve them stopped and inverted into water, till I had got seven or eight, and then take their test; and whenever I observed their test, I filled two bottles, one of which was tried that day, and the other was kept till the next time of trying, in order to see how nearly the test of the same air, tried on different days, would agree. The experiment was always made with distilled water, and care was always taken to observe the diminution which nitrous air suffered by being shaken in the water, as mentioned in p. [133]. The heat of the water in the tub also was commonly set down. Most of the bottles were tried only in the first method; but some of them were also tried by the second, and by the method just described in the manner of Fontana.

The result was, that the test of the different bottles tried on the same day never differed more than ·013, and in general not more than half that quantity. The test, indeed, of those tried on different days differed rather more; for taking a mean between the tests of the bottles tried on the same day, there were two of those means which differed ·025 from each other; but, except those two, there were none which differed more than ·013. Though this difference is but small, yet as each of these means is the mean of seven or eight trials, it is greater than can be expected to proceed from the usual errors of the experiment. This difference also is not much diminished by correcting the observations on account of the heat and absorbing power of the water, according to the rule in p. [134]. This might incline one to think, that the parcels of air examined on some of those days of trial were really more dephlogisticated than the rest; but yet, I believe, that they were not: for whenever there was any considerable difference between the means of two successive days of trial, there was nearly the same difference between the tests of the two bottles of the very same air tried on those two days. For example, the mean of the trials on July 7 was ·016 less than that of those on the 15th of the same month; but then the test of the air caught and tried on the 7th was equally less than that of the air of the same day tried on the 15th; which shews, that this difference between the means of those two days was not owing to the parcels of air tried on the former day being really more dephlogisticated than those tried on the latter, but only to some unperceived difference in the manner of trying the experiment; or else to some unknown difference in the nature of the water or nitrous air employed. A circumstance which seems to shew that it was owing to the first of these two causes is, that it frequently happened, that on those days in which the tests taken in the first method came out greater than usual, those taken in Fontana's manner, or in the second method, did not do so; the trials, however, made in these two methods were too few to determine any thing with certainty. On the whole there is great reason to think, that the air was in reality not sensibly more dephlogisticated on any one of the sixty days on which I tried it than the rest.

The highest test I ever observed was 1·100, the lowest 1·068, the mean 1·082.

I would by all means recommend it to those who desire to compare the air of different places and seasons, to fill bottles with the air of those places, and to try them at the same time and place, rather than to try them at the time they were filled, as all the errors to which this experiment is liable, as well those which proceed from small differences in the manner of trying the experiment, as those which proceed from a difference in the nature of the water and nitrous air, will commonly be much less when the different parcels of air are tried at the same time and place than at different ones; provided only, that air can be kept in this manner a sufficient time without being injured, which I believe it may, if the bottles are pretty large, and care is taken that they, as well as the water used in filling them with air, are perfectly clean. I have tried air kept in the abovementioned manner for upwards of three-quarters of a year in bottles holding about a pint, which I have no reason to think was at all injured; but then I have tried some kept not more than one-third part of that time which seemed to have been a little impaired, though I do not know what it could be owing to, unless it was that the bottles were smaller, namely, holding less than one-fourth of a pint, and that in all of them, except two, which were smaller than the rest, the stopper which, however, fitted in very tight, was tied down by a piece of bladder.

I made some experiments also to try whether the air was sensibly more dephlogisticated at one time of the day than another, but could not find any difference. I also made several trials with a view to examine whether there was any difference between the air of London and the country, by filling bottles with air on the same day, and nearly at the same hour, at Marlborough-street and at Kensington. The result was, that sometimes the air of London appeared rather the purest, and sometimes that of Kensington; but the difference was never more than might proceed from the error of the experiment; and by taking a mean of all, there did not appear to be any difference between them. The number of days compared was 20, and a great part of them taken in winter, when there are a greater number of fires, and on days when there was very little wind to blow away the smoke.

It is very much to be wished, that those gentlemen who make experiments on factitious airs, and have occasion to ascertain their purity by the nitrous test, would reduce their observations to one common scale, as the different instruments employed for that purpose differ so much, that at present it is almost impossible to compare the observations of one person with those of another. This may be done, as there seems to be so very little difference in the purity of common air at different times and places, by assuming common air and perfectly phlogisticated air as fixed points. Thus, if the test of any air is found to be the same as that of a mixture of equal parts of common and phlogisticated air, I would say, that it was half as good as common air; or, for shortness, I would say, that its standard was $\frac{1}{2}$: and, in general, if its test was the same as that of a mixture of one part of common air and x of phlogisticated air, I would say, that its standard was $\frac{1}{1+x}$. In like manner, if one part of this air would bear being mixed with x of phlogisticated air, in order to make its test the same as that of common air, I would say, that it was $1 + x$ times as good as common air, or that its standard was $1 + x$; consequently, if common air, as Mr. Scheele and La Voisier suppose, consists of a mixture of dephlogisticated and phlogisticated air, the standard of any air is in proportion to the quantity of pure dephlogisticated air in it. In order to find what test on the Eudiometer answers to different standards below that of common air, all which is wanted is to mix common and perfectly phlogisticated air in different proportions, and to take the test of those mixtures; but in standards above that of common air, it is necessary to procure some good dephlogisticated air, and to find its standard by trying what proportion of phlogisticated air it must be mixed with, in order to have the same test as common air, and then to mix this dephlogisticated air with different proportions of phlogisticated air, and find the test of those mixtures[1].

[1] The rule for computing the standard of any mixture of dephlogisticated and phlogisticated air is as follows. Suppose the test of a mixture of D parts of de-

On this principle I found the standard answering to different tests on both my Eudiometers, and also on Fontana's, to be as follows:

Standard	Test by first method	Test by second method	Test by Fontana abridged 1	2	3	4	Total diminution
4·8	5·02	3·62	·73	·44	·13	1·02	3·98
3·61	3·72	2·70	·75	·49	1·00	—	3·
2·39	2·55 by large bottle 2·45 by middle bottle	1·87	·76	·96	1·92	—	2·08
1·00	1·11 by middle bottle 1·08 by least bottle	·89	1·00	—	—	—	1·00
·75	·81	·69	1·23	—	—	—	·77
·5	·57	·51	1·45	—	—	—	·55
·25	·32	·31	1·66	—	—	—	·34
·0	·07	·08	1·94	—	—	—	·06

Standard	Test by Fontana's method 1	2	3	4	5	6	7	Total diminution
4·8	1·75	1·43	1·11	·78	·46	·21	1·18	7·82
3·61	1·75	1·46	1·17	·89	1·16	2·13	—	5·87
2·39	1·76	1·50	1·25	2·06	—	—	—	3·94
1·	1·81	2·12	3·12	—	—	—	—	1·88
·75	1·82	2·54	—	—	—	—	—	1·46
·5	1·98	2·94	—	—	—	—	—	1·06
·25	2·42	3·39	—	—	—	—	—	·61
·0	2·91	—	—	—	—	—	—	·09

The phlogisticated air used in these experiments was procured by means of liver of sulphur.

The trials, called Fontana abridged, were made in the Abbé Fontana's manner, except that only one measure of respirable air was used, the nitrous air being added by one measure at a time as usual. The column marked 1 at top is the bulk of the mixture after one measure of nitrous air was added; that marked 2, its bulk after two measures were added, and so on.

It must be observed, that in these experiments a considerable diminution took place in taking the test of the unmixed phlogisticated air, or that whose standard is marked 0 in the table; but, notwithstanding this, the air, as far as I could perceive, was perfectly phlogisticated, the diminution being caused merely by the absorbtion of the nitrous air by the water.

phlogisticated air with P of phlogisticated air is the same as that of common air, then is the standard of the dephlogisticated air $\frac{D+P}{D}$. Let now δ parts of this dephlogisticated air be mixed with ϕ parts of phlogisticated air, the standard of the mixture will be $\frac{D+P}{D} \times \frac{\delta}{\delta + \phi}$.

What shews this to be the case is, that if common and nitrous air are mixed in such proportions as that the nitrous should be predominant, so as to be considerably diminished by the mixture of common air, this mixture will produce as great a diminution with nitrous air as the phlogisticated air used in these experiments; and if plain nitrous air is added to nitrous air, the diminution is still greater. This shews, that a considerable diminution is produced by mixing perfectly phlogisticated air with nitrous air, and also that air may be perfectly phlogisticated by liver of sulphur.

These experiments also shew the necessity of using such a quantity of nitrous air as is sufficient to produce the full diminution, in order to form a proper estimate of the goodness of air; for if the quantity of nitrous air is much less than that, the air you try will appear very little better than air of a much inferior quality. For example, if in taking the test of very good dephlogisticated air, only an equal bulk of nitrous air is used, it will appear very little better than a mixture of equal parts of this and phlogisticated air; and if twice that quantity of nitrous air is used, it will appear very little better than a mixture of three parts of this air with one of phlogisticated. Another great advantage of using the full quantity of nitrous air is, that thereby the error arising from any difference in its purity is very much diminished.

Perfectly phlogisticated air may be conveniently procured by putting some solution of liver of sulphur into a bottle of air well stopped, and shaking it frequently till the air is no longer diminished, which, unless it is shaken very frequently, will take up some days. Care must be taken, however, to loosen the stopper now and then, so as to let in air to supply the place of the diminished air. In order to know when the air is as much diminished as it can be, the best way is, when the air is supposed to be nearly phlogisticated, to place the bottle with its mouth under water, still keeping it stopped, and to loosen the stopper now and then, while under water, so as to let in water to supply the place of the diminished air, by which means the alteration of weight of the bottle shews whether the air is diminished or not. If the solution of liver of sulphur is made by boiling together fixed alkali, lime, and flowers of sulphur, which is the most convenient way of procuring it, the air phlogisticated by it will be perfectly free from fixed air: whether it will be so if the liver of sulphur is made without lime, I am not sure.

A still more convenient way, however, of procuring phlogisticated air is by a mixture of iron filings and sulphur; and, as far as I can perceive, the air procured this way is as completely phlogisticated as that prepared by liver of sulphur.

Where the impurities mixed with the air have any considerable smell, our sense of smelling may be able to discover them, though the quantity is vastly too small to phlogisticate the air in such a degree as to be perceived by the nitrous test, even though those impurities impart their phlogiston to the air very freely. For instance, the great and instantaneous power of nitrous air in phlogisticating common air is well known; and yet ten ounce measures of nitrous air, mixed with the air of a room upwards of twelve feet each way, is sufficient to communicate a strong smell to it, though its effect in phlogisticating the air must be utterly insensible to the nicest Eudiometer; for that quantity of nitrous air is not more than the 140000th part of the air of the room, and therefore can hardly alter its test by more than $\frac{3}{1400000}$ or $\frac{1}{470000}$th part. Liver of sulphur also phlogisticates the air very freely, and yet the air of a room will acquire a very strong smell from a quantity of it vastly too small to phlogisticate it in any sensible degree. In like manner it is certain, that putrifying animal and vegetable substances, paint mixed with oil, and flowers, have a great tendency to phlogisticate the air; and yet it has been found, that the air of an house of office, of a fresh painted room, and of a room in which such a number of flowers were kept as to be very disagreeable to many persons, was not sensibly more phlogisticated than common air. There is no reason to suppose from these instances, either that these substances have not much tendency to phlogisticate the air, or that nitrous air is not a true test of its phlogistication, as both these points have been sufficiently proved by experiment; it only shews, that our sense of smelling can, in many cases, perceive infinitely smaller alterations in the purity of the air than can be perceived by the nitrous test, and that in most rooms the air is so frequently changed, that a considerable quantity of phlogisticating materials may be kept in them without sensibly impairing the air. But it must be observed, that the nitrous test shews the degree of phlogistication of air, and that only; whereas our sense of smelling cannot be considered as any test of its phlogistication, as there are many ways of phlogisticating air without imparting much smell to it; and, I believe, there are many strong smelling substances which do not sensibly phlogisticate it.

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XX. *Observations on Mr.* Hutchins's *Experiments for determining the Degree of Cold at which Quicksilver freezes. By* Henry Cavendish, *Esq., F.R.S.*

Read May 1, 1783

THE design of the following paper is to explain some particulars in the apparatus sent by me to Mr. Hutchins, the intention of which does not readily appear; and also to endeavour to shew the cause of some phenomena which occurred in his experiments; and point out the consequences to be drawn from them.

This apparatus was intended to determine the precise degree of cold at which quicksilver freezes: it consisted of a small mercurial thermometer, the bulb of which reached about 2½ inches below the scale, and was inclosed in a glass cylinder swelled at bottom into a ball, which, when used, was filled with quicksilver, so that the bulb of the thermometer was intirely surrounded with it. If this cylinder is immersed in a freezing mixture till great part of the quicksilver in it is frozen, it is evident, that the degree shewn at that time by the inclosed thermometer is the precise point at which mercury freezes; for as in this case the ball of the thermometer must be surrounded for some time with quicksilver, part of which is actually frozen, it seems impossible, that the thermometer should be sensibly above that point; and while any of the quicksilver in the cylinder remains fluid, it is impossible that it should sink sensibly below it. The ball of the thermometer was kept constantly in the middle of the swelled part of the cylinder, without danger of ever touching the sides, by means of some worsted wound round the tube. This worsted also served to prevent the access of the air to the quicksilver in the cylinder, which, if not prevented, would have made it more difficult to have communicated a sufficient degree of cold. The diameter of the bulb of the thermometer was rather less than one-fourth of an inch, that of the swelled part of the cylinder was two-thirds, so that there was nowhere a much less thickness of quicksilver between the ball and cylinder than one-sixth of an inch. The bulb of the thermometer was purposely made as small as it conveniently could, in order to leave a sufficient space between it and the cylinder, without making the swelled part thereof larger than necessary, which would have caused more difficulty in freezing the quicksilver in it. Two of these instruments were sent for fear of accidents.

One of the most striking circumstances in the experiments which have been made for freezing mercury, is the excessively low degree to which the thermometers sunk, and which, if it had proceeded, as was commonly supposed from the freezing mixture having actually produced such a degree of cold, would have been really astonishing. The experiments, however, made at Petersburg afforded the utmost reason to suppose, and Mr. Hutchins's last experiments have put beyond a possibility of doubt, that quicksilver contracts in the act of freezing, or in other words, that it takes up less room in a solid than in a fluid state; and that the very low degree to which the thermometers sunk was owing to this contraction, and not to the intensity of the cold produced: for example, in one of Mr. Hutchins's experiments a mercurial thermometer, placed in the freezing mixture, sunk to 450° below nothing, though the cold of the mixture was never more than $-46°$; so that the quicksilver was contracted not less than 404° by the action of freezing.

If a glass of water, with a thermometer in it, is exposed to the cold, the thermometer will remain perfectly stationary from the time the water begins to freeze till it is intirely congealed, and will then begin to sink again. In like manner, if a thermometer is dipped into melted tin or lead, it will remain perfectly stationary, as I know by experience, from the time the metal begins to harden round the edges of the pot till it is all become solid, when it will again begin to descend; and there was no reason to doubt that the same thing would obtain in quicksilver.

From what has been just said it was concluded, that if this apparatus was put into a freezing mixture of a sufficient coldness, the thermometer would immediately sink till the quicksilver in the cylinder began to freeze, and would then continue stationary, supposing the mixture still to keep cold enough, till it was intirely congealed. This stationary height of the thermometer is the point at which mercury freezes, though in order to make the experiment convincing, it was necessary to continue the process till so much of the quicksilver in the cylinder was frozen as to put the fact out of doubt.

If the experiment had been tried with no further precautions, I apprehended that considerable difficulties would have occurred, from want of knowing whether the cold of the mixture was sufficiently great, and when a sufficient quantity of the quicksilver was frozen; for, in the first place, there would be no judging when a sufficient quantity was frozen without taking out the apparatus now and then to examine it, which could not be done without a loss of cold; and what is still worse, if before the experiment was completed the cold of the mixture was so much abated as to become less than that of congealing mercury, the frozen quicksilver would begin to melt, and the operator would have no way of detecting it, but by finding that great part of his labour was undone. For this reason two other mercurial thermometers were sent called *A* and *B* by Mr. Hutchins, the scales of which were of wood, for which reason I shall call them, for shortness, the wooden thermometers, as I shall call the two others the ivory ones, their scales being of that material; they were graduated to about 600° below nothing, and their balls were nearly equal in diameter to the swelled part of the cylinders, in order that the quicksilver in both should cool equally fast; and it was recommended to Mr. Hutchins to put one of these into the freezing mixture along with the apparatus: for then, if the cold of the mixture was sufficient, both thermometers would sink fast till the quicksilver in the cylinder began to freeze, when the ivory thermometer would become stationary, but the wooden one would still continue to sink, on account of the contraction of the quicksilver in its ball by freezing; but if this last thermometer, after having continued to sink for some time after the ivory one had become stationary, ceased at last to descend, it would shew, that the mixture was no longer cold enough to freeze mercury; for as long as that was the case, the wooden thermometer would continue to descend by the freezing of fresh portions of quicksilver in its ball, but would cease to do so as soon as the cold was at all less than that. As I was afraid, however, that the quicksilver might possibly freeze and stick tight in the tube of this thermometer, and prevent its sinking, which would make the cold of the mixture appear too small when in reality it was not, one of these thermometers instead of having a vacuum above the quicksilver as usual, was made with a bulb at top filled with air, in order that the pressure might serve to force down the quicksilver.

If the degree of cold at which mercury freezes had been known, a spirit thermometer would have answered better; but that was the point to be determined.

Another advantage which I expected from the wooden thermometer was, that it would afford a guess when a sufficient quantity of the quicksilver in the cylinder was frozen; for if the cold was continued long enough to make that thermometer sink to near 400° below nothing, I supposed, a very visible portion of the quicksilver would be frozen.

It must be observed, however, that in Mr. Hutchins's experiments the natural cold approached so near to the point of mercurial congelation, and in consequence the freezing mixture retained its cold so long as to make these precautions of not so much use as they would otherwise have been.

As it appeared, from Mr. Hutchins's table of comparison, that these thermometers did not agree well together, they were all examined after they came back, except the ivory thermometer *F*, which was broke before it arrived. This loss, however, is of little consequence, as it appeared from the abovementioned table, that *F* and *G* agreed well together. The boiling and freezing points were first examined in the presence of Sir Joseph Banks, Dr. Blagden, Mr. Hutchins, Mr. Nairne, and myself, when the divisions on the scale answering thereto were found to be as follows:

	Boiling point	Freezing point
A	220·3	29·9
B	218·8	30·9
G	215·3	32

The boiling point was tried in the manner recommended in the report of the Committee of the Royal Society, printed in the *Philosophical Transactions* for the year 1777, and allowance made, as there directed, for the height of the barometer at that time. In fixing the freezing point also allowance was made for the temperature of the room in which it was tried.

The great difference in the position of the boiling point on these thermometers seems owing only to care not having been taken to keep the quicksilver in the tube of the same heat as that in the ball, which is a circumstance that was very little attended to when they were made; and I am afraid is not so much observed at present as it ought to be, and which in *A* and *B*, whose tubes contained upwards of 900° of quicksilver, caused an excessively great error, and much more than it did in *G*, which contained fewer degrees in its tube.

In order to see whether the inequalities of the bore of the tube were properly allowed for, a column of quicksilver, about 100° long, was separated from the rest; and it was examined, whether its length comprehended the same number of degrees on the scale in different parts of the tube; when no sensible error could be found in this respect in *G*, and none worth regarding in *B*. The thermometer *A*, by reason of its being constructed with a bulb filled with air at top, could not be examined in this manner; but there is no reason to think, that it was faulty in this respect.

From what has been said it appears, that 183°·3 on the scale of *G* are equal to only 180° on a thermometer adjusted as recommended by the Committee, and therefore 72° are equal to $70°\frac{2}{3}$; so that the point of $-40°$ answers really to $-38°\frac{2}{3}$; that is, the cold shewn by this thermometer at the temperature of about $-40°$ is $1°\frac{1}{3}$ too great. In like manner it appears, that the cold shewn at that temperature by *B* is $4°\frac{1}{3}$, and by *A* $6°\frac{1}{3}$, too great.

On the whole, these thermometers seem to have been carefully made, their disagreement being owing only to a faulty manner of adjusting the boiling point, and to not allowing for the temper of the air in settling the degree of freezing; and as these points were examined after they came back, the experiments made with them are just as much to be depended on as if they had been truly adjusted at first.

These instruments were made in the year 1776, and were intended to have been sent to Mr. Hutchins that year, through the hands of the late

Dr. Maty, who promised to recommend the experiment to him; but, by not being got ready time enough to be sent that year, and a mistaken supposition that Mr. Hutchins was to come back the next summer, they were prevented from being sent till 1781; when Sir Joseph Banks was informed by Mr. Wegg, that there was a gentleman at Hudson's Bay who was willing to undertake any experiments of that kind; and that the Hudson's Bay Company would be at the expence of any instruments necessary for the purpose. Then, as Sir Joseph thought the abovementioned apparatus well adapted to the purpose, I gladly embraced the opportunity of sending it. It appears, however, from the letter inserted by Mr. Hutchins, that Dr. Black, without being acquainted with what I had done, recommended nearly the same method of determining the degree of cold at which mercury freezes.

Besides the abovementioned instruments, there were sent to Mr. Hutchins two spirit thermometers and a thermometer marked *C*, made at the expence of the Hudson's Bay Company. The two spirit thermometers were made at the recommendation, and under the inspection of Dr. Blagden, and were of great use, as they serve to ascertain several circumstances relating to the experiments, which could not otherwise have been determined. The intention of the thermometer *C* will be mentioned in the course of this paper.

Before I enter into the examination of Mr. Hutchins's experiments, it will be proper to take notice of a phenomenon which occurs in the freezing of water, and is now found to take place in that of quicksilver, and which occasioned many remarkable appearances in these experiments.

It is well known, that if a vessel of water, with a thermometer in it, is exposed to the cold, the thermometer will sink several degrees below the freezing point, especially if the water is covered up so as to be defended from the wind, and care is taken not to agitate it; and then, on dropping in a bit of ice, or on mere agitation, spiculæ of ice shoot suddenly through the water, and the inclosed thermometer rises quickly to the freezing point where it remains stationary[1].

This shews, that water is capable of being cooled considerably below the freezing point, without any congelation taking place; and that, as soon as by any means a small part of it is made to freeze, the ice spreads rapidly

[1] Though I here say conformably to the common opinion, that mere agitation may set the water a freezing, yet some experiments, lately made by Dr. Blagden, seem to shew, that it has not much, if any, effect of that kind, otherwise than by bringing the water in contact with some substance colder than itself. Though in general also the ice shoots rapidly, and the inclosed thermometer rises very quick; yet I once observed it to rise very slowly, as, to the best of my remembrance, it took up not less than half a minute before it rose to the freezing point; but in this experiment the water was cooled not more than one or two degrees below freezing; and it should seem, that the more the water is cooled below that point, the more rapidly the ice shoots, and the inclosed thermometer rises.

through the remainder of the water. The cause of the rise of the thermometer, when the water begins to freeze, is the circumstance now pretty well known to philosophers, that all, or almost all, bodies by changing from a fluid to a solid state, or from the state of an elastic to that of an unelastic fluid, generate heat; and that cold is produced by the contrary process. This explains all the circumstances of the phenomenon perfectly well; for as soon as any part of the water freezes, heat will be generated thereby in consequence of the abovementioned law, so that the new formed ice and remaining water will be warmed, and must continue to receive heat by the freezing of fresh portions of water, till it is heated exactly to the freezing point, unless the water could become quite solid before a sufficient quantity of heat was generated to raise it to that point, which is not the case; and it is evident, that it cannot be heated above the freezing point, for as soon as it comes thereto, no more water will freeze, and consequently no more heat will be generated.

The reason why the ice spreads all over the water, instead of forming a solid lump in one part, is, that as soon as any small portion of ice is formed, the water in contact with it will be so much warmed as to be prevented from freezing; but the water at a little distance from it will still be below the freezing point, and will consequently begin to freeze.

If it was not for this generation of heat by the act of freezing, whenever a vessel of water, exposed to the cold, was arrived at the freezing point, and began to freeze, the whole would instantly be turned into solid ice; for as the new formed ice is not sensibly colder than water beginning to freeze, it follows, that as soon as all the water in the vessel was cooled to that point, the least addition of cold would convert the whole into ice; whereas it is well known, that though the whole vessel of water is cooled to, or even below, the freezing point, there is a long interval of time between its beginning to freeze and being intirely frozen, during all which time it does not grow at all colder.

In like manner, it is the cold generated by the melting of ice which is the cause of the long time required to thaw ice or snow. It is this also which is the cause of the cold produced by freezing mixtures; for no cold is produced by mixing snow with any substance, unless part of the snow is dissolved.

I formerly found, by adding snow to warm water, and stirring it about till all was melted, that the water was as much cooled as it would have been by the addition of the same quantity of water, rather more than 150° colder than the snow; or, in other words, somewhat more than 150° of cold are generated by the thawing of snow; and there is great reason to think, that just as much heat is produced by the freezing of water. The cold generated was exactly the same whether I used ice or snow[1].

[1] I am informed, that Dr. Black explains the abovementioned phenomena in the same manner; only, instead of using the expression, heat is generated or produced,

I have formerly kept a thermometer in melted tin and lead till they became solid; the thermometer remained perfectly stationary from the time the metal began to harden round the sides of the pot till it was intirely solid; but I could not perceive it to sink at all below that point, and rise up to it when the metal began to harden. It is not unlikely, however, that the great difference of heat between the air and melted metal might prevent this effect from taking place; so that though I did not perceive it in those experiments, it is not unlikely that those metals, as well as water and quicksilver, may bear being cooled a little below the freezing or hardening point (for the hardening of melted metals and freezing of water seems exactly the same process) without beginning to lose their fluidity.

Mr. Hutchins's five first experiments were made with the apparatus, and in the manner above described. In the first experiment the ivory thermometer, inclosed in the cylinder, sunk to $-40°$, where it remained stationary for about half an hour, though the wooden thermometer, placed in the same mixture, kept sinking almost all the while. At the end of that time the apparatus was taken out of the mixture to be examined, and the quicksilver in the cylinder was found frozen. It seems evident, therefore, that the true point at which mercury freezes is 40° below nothing on the thermometer *F*, which was that made use of in the experiment. It cannot be lower than that, for if it was, the thermometer could not have remained so long stationary at that point, while surrounded with freezing quicksilver; and it cannot be higher, as the thermometer could not sink below the freezing point, while much of the quicksilver, with which it was surrounded, remained unfrozen.

To those who have attended to the former part of this paper it is needless saying, that the reason why the wooden thermometer continued sinking so long after the ivory thermometer became stationary is, that as the former was placed in the freezing mixture, the quicksilver in its ball froze, and therefore it continued descending during the greatest part of that half hour, by the continual freezing of fresh portions of quicksilver in its ball, and the contraction occasioned thereby; whereas the latter, which was placed only in freezing quicksilver, did not freeze.

There is a circumstance, however, in this experiment, the reason of which does not so readily appear; namely, on putting back the apparatus

he says, latent heat is evolved or set free; but as this expression relates to an hypothesis depending on the supposition, that the heat of bodies is owing to their containing more or less of a substance called the matter of heat; and as I think Sir Isaac Newton's opinion, that heat consists in the internal motion of the particles of bodies, much the most probable, I chose to use the expression, heat is generated. Mr. Wilke also, in the *Transactions of the Stockholm Academy of Sciences*, explains the phenomena in the same way, and makes use of an hypothesis nearly similar to that of Dr. Black. Dr. Black, as I have been informed, makes the cold produced by the thawing of snow 140°; Mr. Wilke, 130°.

into the freezing mixture, after it was taken out to be examined, the thermometer sunk to − 42°; but in about four or five minutes returned back to − 40°. The like happened on removing the apparatus into a fresh freezing mixture, and it then remained about ten minutes before it returned to − 40°. It seems probable from this, that the quicksilver in the cylinder became intirely frozen about the time that it was first taken out to be examined, and that it then grew 2° colder than the freezing point; and that this degree of cold was not sufficient to make the quicksilver in the inclosed thermometer freeze, since mercury, as was before said, will bear being cooled a little below its freezing point without freezing. What confirms this explanation is, that the spirit thermometers shew that the cold of the mixture was actually much the same as that shewn by the ivory thermometer.

In the second experiment, tried with the same apparatus, the ivory thermometer quickly sunk to − 43°; but, in about half a minute, rose to − 40°, where it remained stationary for upwards of 17′. It appears, therefore, that in this experiment the quicksilver was cooled 3° below the freezing point, without losing its fluidity; it then began to freeze, and the inclosed thermometer immediately rose to − 40°: so that this experiment, besides confirming the former, shews, that quicksilver is capable of being cooled a little below the freezing point without freezing; and that it suddenly rises up to it as soon as it begins to lose its fluidity.

In this experiment the cold was carried far enough to freeze the quicksilver in the ivory thermometer, which was not the case in the former: for after it had remained 17′ stationary at − 40°, it began to sink again, and in about a minute sunk to − 44°½; it then sunk instantaneously to − 92°, and soon after remained fixed for an hour and a quarter at 95° (*sic*) [−95°]; being then left without examination for three-quarters of an hour, the mercury was found to have sunk into the ball, the spirit thermometer shewing at that time that the mixture was rather above the point of freezing, whereas before it had been below it. It appears, therefore, that the quicksilver in the thermometer, after having descended to − 44°½, froze in the tube, and stuck there; but, being by some means loosened, sunk instantly to − 92°, and again stuck tight at − 95°, till at last the mixture rising above the freezing point, the quicksilver in the tube melted, and sunk into the ball, to supply the vacuum formed there by the frozen quicksilver. A similar accident of the quicksilver freezing in the tube of the thermometer, and sticking there, and then melting and sinking into the ball as the weather grew warmer, has been found by Dr. Blagden to have happened to several gentlemen whose thermometers froze by the natural cold of the atmosphere, and with reason caused much perplexity to some of them.

In this experiment the apparatus was not taken out to be examined till the ivory thermometer had sunk to − 95°; it was then found to be frozen solid.

The third experiment was tried while the former was carrying on, and was made by putting the other apparatus, namely, that with the thermometers *G* and *B*, into the first mixture made for the former experiment, and which may consequently be supposed to have lost a great part of its cold. The ivory thermometer quickly sunk to $-43°$, where it remained stationary for near 12′. The apparatus being then taken out to be examined, the quicksilver in the cylinder was found fluid, but thick and in grains, like crumbs of bread. The apparatus was then put back into the mixture; and, on observing the thermometer, it was found to have risen to $-40°$, where it remained stationary about 40′; being then examined, the quicksilver was found solid.

It appears, therefore, that the cold of the mixture was sufficient to cool the quicksilver in the cylinder about 3° below the point of freezing, but did not make it freeze till, on taking out the apparatus, the agitation suddenly set it a freezing, and produced the appearance described by Mr. Hutchins. This immediately made the inclosed thermometer rise; so that when it was re-placed in the mixture and observed, it stood exactly at the freezing point. It appeared, by the spirit thermometer, that the cold of the mixture, at the time the apparatus was first taken out to be examined, was only 2° below the point of freezing, which agrees very well with this explanation.

This experiment, therefore, affords a fresh confirmation that the point of mercurial congelation is $-40°$ on these thermometers; and that quicksilver will bear being cooled a little below that point without freezing.

As in these two experiments the quicksilver in the cylinder and ivory thermometer bore being cooled a few degrees below the freezing point without freezing, it is natural to conclude, that the same fluid in the wooden thermometer should do so too; and it may, perhaps, be supposed that, in consequence of it, this thermometer, after having sunk a little below the point of freezing, ought suddenly to have risen up to it, which was not observed. But there is great reason to think, that though the quicksilver in it did bear cooling in this manner, it would not have occasioned any such appearance: for suppose that it is cooled below the freezing point, and then suddenly freezes, its bulk will be increased, on account of the heat generated thereby; but then it will be diminished on account of the contraction in freezing; so that, unless the expansion by the heat generated exceeds the contraction by freezing it will cause no rise in the thermometer. I do not, indeed, know how much the heat generated by freezing in quicksilver is, but in water it is about 150°, and the contraction by freezing is at least as much as its expansion by 400°; so that, unless the heat generated by freezing is two or three times as great in quicksilver as in water, the thermometer ought not to rise on this account.

In the fourth, fifth, sixth, and seventh experiments a new phenomenon occurred, namely, the ivory thermometer sunk a great deal below the freezing point without ever becoming stationary at $-40°$. In the fifth experiment, tried with the apparatus *G*, it quickly sunk to $-42°$, and then, without remaining stationary at any point, sunk in half a minute to $-72°$, and soon after remained fixed at $-79°$. While it was at $-79°$, the apparatus was twice examined, and the quicksilver found fluid; but being again examined after having been removed into a fresh mixture, it was found solid.

It seems likely from hence, that the quicksilver, in the cylinder was quickly cooled so much below the freezing point as to make that in the inclosed thermometer freeze, though it did not freeze itself. If so, it accounts for the appearances perfectly well; nor does there seem any thing improbable in the explanation, except that it is contrary to what happened in the three first experiments; but the degree to which fluids will bear being cooled below the freezing point without freezing seems to depend on such minute circumstances, that, I think, this forms no objection. It must be observed, that the cold of the mixture appeared by the spirit thermometer to be five or six degrees below the freezing point; so that if the quicksilver in the cylinder was as cold as the mixture, and I have no reason to think it was not, it is not at all extraordinary that the thermometer should have froze; the only thing extraordinary is, that the quicksilver in the cylinder should have borne that cold without freezing.

The same phenomenon occurred in the sixth and seventh experiments, on putting the same apparatus into the freezing mixture.

In the fourth experiment the ivory thermometer sunk quickly to $-42°$; but soon after rose half a degree, probably from the cold of the mixture diminishing; it then, after having remained six or seven minutes at those two points, sunk very quick to $-77°$. It does not appear, at what time the quicksilver in the cylinder began to freeze, as it was not examined till long after the thermometer had sunk to $-77°$, when it was found solid; but from the resemblance of this to the three former experiments, I think it much most likely, that it did not begin to freeze till after the thermometer had sunk to $-77°$.

In the fifth experiment the wooden thermometer was partly frozen before it was put into the freezing mixture, and the ivory one was at $-40°$. On putting them into the mixture, they both rose; the latter, half a degree; the former, many degrees; which shews that the part of the mixture in which they were placed was rather warmer than the freezing point, though that in which the spirit thermometer was placed was colder; but as there seems nothing to be learnt from this, it is not worth while entering into a detail of the circumstances.

Though these experiments do not serve to shew what the freezing point of quicksilver is, yet they do not at all contradict the conclusion drawn from the three former.

If these experiments only had been made, I should have been inclined to suppose, that quicksilver froze with a less degree of cold in vacuo than in the open air, as the quicksilver in the ivory thermometer was in vacuo, and that in the cylinder was not; but, as in the three former experiments, the event was different, the quicksilver in the cylinder there freezing first, I have no reason to think that this is the case.

Though in the sixth experiment the thermometer in the apparatus *G* froze without the quicksilver with which it was surrounded freezing, yet in trying the apparatus *F* in the same mixture, this did not happen; but, on the contrary, it afforded as striking a proof that the point of freezing quicksilver answers to about $-40°$ on this thermometer as any of Mr. Hutchins's experiments; for, on taking out the apparatus after it had been two minutes in the mixture, the quicksilver in the cylinder was found frozen solid, the inclosed thermometer standing at 40° or 41° below nothing. After having been exposed for near an hour to the air, which was then very little above the point of freezing quicksilver, only a small quantity of the surface was become fluid; the rest formed a frozen globe round the ball of the thermometer, resembling polished silver, and in 17′ after this only a segment of a globe of frozen quicksilver, with a concavity on the inside, formed by the ball of the thermometer, was observed, the thermometer all this while continuing the same as before, namely, at 40° or 41° below nothing; so that in this experiment the ball of the thermometer was surrounded for more than an hour with quicksilver, which was visibly frozen and slowly melting, and during all which time it continued stationary at 40° or 41° below nothing.

It must be observed, however, that in the first and second experiments, which were both tried with this apparatus, the freezing point came out exactly $-40°$, whereas in this it seemed about half a degree lower; the reason of which, in all probability, is, that the tube of this thermometer was not so well fitted to its scale but that it had a little play, which would make the freezing point appear near half a degree higher or lower, according as the tube was pushed up or down.

Though the foregoing experiments leave no reasonable room to doubt, that this is the true point at which quicksilver freezes, yet Mr. Hutchins has, if possible, made this still more evident by his two last experiments; as, in the first of them, he froze some quicksilver in a gally-pot immersed in a freezing mixture, so that the quicksilver was in contact with, and covered by, the snow and spirit of nitre; and in the latter in the open air, by the natural cold of the weather, and then dipping the ball of the thermometer into the unfrozen part, observed what degree it stood at. These experiments agree with the former in shewing the freezing point to be $-40°$ on the two mercurial thermometers; and also shew what degree on the spirit thermometers answers thereto, namely, $29°\frac{3}{4}$ or $28°\frac{1}{2}$ on *D*, and 30° on *E*; for in these two experiments the spirit thermometers also were dipped into the frozen quicksilver.

In all the experiments, therefore, tried with the thermometer *G*, the freezing point came out $-40°$. In those tried with *F*, it came out either $-40°$, or about $-40°\frac{1}{2}$; so that as it appears, from Mr. Hutchins's table of comparison, that *F* stood at a medium a quarter of a degree lower than *G*, the experiments made with that thermometer also shew the freezing point to be $-40°$ on *G*; and as it appeared from the examination of this thermometer after it came home, that $-40°$ thereon answers to $-38°\frac{2}{3}$, on a thermometer adjusted in the manner recommended by the Committee of the Royal Society, it follows, that all the experiments agree in shewing that the true point at which quicksilver freezes is $38°\frac{2}{3}$, or in whole numbers $39°$ below nothing.

From what has been said it appears, that the point at which quicksilver freezes has been determined by Mr. Hutchins in different ways, all perfectly satisfactory, and all agreeing in the same result. In the three first experiments the thermometer was surrounded by quicksilver, which continued freezing till it became solid. In the sixth experiment the quicksilver with which it was surrounded continued slowly melting till the whole was dissolved; and in both cases the thermometer remained stationary all the while at what we have just said to be the freezing point. In the ninth and tenth experiments, the ball of the thermometer was dipped into quicksilver, previously frozen and beginning to melt, as usually practised in settling the freezing point on thermometers, and agreed in the same result, the quicksilver in the last experiment being frozen by the natural cold of the atmosphere; and in the former, by being immersed in, and in contact with, a freezing mixture; so that this point appears to be determined in as satisfactory a manner as can be desired; and the more so, as it seems impossible that experiments should be made with more care and attention, or more faithfully and circumstantially related than these have been. The second and third experiments also shew, that quicksilver, as well as water, can bear being cooled a little below the freezing point without freezing, and is suddenly heated to that point as soon as it begins to congeal.

On the Contraction of quicksilver in freezing.

All these experiments prove, that quicksilver contracts or diminishes in bulk by freezing; and that the very low degrees to which the thermometers have been made to sink, is owing to this contraction, and not to the cold having been in any degree equal to that shewn by the thermometer. In the fourth experiment the thermometer *A* sunk to $-450°$, though it appeared by the spirit thermometers that the cold of the mixture was not more than $5°$ or $6°$ below the point of freezing quicksilver. In the first experiment also, it sunk to $-448°$, at a time when the cold of the mixture was only $2°\frac{1}{2}$ below that point; so that it appears, that the contraction of quicksilver, by freezing, must be at least equal to its expansion by 404° of heat[1]. This, however, is not the whole contraction which it suffers; for it appears, by an extract which Mr. Hutchins was so good as to give me from a meteorological journal, kept by him at Albany Fort, that his thermometer once sunk to 490° below nothing, though it appeared, by a spirit thermometer, that the cold scarcely exceeded the point of freezing quicksilver. There are two experiments also of Professor Braun, in which the thermometer sunk to 544° and 556° below nothing, which is the greatest descent he ever observed without the ball being cracked. It is not indeed known how cold his mixtures were; but from Mr. Hutchins's, there is great reason to think that they could not be many degrees below $-40°$. If so, the contraction which quicksilver suffers in freezing is sometimes not much less than its expansion by 500° or 510° of heat, that is almost $\frac{1}{23}$d of its whole bulk, and in all probability is never much more than that.

It is very likely, however, that the contraction which quicksilver suffers in freezing is no very determinate quantity; for a considerable difference may frequently be observed in the specific gravity of the same piece of metal, cast different times over, and almost all cast metals become heavier by hammering; and it is likely that the same thing may obtain in quicksilver, which is only a metal which melts with a much less degree of heat than the rest. I do not know, indeed, how much this variation can amount to; but, on casting the same piece of tin three times over, I found its density to vary from 7·252 to 7·294, though I have great reason to think that no hollows were left in it, and that only a small part of this difference could proceed from the error of the experiment. This variation of density is as much as is produced in quicksilver by an alteration of 66° of heat; and it is not unlikely, that the descent of a thermometer, on account of the contraction of the quicksilver in its ball by freezing, may vary as much in different trials, though the whole mass of quicksilver is frozen and without any vacuities.

The thermometer marked *C* was intended for trying how much the contraction of quicksilver is; but the experiments made with it were not attended with success, as in the first experiment it did not sink so low as *A* had done, owing, most likely, to the great cold of the weather which froze the quicksilver in the tube; and in the second experiment the ball broke.

On the cold of the freezing mixtures.

The cold produced by mixing spirit of nitre with snow is owing, as was before said, to the melting of the snow. Now, in all probability, there is a

[1] The numbers here given are those shewn by the thermometer without any correction; but if a proper allowance is made for the error of that instrument it will appear, that the true contraction was 25° less than here set down, and from the manner in which thermometers have been usually adjusted, it is likely, that in the following experiment of Mr Hutchins, as well as those of Professor Braun, the true contraction might equally fall short of that shewn by observation.

certain degree of cold in which the spirit of nitre, so far from dissolving snow, will yield out part of its own water, and suffer that to freeze, as is the case with solutions of common salt; so that if the cold of the materials before mixing is equal to this, no additional cold can be produced. If the cold of the materials is less, some increase of cold will be produced; but the total cold will be less than in the former case, since the additional cold cannot be generated without some of the snow being dissolved, and thereby weakening the acid, and making it less able to dissolve more snow; but yet the less the cold of the materials is, the greater will be the additional cold produced. This is conformable to Mr. Hutchins's experiments; for in the fifth experiment, in which the cold of the materials was $-40°$, the additional cold produced was only $5°$. In the first experiment, in which the cold of the materials was only $-23°$, an addition of at least $19°$ of cold was obtained; and by mixing some of the same spirit of nitre with snow in this climate, when the heat of the materials was $+26°$, I have sunk the thermometer to $-29°$; so that an addition of $55°$ of cold was produced.

It is remarkable, that in none of Mr. Hutchins's experiments the cold of the mixture was more than $6°$ of the spirit thermometer below the point of freezing quicksilver, which is so little that it might incline one to think, that the spirit of nitre used by him was weak. This, however, was not the case, as its specific gravity at $58°$ of heat was $1{\cdot}4923$. It was able to dissolve $\frac{1}{1{\cdot}42}$ its weight of marble, and contained very little mixture of the vitriolic or marine acid: as well as I could judge from what experience I have of spirit of nitre, it was as little phlogisticated as acid of that strength usually is.

But, however extraordinary it may at first appear, there is the utmost reason to think, that a rather greater degree of cold would have been obtained if the spirit of nitre had been weaker; for I found, by adding snow gradually to some of this acid, that the addition of a small quantity produced heat instead of cold; and it was not until so much was added as to increase the heat from $28°$ to $51°$, that the addition of more snow began to produce cold; the quantity of snow required for this purpose being pretty exactly one-quarter of the weight of the spirit of nitre, and the heat of the snow and air of the room, as well as of the acid, being $28°$. The reason of this is, that a great deal of heat is produced by mixing water with spirit of nitre, and the stronger the spirit is, the greater is the heat produced. Now it appears from this experiment, that before the acid was diluted, the heat produced by its union with the water formed from the melted snow was greater than the cold produced by the melting of the snow; and it was not till it was diluted by the addition of one-quarter of its weight of that substance, that the cold generated by the latter cause began to exceed the heat generated by the former. From what has been said, it is evident, that the cold of a freezing mixture, made with the undiluted acid, cannot be quite so great as that of one made with the same acid, diluted with a quarter of its weight of water, supposing the acid and snow to be both at 28° of heat, and there is no reason to think, that the event will be different if they are colder; for the undiluted acid will not begin to generate cold until so much snow is dissolved as to increase its heat from 28° to 51°, so that no greater cold will be produced than would be obtained by mixing the diluted acid heated to 51° with snow of the heat of 28°. This method of adding snow gradually to an acid is much the best way I know of finding what strength it ought to be of, in order to produce the greatest effect possible.

By means of this acid, diluted in the above-mentioned proportion, I froze the quicksilver in the thermometer called *G* by Mr. Hutchins, on the 26th of last February. I did not, indeed, break the thermometer to examine the state of the quicksilver therein; for as it sunk to – 110° it must certainly have been in part frozen; but immediately took it out, and put the spirit thermometer in its room, in order to find the cold of the mixture. It sunk only to – 30°; but, by making allowance for the spirit in the tube being not so cold as that in the ball, it appears, that if it had not been for this cause it would have sunk to – 35°[1], which is 5° below the point of freezing, and is as great a degree of cold, within 1°, as was produced in any of Mr. Hutchins's experiments.

In this experiment the thermometer *G* sunk very rapidly, and, as far as I could perceive, without stopping at any intermediate point, till it came to the above-mentioned degree of – 110°, where it stuck. The materials used in making the mixture were previously cooled, by means of salt and snow, to near nothing; the temper of the air was between 20° and 25°; the quantity of acid used was $4\frac{1}{4}$ oz.; and the glass in which the mixture was made was surrounded with wool, and placed in a wooden box, to prevent its losing its cold so fast as it would otherwise have done.

Some weeks before this, I made a freezing mixture with some spirit of nitre, much stronger than that used in the foregoing experiment, though not quite so strong as the undiluted acid, in which the cold was less intense by $4°\frac{1}{2}$, as the thermometer *G* sunk to – $40°\frac{1}{2}$. It is true, that the temper of the air was much less cold, namely, 35°; but the spirit of nitre was at least as cold, and the snow not much less so. The experiment was tried in the same vessel and with the same precautions as the former.

[1] As the surface of the freezing mixture answered to – 185° on the tube, there were 155° of spirit in the tube which could hardly be cooled much below the temper of the air, and which must, therefore, be warmer than that in the ball by about 55° of this thermometer, as the heat of the spirit in the ball was before said to be – 35°, and the temper of the air above + 20°. Therefore, the correction must be equal to the expansion of a column of spirits 155° long, by an alteration of heat equal to 55° on this thermometer, which, if 1° on the scale answers to $\frac{1}{1700}$th of the bulk of the spirit, is equal to $\frac{55 \times 155}{1700}$ or 5°.

The cold produced by mixing oil of vitriol, properly diluted with snow, is not so great as that procured by spirit of nitre, though it seems not to differ from it by so much as 8°; for a freezing mixture, prepared with diluted oil of vitriol, whose specific gravity, at 60° of heat, was 1·5642, sunk the thermometer *G* to – 37°, the experiment being tried at the same time, and with the same precautions, as the foregoing. It was previously found, by adding snow gradually to some of this acid, as was done by the spirit of nitre, that it was a little, but not much stronger than it ought to be, in order to produce the greatest effect.

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XIII. *Experiments on Air. By* Henry Cavendish, *Esq., F.R.S. & S.A.*

Read January 15, 1784

THE following experiments were made principally with a view to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed; and as they seem not only to determine this point, but also to throw great light on the constitution and manner of production of dephlogisticated air, I hope they may be not unworthy the acceptance of this society.

Many gentlemen have supposed that fixed air is either generated or separated from atmospheric air by phlogistication, and that the observed diminution is owing to this cause; my first experiments therefore were made in order to ascertain whether any fixed air is really produced thereby. Now, it must be observed, that as all animal and vegetable substances contain fixed air, and yield it by burning, distillation, or putrefaction, nothing can be concluded from experiments in which the air is phlogisticated by them. The only methods I know, which are not liable to objection, are by the calcination of metals, the burning of sulphur or phosphorus, the mixture of nitrous air, and the explosion of inflammable air. Perhaps it may be supposed, that I ought to add to these the electric spark; but I think it much most likely, that the phlogistication of the air, and production of fixed air, in this process, is owing to the burning of some inflammable matter in the apparatus. When the spark is taken from a solution of tournsol, the burning of the tournsol may produce this effect; when it is taken from lime-water, the burning of some foulness adhering to the tube, or perhaps of some inflammable matter contained in the lime, may have the same effect; and when quicksilver or metallic knobs are used, the calcination of them may contribute to the phlogistication of the air, though not to the production of fixed air.

There is no reason to think that any fixed air is produced by the first method of phlogistication. Dr. Priestley never found lime-water to become turbid by the calcination of metals over it[1]: Mr. Lavoisier also found only

[1] *Experiments on Air*, vol. I. p. 137.

a very slight and scarce perceptible turbid appearance, without any precipitation, to take place when lime-water was shaken in a glass vessel full of the air in which lead had been calcined; and even this small diminution of transparency in the lime-water might very likely arise, not from fixed air, but only from its being fouled by particles of the calcined metal, which we are told adhered in some places to the glass. This want of turbidity has been attributed to the fixed air uniting to the metallic calx, in preference to the lime; but there is no reason for supposing that the calx contained any fixed air; for I do not know that any one has extracted it from calces prepared in this manner; and though most metallic calces prepared over the fire, or by long exposure to the atmosphere, where they are in contact with fixed air, contain that substance, it by no means follows that they must do so when prepared by methods in which they are not in contact with it.

Dr. Priestley also observed, that quicksilver, fouled by the addition of lead or tin, deposits a powder by agitation and exposure to the air, which consists in great measure of the calx of the imperfect metal. He found too some powder of this kind to contain fixed air[1]; but it is by no means clear that this air was produced by the phlogistication of the air in which the quicksilver was shaken; as the powder was not prepared on purpose, but was procured from quicksilver fouled by having been used in various experiments, and may therefore have contained other impurities besides the metallic calces.

I never heard of any fixed air being produced by the burning of sulphur or phosphorus; but it has been asserted, and commonly believed, that lime water is rendered cloudy by a mixture of common and nitrous air; which, if true, would be a convincing proof that on mixing those two substances some fixed air is either generated or separated; I therefore examined this carefully. Now it must be observed, that as common air usually contains a little fixed air, which is no essential part of it, but is easily separated by lime water; and as nitrous air may also contain fixed air, either if the metal from which it is procured be rusty, or if the water of the vessel in which it is caught contain calcareous earth, suspended by fixed air, as most waters do, it is proper first to free both airs from it by previously washing them with lime water[2]. Now I found, by repeated experiments, that if the lime water was clean, and the two airs were previously washed with that substance, not the least cloud was produced, either immediately

[1] *Exper. in Nat. Phil.* vol. I. p. 144.

[2] Though fixed air is absorbed in considerable quantity by water, as I shewed in *Phil. Trans.* vol. LVI., yet it is not easy to deprive common air of all the fixed air contained in it by means of water. On shaking a mixture of ten parts of common air, and one of fixed air, with more than an equal bulk of distilled water, not more than half of the fixed air was absorbed, and on transferring the air into fresh distilled water only half the remainder was absorbed, as appeared by the diminution which it still suffered on adding lime water.

on mixing them, or on suffering them to stand upwards of an hour, though it appeared by the thick clouds which were produced in the lime water, by breathing through it after the experiment was finished, that it was more than sufficient to saturate the acid formed by the decomposition of the nitrous air, and consequently that if any fixed air had been produced, it must have become visible. Once indeed I found a small cloud to be formed on the surface, after the mixture had stood a few minutes. In this experiment the lime water was not quite clean; but whether the cloud was owing to this circumstance, or to the air's having not been properly washed, I cannot pretend to say.

Neither does any fixed air seem to be produced by the explosion of the inflammable air obtained from metals, with either common or dephlogisticated air. This I tried by putting a little lime-water into a glass globe fitted with a brass cock, so as to make it air tight, and an apparatus for firing air by electricity. This globe was exhausted by an air-pump, and the two airs, which had been previously washed with lime-water, let in, and suffered to remain some time, to shew whether they would affect the lime-water, and then fired by electricity. The event was, that not the least cloud was produced in the lime-water, when the inflammable air was mixed with common air, and only a very slight one, or rather diminution of transparency, when it was combined with dephlogisticated air. This, however, seemed not to be produced by fixed air; as it appeared instantly after the explosion, and did not increase on standing, and was spread uniformly through the liquor; whereas if it had been owing to fixed air, it would have taken up some short time before it appeared, and would have begun first at the surface, as was the case in the abovementioned experiment with nitrous air. What it was really owing to I cannot pretend to say; but if it did proceed from fixed air it would shew that only an excessively minute quantity was produced[1]. On the whole, though it is not improbable that fixed air may be generated in some chymical processes, yet it seems certain that it is not the general effect of phlogisticating air, and that the diminution of common air is by no means owing to the generation or separation of fixed air from it.

As there seemed great reason to think, from Dr. Priestley's experiments, that the nitrous and vitriolic acids were convertible into dephlogisticated air, I tried whether the dephlogisticated part of common air might not, by phlogistication, be changed into nitrous or vitriolic acid. For this purpose I impregnated some milk of lime with the fumes of burning sulphur, by putting a little of it into a large glass receiver, and burning sulphur therein, taking care to keep the mouth of the receiver stopt till the fumes were all absorbed; after which the air of the receiver was changed, and more sulphur burnt in it as before, and the process repeated

[1] Dr. Priestley also found no fixed air to be produced by the explosion of inflammable and common air. Vol. v. p. 124.

till 122 grains of sulphur were consumed. The milk of lime was then filtered and evaporated, but it yielded no nitrous salt, nor any other substance except selenite; so that no sensible quantity of the air was changed into nitrous acid. It must be observed, that as the vitriolic acid produced by the burning sulphur is changed by its union with the lime into selenite, which is very little soluble in water, a very small quantity of nitrous salt, or any other substance which is soluble in water, would have been perceived.

I also tried whether any nitrous acid was produced by phlogisticating common air with liver of sulphur; for this purpose I made a solution of flowers of sulphur by boiling it with lime, and put a little of it into a large receiver, and shook it frequently, changing now and then the air, till the yellow colour of the solution was quite gone; a sign that all the sulphur was, by the loss of its phlogiston, turned into vitriolic acid, and united to the lime, or precipitated; the liquor was then filtered and evaporated, but it yielded not the least nitrous salt.

The experiment was repeated in nearly the same manner with dephlogisticated air procured from red precipitate; but not the least nitrous acid was obtained.

It is well known that common selenite is very little soluble in water; whereas that procured in the two last experiments was very soluble, and even crystallized readily, and was intensely bitter; this however appeared to be owing merely to the acid with which it was formed being very much phlogisticated; for on evaporating it to dryness, and exposing it to the air for a few days, it became much less soluble, so that on adding water to it not much dissolved; and by repeating this process once or twice, it seemed to become not more soluble than selenite made in the common manner.

This solubility of the selenite caused some trouble in trying the experiment; for while it continued much soluble it would have been impossible to have distinguished a small mixture of nitrous salt; but by the above-mentioned process I was able to distinguish as small a proportion as if the selenite had been originally no more soluble than usual.

The nature of the neutral salts made with the phlogisticated vitriolic and nitrous acids has not been much examined by the chymists, though it seems well worth their attention; and it is likely that many besides the foregoing may differ remarkably from those made with the same acids in their common state. Nitre formed with the phlogisticated nitrous acid has been found to differ considerably from common nitre, as well as Sal Polychrest from vitriolated tartar.

In order to try whether any vitriolic acid was produced by the phlogistication of air, I impregnated fifty ounces of distilled water with the fumes produced on mixing fifty-two ounce measures of nitrous air with a quantity of common air sufficient to decompound it. This was done by filling a bottle with some of this water, and inverting it into a bason of the same, and then, by a syphon, letting in as much nitrous air as filled it half-full; after which common air was added slowly by the same syphon, till all the nitrous air was decompounded. When this was done, the distilled water was further impregnated in the same manner till the whole of the above-mentioned quantity of nitrous air was employed. This impregnated water, which was very sensibly acid to the taste, was distilled in a glass retort. The first runnings were very acid, and smelt pungent, being nitrous acid much phlogisticated; what came next had no sensible taste or smell; but the last runnings were very acid, and consisted of nitrous acid not phlogisticated. Scarce any sediment was left behind. These different parcels of distilled liquor were then exactly saturated with salt of tartar, and evaporated; they yielded 87½ grains of nitre, which, as far as I could perceive, was unmixed with vitriolated tartar or any other substance, and consequently no sensible quantity of the common air with which the nitrous air was mixed was turned into vitriolic acid.

It appears, from this experiment, that nitrous air contains as much acid as 2¾ times its weight of saltpetre; for fifty-two ounce measures of nitrous air weigh 32 grains, and, as was before said, yield as much acid as is contained in 87½ grains of saltpetre; so that the acid in nitrous air is in a remarkably concentrated state, and I believe more than 1½ times as much so as the strongest spirit of nitre ever prepared.

Having now mentioned the unsuccessful attempts I made to find out what becomes of the air lost by phlogistication, I proceed to some experiments, which serve really to explain the matter.

In Dr. Priestley's last volume of experiments is related an experiment of Mr. Warltire's, in which it is said that, on firing a mixture of common and inflammable air by electricity in a close copper vessel holding about three pints, a loss of weight was always perceived, on an average about two grains, though the vessel was stopped in such a manner that no air could escape by the explosion. It is also related, that on repeating the experiment in glass vessels, the inside of the glass, though clean and dry before, immediately became dewy; which confirmed an opinion he had long entertained, that common air deposits its moisture by phlogistication. As the latter experiment seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely. The first experiment also, if there was no mistake in it, would be very extraordinary and curious; but it did not succeed with me; for though the vessel I used held more than Mr. Warltire's, namely, 24,000 grains of water, and though the experiment was repeated several times with different proportions of common and inflammable air, I could never perceive a loss of weight of more than one-fifth of a grain, and commonly none at all. It must be observed, however, that though there were some of the experiments in which it seemed to diminish a little in weight, there were none in which it increased[1].

[1] Dr. Priestley, I am informed, has since found the experiment not to succeed.

In all the experiments, the inside of the glass globe became dewy, as observed by Mr. Warltire; but not the least sooty matter could be perceived. Care was taken in all of them to find how much the air was diminished by the explosion, and to observe its test. The result is as follows: the bulk of the inflammable air being expressed in decimals of the common air,

Common air	Inflammable air	Diminution	Air remaining after the explosion	Test of this air in first method	Standard
	1,241	,686	1,555	,055	,0
1	1,055	,642	1,413	,063	,0
	,706	,647	1,059	,066	,0
	,423	,612	,811	,097	,03
	,331	,476	,855	,339	,27
	,206	,294	,912	,648	,58

In these experiments the inflammable air was procured from zinc, as it was in all my experiments, except where otherwise expressed: but I made two more experiments, to try whether there was any difference between the air from zinc and that from iron, the quantity of inflammable air being the same in both, namely, 0,331 of the common; but I could not find any difference to be depended on between the two kinds of air, either in the diminution which they suffered by the explosion, or the test of the burnt air.

From the fourth experiment it appears, that 423 measures of inflammable air are nearly sufficient to completely phlogisticate 1000 of common air; and that the bulk of the air remaining after the explosion is then very little more than four-fifths of the common air employed; so that as common air cannot be reduced to a much less bulk than that by any method of phlogistication, we may safely conclude, that when they are mixed in this proportion, and exploded, almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity, and are condensed into the dew which lines the glass.

The better to examine the nature of this dew, 500,000 grain measures of inflammable air were burnt with about $2\frac{1}{2}$ times that quantity of common air, and the burnt air made to pass through a glass cylinder eight feet long and three-quarters of an inch in diameter, in order to deposit the dew. The two airs were conveyed slowly into this cylinder by separate copper pipes, passing through a brass plate which stopped up the end of the cylinder; and as neither inflammable nor common air can burn by themselves, there was no danger of the flame spreading into the magazines from which they were conveyed. Each of these magazines consisted of a large tin vessel, inverted into another vessel just big enough to receive it. The inner vessel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer vessel; and in order that the quantity of common air expelled should be $2\frac{1}{2}$ times that of the inflammable, the water was let into the outer vessels by two holes in the bottom of the same tin pan, the hole which conveyed the water into that vessel in which the common air was confined being $2\frac{1}{2}$ times as big as the other.

In trying the experiment, the magazines being first filled with their respective airs, the glass cylinder was taken off, and water let, by the two holes, into the outer vessels, till the airs began to issue from the ends of the copper pipes; they were then set on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grains of water were condensed in the cylinder, which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it yield any pungent smell during the evaporation; in short, it seemed pure water.

In my first experiment, the cylinder near that part where the air was fired was a little tinged with sooty matter, but very slightly so; and that little seemed to proceed from the putty with which the apparatus was luted, and which was heated by the flame; for in another experiment, in which it was contrived so that the luting should not be much heated, scarce any sooty tinge could be perceived.

By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almost all the inflammable air, and near one-fifth of the common air, lose their elasticity, and are condensed into dew. And by this experiment it appears, that this dew is plain water, and consequently that almost all the inflammable air, and about one-fifth of the common air, are turned into pure water.

In order to examine the nature of the matter condensed on firing a mixture of dephlogisticated and inflammable air, I took a glass globe, holding 8800 grain measures, furnished with a brass cock and an apparatus for firing air by electricity. This globe was well exhausted by an air-pump, and then filled with a mixture of inflammable and dephlogisticated air, by shutting the cock, fastening a bent glass tube to its mouth, and letting up the end of it into a glass jar inverted into water, and containing a mixture of 19,500 grain measures of dephlogisticated air, and 37,000 of inflammable; so that, upon opening the cock, some of this mixed air rushed through the bent tube, and filled the globe[1]. The cock was then shut, and the included air fired by electricity, by which means almost all of it lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the sixth part of the mixture, almost the

[1] In order to prevent any water from getting into this tube, while dipped under water to let it up into the glass jar, a bit of wax was stuck upon the end of it, which was rubbed off when raised above the surface of the water.

whole of it was exploded therein, without any fresh exhaustion of the globe.

As I was desirous to try the quantity and test of this burnt air, without letting any water into the globe, which would have prevented my examining the nature of the condensed matter, I took a larger globe, furnished also with a stop cock, exhausted it by an air-pump, and screwed it on upon the cock of the former globe; upon which, by opening both cocks, the air rushed out of the smaller globe into the larger, till it became of equal density in both; then, by shutting the cock of the larger globe, unscrewing it again from the former, and opening it under water, I was enabled to find the quantity of the burnt air in it; and consequently, as the proportion which the contents of the two globes bore to each other was known, could tell the quantity of burnt air in the small globe before the communication was made between them. By this means the whole quantity of the burnt air was found to be 2950 grain measures; its standard was 1,85.

The liquor condensed in the globe, in weight about 30 grains, was sensibly acid to the taste, and by saturation with fixed alkali, and evaporation, yielded near two grains of nitre; so that it consisted of water united to a small quantity of nitrous acid. No sooty matter was deposited in the globe. The dephlogisticated air used in this experiment was procured from red precipitate, that is, from a solution of quicksilver in spirit of nitre distilled till it acquires a red colour.

As it was suspected, that the acid contained in the condensed liquor was no essential part of the dephlogisticated air, but was owing to some acid vapour which came over in making it and had not been absorbed by the water, the experiment was repeated in the same manner, with some more of the same air, which had been previously washed with water, by keeping it a day or two in a bottle with some water, and shaking it frequently; whereas that used in the preceding experiment had never passed through water, except in preparing it. The condensed liquor was still acid.

The experiment was also repeated with dephlogisticated air, procured from red lead by means of oil of vitriol; the liquor condensed was acid, but by an accident I was prevented from determining the nature of the acid.

I also procured some dephlogisticated air from the leaves of plants, in the manner of Doctors Ingenhousz and Priestley, and exploded it with inflammable air as before; the condensed liquor still continued acid, and of the nitrous kind.

In all these experiments the proportion of inflammable air was such, that the burnt air was not much phlogisticated; and it was observed, that the less phlogisticated it was, the more acid was the condensed liquor. I therefore made another experiment, with some more of the same air from plants, in which the proportion of inflammable air was greater, so that the burnt air was almost completely phlogisticated, its standard being $\frac{1}{10}$. The condensed liquor was then not at all acid, but seemed pure water: so that it appears, that with this kind of dephlogisticated air, the condensed liquor is not at all acid, when the two airs are mixed in such a proportion that the burnt air is almost completely phlogisticated, but is considerably so when it is not much phlogisticated.

In order to see whether the same thing would obtain with air procured from red precipitate, I made two more experiments with that kind of air, the air in both being taken from the same bottle, and the experiment tried in the same manner, except that the proportions of inflammable air were different. In the first, in which the burnt air was almost completely phlogisticated, the condensed liquor was not at all acid. In the second, in which its standard was 1,86, that is, not much phlogisticated, it was considerably acid; so that with this air, as well as with that from plants, the condensed liquor contains, or is entirely free from, acid, according as the burnt air is less or more phlogisticated; and there can be little doubt but that the same rule obtains with any other kind of dephlogisticated air.

In order to see whether the acid, formed by the explosion of dephlogisticated air obtained by means of the vitriolic acid, would also be of the nitrous kind, I procured some air from turbith mineral, and exploded it with inflammable air, the proportion being such that the burnt air was not much phlogisticated. The condensed liquor manifested an acidity, which appeared, by saturation with a solution of salt of tartar, to be of the nitrous kind; and it was found, by the addition of some terra ponderosa salita, to contain little or no vitriolic acid.

When inflammable air was exploded with common air, in such a proportion that the standard of the burnt air was about $\frac{4}{10}$, the condensed liquor was not in the least acid. There is no difference, however, in this respect between common air, and dephlogisticated air mixed with phlogisticated in such a proportion as to reduce it to the standard of common air; for some dephlogisticated air from red precipitate, being reduced to this standard by the addition of perfectly phlogisticated air, and then exploded with the same proportion of inflammable air as the common air was in the foregoing experiment, the condensed liquor was not in the least acid.

From the foregoing experiments it appears, that when a mixture of inflammable and dephlogisticated air is exploded in such proportion that the burnt air is not much phlogisticated, the condensed liquor contains a little acid, which is always of the nitrous kind, whatever substance the dephlogisticated air is procured from; but if the proportion be such that the burnt air is almost entirely phlogisticated, the condensed liquor is not at all acid, but seems pure water, without any addition whatever; and as, when they are mixed in that proportion, very little air remains after the explosion, almost the whole being condensed, it follows, that almost the whole of the inflammable and dephlogisticated air is converted into pure water. It is not easy, indeed, to determine from these experiments what proportion the burnt air, remaining after the explosions, bore to the dephlogisticated air employed, as neither the small nor the large globe could be perfectly exhausted of air, and there was no saying with exactness what quantity was left in them; but in most of them, after allowing for this uncertainty, the true quantity of burnt air seemed not more than $\frac{1}{17}$th of the dephlogisticated air employed, or $\frac{1}{50}$th of the mixture. It seems, however, unnecessary to determine this point exactly, as the quantity is so small, that there can be little doubt but that it proceeds only from the impurities mixed with the dephlogisticated and inflammable air, and consequently that, if those airs could be obtained perfectly pure, the whole would be condensed.

With respect to common air, and dephlogisticated air reduced by the addition of phlogisticated air to the standard of common air, the case is different; as the liquor condensed in exploding them with inflammable air, I believe I may say in any proportion, is not at all acid; perhaps, because if they are mixed in such a proportion as that the burnt air is not much phlogisticated, the explosion is too weak, and not accompanied with sufficient heat.

All the foregoing experiments, on the explosion of inflammable air with common and dephlogisticated airs, except those which relate to the cause of the acid found in the water, were made in the summer of the year 1781, and were mentioned by me to Dr. Priestley, who in consequence of it made some experiments of the same kind, as he relates in a paper printed in the preceding volume of the *Transactions*. During the last summer also, a friend of mine gave some account of them to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time so far was M. Lavoisier from thinking any such opinion warranted, that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable, that neither of these gentlemen found any acid in the water produced by the combustion; which might proceed from the latter having burnt the two airs in a different manner from what I did; and from the former having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it.

Before I enter into the cause of these phænomena, it will be proper to take notice, that phlogisticated air appears to be nothing else than the nitrous acid united to phlogiston; for when nitre is deflagrated with charcoal, the acid is almost entirely converted into this kind of air. That the acid is entirely converted into air, appears from the common process for making what is called clyssus of nitre; for if the nitre and charcoal are dry, scarce any thing is found in the vessels prepared for condensing the fumes; but if they are moist a little liquor is collected, which is nothing but the water contained in the materials, impregnated with a little volatile alkali, proceeding in all probability from the imperfectly burnt charcoal, and a little fixed alkali, consisting of some of the alkalized nitre carried over by the heat and watery vapours. As far as I can perceive too, at present, the air into which much the greatest part of the acid is converted, differs in no respect from common air phlogisticated. A small part of the acid, however, is turned into nitrous air, and the whole is mixed with a good deal of fixed, and perhaps a little inflammable air, both proceeding from the charcoal.

It is well known, that the nitrous acid is also converted by phlogistication into nitrous air, in which respect there seems a considerable analogy between that and the vitriolic acid; for the vitriolic acid, when united to a smaller proportion of phlogiston, forms the volatile sulphureous acid and vitriolic acid air, both of which, by exposure to the atmosphere, lose their phlogiston, though not very fast, and are turned back into vitriolic acid; but, when united to a greater proportion of phlogiston, it forms sulphur, which shews no signs of acidity, unless a small degree of affinity to alkalies can be called so, and in which the phlogiston is more strongly adherent, so that it does not fly off when exposed to the air, unless assisted by a heat sufficient to set it on fire. In like manner the nitrous acid, united to a certain quantity of phlogiston, forms nitrous fumes and nitrous air, which readily quit their phlogiston to common air; but when united to a different, in all probability a larger quantity, it forms phlogisticated air, which shews no signs of acidity, and is still less disposed to part with its phlogiston than sulphur.

This being premised, there seem two ways by which the phænomena of the acid found in the condensed liquor may be explained; first, by supposing that dephlogisticated air contains a little nitrous acid which enters into it as one of its component parts, and that this acid, when the inflammable air is in a sufficient proportion, unites to the phlogiston, and is turned into phlogisticated air, but does not when the inflammable air is in too small a proportion; and, secondly, by supposing that there is no nitrous acid mixed with, or entering into the composition of, dephlogisticated air, but that, when this air is in a sufficient proportion, part of the phlogisticated air with which it is debased is, by the strong affinity of phlogiston to dephlogisticated air, deprived of its phlogiston and turned into nitrous acid; whereas, when the dephlogisticated air is not more than sufficient to consume the inflammable air, none then remains to deprive the phlogisticated air of its phlogiston, and turn it into acid.

If the latter explanation be true, I think, we must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston[1]; since, according to this supposition, these two substances united together form pure water. On the other hand, if the first explanation be true, we must suppose that dephlogisticated air consists of water united to a little nitrous acid and deprived of its phlogiston; but still the nitrous acid in it must make only a very small part of the whole, as it is found, that the phlogisticated air, which it is converted into, is very small in comparison of the dephlogisticated air.

I think the second of these explanations seems much the most likely; as it was found, that the acid in the condensed liquor was of the nitrous kind, not only when the dephlogisticated air was prepared from red precipitate, but also when it was procured from plants or from turbith mineral: and it seems not likely, that air procured from plants, and still less likely that air procured from a solution of mercury in oil of vitriol, should contain any nitrous acid.

Another strong argument in favour of this opinion is, that dephlogisticated air yields no nitrous acid when phlogisticated by liver of sulphur; for if this air contains nitrous acid, and yields it when phlogisticated by explosion with inflammable air, it is very extraordinary that it should not do so when phlogisticated by other means.

But what forms a stronger and, I think, almost decisive argument in favour of this explanation is, that when the dephlogisticated air is very pure, the condensed liquor is made much more strongly acid by mixing the air to be exploded with a little phlogisticated air, as appears by the following experiments.

A mixture of 18,500 grain measures of inflammable air with 9750 of dephlogisticated air procured from red precipitate were exploded in the usual manner; after which, a mixture of the same quantities of the same dephlogisticated and inflammable air, with the addition of 2500 of air

[1] Either of these suppositions will agree equally well with the following experiments; but the latter seems to me much the most likely. What principally makes me think so is, that common or dephlogisticated air do not absorb phlogiston from inflammable air, unless assisted by a red heat, whereas they absorb the phlogiston of nitrous air, liver of sulphur, and many other substances, without that assistance; and it seems inexplicable, that they should refuse to unite to pure phlogiston, when they are able to extract it from substances to which it has an affinity; that is, that they should overcome the affinity of phlogiston to other substances, and extract it from them, when they will not even unite to it when presented to them. On the other hand, I know no experiment which shews inflammable air to be pure phlogiston rather than an union of it with water, unless it be Dr. Priestley's experiment of expelling inflammable air from iron by heat alone. I am not sufficiently acquainted with the circumstances of that experiment to argue with certainty about it; but I think it much more likely, that the inflammable air was formed by the union of the phlogiston of the iron filings with the water dispersed among them, or contained in the retort or other vessel in which it was heated; and in all probability this was the cause of the separation of the phlogiston, as iron seems not disposed to part with its phlogiston by heat alone, without being assisted by the air or some other substance.

phlogisticated by iron filings and sulphur, was treated in the same manner. The condensed liquor, in both experiments, was acid, but that in the latter evidently more so, as appeared also by saturating each of them separately with marble powder, and precipitating the earth by fixed alkali, the precipitate of the second experiment weighing one-fifth of a grain, and that of the first being several times less. The standard of the burnt air in the first experiment was 1,86, and in the second only 0,9.

It must be observed, that all circumstances were the same in these two experiments, except that in the latter the air to be exploded was mixed with some phlogisticated air, and that in consequence the burnt air was more phlogisticated than in the former; and from what has been before said, it appears, that this latter circumstance ought rather to have made the condensed liquor less acid; and yet it was found to be much more so, which shews strongly that it was the phlogisticated air which furnished the acid.

As a further confirmation of this point, these two comparative experiments were repeated with a little variation, namely, in the first experiment there was first let into the globe 1500 of dephlogisticated air, and then the mixture, consisting of 12,200 of dephlogisticated air and 25,900 of inflammable, was let in at different times as usual. In the second experiment, besides the 1500 of dephlogisticated air first let in, there was also admitted 2500 of phlogisticated air, after which the mixture, consisting of the same quantities of dephlogisticated and inflammable air as before, was let in as usual. The condensed liquor of the second experiment was about three times as acid as that of the first, as it required 119 grains of a diluted solution of salt of tartar to saturate it, and the other only 37. The standard of the burnt air was 0,78 in the second experiment, and 1,96 in the first.

The intention of previously letting in some dephlogisticated air in the two last experiments was, that the condensed liquor was expected to become more acid thereby, as proved actually to be the case.

In the first of these two experiments, in order that the air to be exploded should be as free as possible from common air, the globe was first filled with a mixture of dephlogisticated and inflammable air, it was then exhausted, and the air to be exploded let in; by which means, though the globe was not perfectly exhausted, very little common air could be left in it. In the first set of experiments this circumstance was not attended to, and the purity of the dephlogisticated air was forgot to be examined in both sets.

From what has been said there seems the utmost reason to think, that dephlogisticated air is only water deprived of its phlogiston, and that inflammable air, as was before said, is either phlogisticated water, or else pure phlogiston; but in all probability the former.

As Mr. Watt, in a paper lately read before this Society, supposes water to consist of dephlogisticated air and phlogiston deprived of part of their latent heat, whereas I take no notice of the latter circumstance, it may be proper to mention in a few words the reason of this apparent difference between us. If there be any such thing as elementary heat, it must be allowed that what Mr. Watt says is true; but by the same rule we ought to say, that the diluted mineral acids consist of the concentrated acids united to water and deprived of part of their latent heat; that solutions of sal ammoniac, and most other neutral salts, consist of the salt united to water and elementary heat; and a similar language ought to be used in speaking of almost all chemical combinations, as there are very few which are not attended with some increase or diminution of heat. Now I have chosen to avoid this form of speaking, both because I think it more likely that there is no such thing as elementary heat, and because saying so in this instance, without using similar expressions in speaking of other chemical unions, would be improper, and would lead to false ideas; and it may even admit of doubt, whether the doing it in general would not cause more trouble and perplexity than it is worth.

There is the utmost reason to think, that dephlogisticated and phlogisticated air, as M. Lavoisier and Scheele suppose, are quite distinct substances, and not differing only in their degree of phlogistication; and that common air is a mixture of the two; for if the dephlogisticated air is pretty pure, almost the whole of it loses its elasticity by phlogistication, and, as appears by the foregoing experiments, is turned into water, instead of being converted into phlogisticated air. In most of the foregoing experiments, at least $\frac{16}{17}$ths of the whole was turned into water; and by treating some dephlogisticated air with liver of sulphur, I have reduced it to less than $\frac{1}{30}$th of its original bulk, and other persons, I believe, have reduced it to a still less bulk; so that there seems the utmost reason to suppose, that the small residuum which remains after its phlogistication proceeds only from the impurities mixed with it.

It was just said, that some dephlogisticated air was reduced by liver of sulphur to $\frac{1}{30}$th of its original bulk; the standard of this air was 4,8, and consequently the standard of perfectly pure dephlogisticated air should be very nearly 5, which is a confirmation of the foregoing opinion; for if the standard of pure dephlogisticated air is 5, common air must, according to this opinion, contain one-fifth of it, and therefore ought to lose one-fifth of its bulk by phlogistication, which is what it is actually found to lose.

From what has been said, it follows, that instead of saying air is phlogisticated or dephlogisticated by any means, it would be more strictly just to say, it is deprived of, or receives, an addition of dephlogisticated air; but as the other expression is convenient, and can scarcely be considered as improper, I shall still frequently make use of it in the remainder of this paper.

There seemed great reason to think, from Dr. Priestley's experiments, that both the nitrous and vitriolic acids were convertible into dephlogisticated air, as that air is procured in the greatest quantity from substances containing those acids, especially the former. The foregoing experiments, however, seem to shew that no part of the acid is converted into dephlogisticated air, and that their use in preparing it is owing only to the great power which they possess of depriving bodies of their phlogiston. A strong confirmation of this is, that red precipitate, which is one of the substances yielding dephlogisticated air in the greatest quantity, and which is prepared by means of the nitrous acid, contains in reality no acid. This I found by grinding 400 grains of it with spirits of sal ammoniac, and keeping them together for some days in a bottle, taking care to shake them frequently. The red colour of the precipitate was rendered pale, but not entirely destroyed; being then washed with water and filtered, the clear liquor yielded on evaporation not the least ammoniacal salt.

It is natural to think, that if any nitrous acid had been contained in the red precipitate, it would have united to the volatile alkali and have formed ammoniacal nitre, and would have been perceived on evaporation; but in order to determine more certainly whether this would be the case, I dried some of the same solution of quicksilver from which the red precipitate was prepared with a less heat, so that it acquired only an orange colour, and treated the same quantity of it with volatile alkali in the same manner as before. It immediately caused an effervescence, changed the colour to grey, and yielded 52 grains of ammoniacal nitre. There is the utmost reason to think, therefore, that red precipitate contains no nitrous acid; and consequently that, in procuring dephlogisticated air from it, no acid is converted into air; and it is reasonable to conclude, therefore, that no such change is produced in procuring it from any other substance.

It remains to consider in what manner these acids act in producing dephlogisticated air. The way in which the nitrous acid acts, in the production of it from red precipitate, seems to be as follows. On distilling the mixture of quicksilver and spirit of nitre, the acid comes over, loaded with phlogiston, in the form of nitrous vapour, and continues to do so till the remaining matter acquires its full red colour, by which time all the nitrous acid is driven over, but some of the watery part still remains behind, and adheres strongly to the quicksilver; so that the red precipitate may be considered, either as quicksilver deprived of part of its phlogiston, and united to a certain portion of water, or as quicksilver united to dephlogisticated air[1]; after which, on further increasing the heat, the water in it

[1] Unless we were much better acquainted than we are with the manner in which different substances are united together in compound bodies, it would be ridiculous to say, that it is the quicksilver in the red precipitate which is deprived of its phlogiston, and not the water, or that it is the water and not the quicksilver; all that we can say is, that red precipitate consists of quicksilver and water, one or both of which are deprived of part of their phlogiston. In like manner, during the preparation of the red precipitate, it is certain that the acid absorbs phlogiston, either from the quicksilver or the water; but we are by no means authorised to say from which.

rises deprived of its phlogiston, that is, in the form of dephlogisticated air, and at the same time the quicksilver distils over in its metallic form. It is justly remarked by Dr. Priestley, that the solution of quicksilver does not begin to yield dephlogisticated air till it acquires its red colour.

Mercurius calcinatus appears to be only quicksilver which has absorbed dephlogisticated air from the atmosphere during its preparation; accordingly, by giving it a sufficient heat, the dephlogisticated air is driven off, and the quicksilver acquires its original form. It seems therefore that mercurius calcinatus and red precipitate, though prepared in a different manner, are very nearly the same thing.

From what has been said it follows, that red precipitate and mercurius calcinatus contain as much phlogiston as the quicksilver they are prepared from; but yet, as uniting dephlogisticated air to a metal comes to the same thing as depriving it of part of its phlogiston and adding water to it, the quicksilver may still be considered as deprived of its phlogiston; but the imperfect metals seem not only to absorb dephlogisticated air during their calcination, but also to be really deprived of part of their phlogiston, as they do not acquire their metallic form by driving off the dephlogisticated air.

In procuring dephlogisticated air from nitre, the acid acts in a different manner, as, upon heating the nitre red-hot, the dephlogisticated air rises mixed with a little nitrous acid, and at the same time the acid remaining in the nitre becomes very much phlogisticated; which shews that the acid absorbs phlogiston from the water in the nitre, and becomes phlogisticated, while the water is thereby turned into dephlogisticated air. On distilling 3155 grains of nitre in an unglazed earthen retort, it yielded 256,000 grain measures of dephlogisticated air[1], the standard of different parts of which varied from 3 to 3,65, but at a medium was 3,35. The matter remaining in the retort dissolved readily in water, and tasted alcaline and caustic. On adding diluted spirit of nitre to the solution, strong red fumes were produced; a sign that the acid in it was very much phlogisticated, as no fumes whatever would have been produced on adding the same acid to a solution of common nitre; that part of the solution also which was supersaturated with acid became blue; a colour which the diluted nitrous acid is known to assume when much phlogisticated. The solution, when saturated with this acid, lost its alcaline and caustic taste, but yet tasted very different from true nitre, seeming as if it had been mixed with sea-salt, and also required much less water to dissolve it; but on exposing it for some days to the air, and adding fresh acid as fast as by the flying off

[1] This is, about eighty-one grain measures from one grain of nitre; and the weight of the dephlogisticated air, supposing it 800 times lighter than water is one-tenth of that of the nitre. In all probability it would have yielded a much greater quantity of air, if a greater heat had been applied.

of the fumes the alcali predominated, it became true nitre, unmixed, as far as I could perceive, with any other salt[1].

It has been remarked, that the dephlogisticated air procured from nitre is less pure, than that from red precipitate and many other substances, which may perhaps proceed from unglazed earthen retorts having been commonly used for this purpose, and which, conformably to Dr. Priestley's discovery, may possibly absorb some common air from without, and emit it along with the dephlogisticated air; but if it should be found that the dephlogisticated air procured from nitre in glass or glazed earthen vessels is also impure, it would seem to shew that part of the acid in the nitre is turned into phlogisticated air, by absorbing phlogiston from the watery part.

From what has been said it appears, that there is a considerable difference in the manner in which the acid acts in the production of dephlogisticated air from red precipitate and from nitre; in the former case the acid comes over first, leaving the remaining substance deprived of part of its phlogiston; in the latter the dephlogisticated air comes first, leaving the acid loaded with the phlogiston of the water from which it was formed.

On distilling a mixture of quicksilver and oil of vitriol to dryness, part of the acid comes over, loaded with phlogiston, in the form of volatile sulphureous acid and vitriolic acid air; so that the remaining white mass may be considered as consisting of quicksilver deprived of its phlogiston, and united to a certain proportion of acid and water, or of plain quicksilver united to a certain proportion of acid and dephlogisticated air. Accordingly on urging this white mass with a more violent heat, the dephlogisticated air comes over, and at the same time part of the quicksilver rises in its metallic form, and also part of the white mass, united in all probability to a greater proportion of acid than before, sublimes; so that the rationale of the production of dephlogisticated air from turbith mineral, and from red precipitate, are nearly similar.

True turbith mineral consists of the abovementioned white mass, well washed with water, by which means it acquires a yellow colour, and contains much less acid than the unwashed mass. Accordingly it seems likely, that on exposing this to heat, less of it should sublime without being decompounded, and consequently that more dephlogisticated air should be procured from it than from the unwashed mass.

This is an instance, that the superabundant vitriolic acid may, in some cases, be better extracted from the base it is united to by water than by heat. Vitriolated tartar is another instance; for, if vitriolated tartar be mixed with oil of vitriol and exposed even to a pretty strong red heat, the mass will be very acid; but, if this mass is dissolved in water, and evaporated, the crystals will be not sensibly so.

[1] This phlogistication of the acid in nitre by heat has been observed by Mr. Scheele; see his experiments on air and fire, p. 45, English translation.

In all probability, the vitriolic acid acts in the same manner in the production of dephlogisticated air from alum, as the nitrous does in its production from nitre; that is, the watery part comes over first in the form of dephlogisticated air, leaving the acid charged with its phlogiston. Whether this is also the case with regard to green and blue vitriol, or whether in them the acid does not rather act in the same manner as in turbith mineral, I cannot pretend to say, but I think the latter more likely.

There is another way by which dephlogisticated air has been found to be produced in great quantities, namely, the growth of vegetables exposed to the sun or day-light; the rationale of which, in all probability, is, that plants, when assisted by the light, deprive part of the water sucked up by their roots of its phlogiston, and turn it into dephlogisticated air, while the phlogiston unites to, and forms part of, the substance of the plant.

There are many circumstances which shew, that light has a remarkable power in enabling one body to absorb phlogiston from another. Mr. Senebier has observed, that the green tincture procured from the leaves of vegetables by spirit of wine, quickly loses its colour when exposed to the sun in a bottle not more than one-third part full, but does not do so in the dark, or if the bottle is quite full of the tincture, or if the air in it is phlogisticated; whence it is natural to conclude, that the light enables the dephlogisticated part of the air to absorb phlogiston from the tincture; and this appears to be really the case, as I find that the air in the bottle is considerably phlogisticated thereby. Dephlogisticated spirit of nitre also acquires a yellow colour, and becomes phlogisticated, by exposure to the sun's rays[1]; and I find on trial that the air in the bottle in which it is contained becomes dephlogisticated, or, in other words, receives an increase of dephlogisticated air, which shews that the change in the acid is not owing to the sun's rays communicating phlogiston to it, but to their enabling it to absorb phlogiston from the water contained in it, and thereby to produce dephlogisticated air. Mr. Scheele also found, that the dark colour acquired by luna cornea on exposure to the light, is owing to part of the silver being revived; and that gold, dissolved in aqua regia and deprived by distillation of the nitrous and superfluous marine acid, is revived by the same means; and there is the utmost reason to think, that, in both cases, the revival of the metal is owing to its absorbing phlogiston from the water.

[1] If spirit of nitre is distilled with a very gentle heat, the part which comes over is high coloured and fuming, and that which remains behind is quite colourless, and fumes much less than other nitrous acid of the same strength, and the fumes are colourless. This is called dephlogisticated spirit of nitre, as it appears to be really deprived of phlogiston by the process. The manner of preparing it, as well as its property of regaining its yellow colour by exposure to the light, is mentioned by Mr. Scheele in the *Stockholm Memoirs*, 1774.

Vegetables seem to consist almost intirely of fixed and phlogisticated air, united to a large proportion of phlogiston and some water, since by burning in the open air, in which their phlogiston unites to the dephlogisticated part of the atmosphere and forms water, they seem to be reduced almost intirely to water and those two kinds of air. Now plants growing in water without earth, can receive nourishment only from the water and air, and must therefore in all probability absorb their phlogiston from the water. It is known also that plants growing in the dark do not thrive well, and grow in a very different manner from what they do when exposed to the light.

From what has been said it seems likely that the use of light, in promoting the growth of plants and the production of dephlogisticated air from them, is, that it enables them to absorb phlogiston from the water. To this it may perhaps be objected, that though plants do not thrive well in the dark, yet they do grow, and should therefore, according to this hypothesis, absorb water from the atmosphere, and yield dephlogisticated air, which they have not been found to do. But we have no proof that they grew at all in any of those cases in which they were found not to yield dephlogisticated air; for though they will grow in the dark, yet their vegetative powers may perhaps at first be intirely checked by it, especially considering the unnatural situation in which they must be placed in such experiments. Perhaps too plants growing in the dark may be able to absorb phlogiston from water not much impregnated with dephlogisticated air, but not from water strongly impregnated with it; and consequently, when kept under water in the dark, may perhaps at first yield some dephlogisticated air, which, instead of rising to the surface, may be absorbed by the water, and, before the water is so much impregnated as to suffer any to escape, the plant may cease to vegetate, unless the water is changed. Unless therefore it could be shewn that plants growing in the dark, in water alone, will increase in size, without yielding dephlogisticated air, and without the water becoming more impregnated with it than before, no objection can be drawn from thence.

Mr. Senebier finds, that plants yield much more dephlogisticated air in distilled water impregnated with fixed air, than in plain distilled water, which is perfectly conformable to the abovementioned hypothesis; for as fixed air is a principal constituent part of vegetable substances, it is reasonable to suppose that the work of vegetation will go on better in water containing this substance, than in other water.

There are several memoirs of Mr. Lavoisier published by the Academy of Sciences, in which he intirely discards phlogiston, and explains those phænomena which have been usually attributed to the loss or attraction of that substance, by the absorption or expulsion of dephlogisticated air; and as not only the foregoing experiments, but most other phænomena of nature, seem explicable as well, or nearly as well, upon this as upon the commonly believed principle of phlogiston, it may be proper briefly to mention in what manner I would explain them on this principle, and why I have adhered to the other. In doing this, I shall not conform strictly to his theory, but shall make such additions and alterations as seem to suit it best to the phænomena; the more so, as the foregoing experiments may, perhaps, induce the author himself to think some such additions proper.

According to this hypothesis, we must suppose, that water consists of inflammable air united to dephlogisticated air; that nitrous air, vitriolic acid air, and the phosphoric acid, are also combinations of phlogisticated air, sulphur, and phosphorus, with dephlogisticated air; and that the two former, by a further addition of the same substance, are reduced to the common nitrous and vitriolic acids; that the metallic calces consist of the metals themselves united to the same substance, commonly, however, with a mixture of fixed air; that on exposing the calces of the perfect metals to a sufficient heat, all the dephlogisticated air is driven off, and the calces are restored to their metallic form; but as the calces of the imperfect metals are vitrified by heat, instead of recovering the metallic form, it should seem as if all the dephlogisticated air could not be driven off from them by heat alone. In like manner, according to this hypothesis, the rationale of the production of dephlogisticated air from red precipitate is, that during the solution of the quicksilver in the acid and the subsequent calcination, the acid is decompounded, and quits part of its dephlogisticated air to the quicksilver, whereby it comes over in the form of nitrous air, and leaves the quicksilver behind united to dephlogisticated air, which, by a further increase of heat, is driven off, while the quicksilver re-assumes its metallic form. In procuring dephlogisticated air from nitre, the acid is also decompounded; but with this difference, that it suffers some of its dephlogisticated air to escape, while it remains united to the alkali itself, in the form of phlogisticated nitrous acid. As to the production of dephlogisticated air from plants, it may be said, that vegetable substances consist chiefly of various combinations of three different bases, one of which, when united to dephlogisticated air, forms water, another fixed air, and the third phlogisticated air; and that by means of vegetation each of these substances are decomposed, and yield their dephlogisticated air; and that in burning they again acquire dephlogisticated air, and are restored to their pristine form.

It seems, therefore, from what has been said, as if the phænomena of nature might be explained very well on this principle, without the help of phlogiston; and indeed, as adding dephlogisticated air to a body comes to the same thing as depriving it of its phlogiston and adding water to it, and as there are, perhaps, no bodies entirely destitute of water, and as I know no way by which phlogiston can be transferred from one body to another, without leaving it uncertain whether water is not at the same time transferred, it will be very difficult to determine by experiment which of these opinions is the truest; but as the commonly received principle of phlogiston explains all phænomena, at least as well as Mr. Lavoisier's, I have adhered to that. There is one circumstance also, which though it may appear to many not to have much force, I own has some weight with me; it is, that as plants seem to draw their nourishment almost intirely from water and fixed and phlogisticated air, and are restored back to those substances by burning, it seems reasonable to conclude, that notwithstanding their infinite variety they consist almost intirely of various combinations of water and fixed and phlogisticated air, united according to one of these opinions to phlogiston, and deprived according to the other of dephlogisticated air; so that, according to the latter opinion, the substance of a plant is less compounded than a mixture of those bodies into which it is resolved by burning; and it is more reasonable to look for great variety in the more compound than in the more simple substance.

Another thing which Mr. Lavoisier endeavours to prove is, that dephlogisticated air is the acidifying principle. From what has been explained it appears, that this is no more than saying, that acids lose their acidity by uniting to phlogiston, which with regard to the nitrous, vitriolic, phosphoric, and arsenical acids is certainly true. The same thing, I believe, may be said of the acid of sugar; and Mr. Lavoisier's experiment is a strong confirmation of Bergman's opinion, that none of the spirit of nitre enters into the composition of the acid, but that it only serves to deprive the sugar of part of its phlogiston. But as to the marine acid and acid of tartar, it does not appear that they are capable of losing their acidity by any union with phlogiston. It is to be remarked also, that the acids of sugar and tartar, and in all probability almost all the vegetable and animal acids, are by burning reduced to fixed and phlogisticated air, and water, and therefore contain more phlogiston, or less dephlogisticated air, than those three substances.

PHILOSOPHICAL TRANSACTIONS. VOL. 74, 1784, p. 170

XV. *Answer to Mr.* Kirwan's *Remarks upon the Experiments on Air. By* Henry Cavendish, *Esq.*, *F.R.S. and S.A.*

Read March 4, 1784

IN a paper lately read before this Society, containing many experiments on air, I gave my reasons for supposing that the diminution which respirable air suffers by phlogistication, is not owing either to the generation or separation of fixed air from it; but without any arguments of a personal nature, or which related to any one person who espouses the contrary doctrine more than to another. This being contrary to the opinion maintained by Mr. Kirwan, he has written a paper in answer to it, which was read on the fifth of February. As I do not like troubling the Society with controversy, I shall take no notice of the arguments used by him, but shall leave them for the reader to form his own judgement of; much less will I endeavour to point out any inconsistencies or false reasonings, should any such have crept into it; but as there are two or three experiments mentioned there, which may perhaps be considered as disagreeing with my opinion, I beg leave to say a few words concerning them.

Mr. de Lassone found that filings of zinc, digested in a caustic fixed alkali, were partially dissolved with a small effervescence, and that the alkali was rendered in some measure mild. This mildness of the alkali Mr. Kirwan accounts for by supposing, that the inflammable air, which is separated during the solution, and causes the effervescence, unites to the atmospheric air contiguous to it, and thereby generates fixed air, which is absorbed by the alkali. But, in reality, the only circumstance from which Mr. de Lassone judged the alkali to become mild, was its making some effervescence when saturated with acids; and this effervescence is more likely to have proceeded from the expulsion of inflammable air than of fixed air, as it seems likely, that the zinc might be more completely deprived of its phlogiston by the acid than by the alkali.

In the abovementioned paper I say, Dr. Priestley observed, that quicksilver fouled by the addition of lead or tin, deposits a powder by agitation and exposure to the air, which consists in great measure of the calx of the imperfect metal. He found too some powder of this kind to contain fixed air; but it must be observed, that the powder used in this experiment was not prepared on purpose, but was procured from quicksilver fouled by having been used in various experiments, and may therefore have contained other impurities besides the metallic calces. On this Mr. Kirwan remarks, that Dr. Priestley did not at first prepare this powder on purpose, but he afterwards did so prepare it (4 PR. p. 148 and 149), and obtained a powder exactly of the same sort. It was natural to suppose from this remark, that Dr. Priestley must have obtained fixed air from the powder prepared on purpose, and that I had overlooked the passage; but, on turning to the pages referred to, I was surprised to find that it was otherwise, and that Dr. Priestley not so much as hints that he procured fixed air from the powder thus prepared.

With regard to the calcination of metals it may be proper to remark, that this operation is usually performed over the fire, by methods in which they are exposed to the fumes of the burning fuel, and which are so replete with fixed air, that it is not extraordinary, that the metallic calx should, in a short time, absorb a considerable quantity of it; and in particular red lead, which is the calx on which most experiments have been made, is always so prepared. There is another kind of calcination, however, called rusting, which is performed in the open air; but this is so slow an operation, that the rust may easily imbibe a sufficient quantity of fixed air, notwithstanding the small quantity of it usually contained in the atmosphere.

Mr. Kirwan allows that lime-water is not rendered cloudy by the mixture of nitrous and common air; but contends that this does not prove that fixed air is not generated by the union, as he thinks it may be absorbed by the nitrous selenite produced by the union of the nitrous acid with the lime. This induced me to try how small a quantity of fixed air would be perceived in this experiment. I accordingly repeated it in the same manner as described in my paper, except that I purposely added a little fixed air to the common air, and found that when this addition was $\frac{1}{78}$th of the bulk, or $\frac{1}{50}$th of the weight of the common air, the effect on the lime-water was such as could not possibly have been overlooked in my experiments. But as those who suppose fixed air to be generated by the mixture of nitrous and common air, may object to this manner of trying the experiment, and say, that the quantity of fixed air absorbed by the lime-water was really more than $\frac{1}{78}$th of the bulk of the common air, being equal to that quantity over and above the air generated by the mixture, I made another experiment in a different manner; namely, I filled a bottle with lime-water, previously mixed with as much nitrous acid as is contained in an equal bulk of nitrous air, and having inverted it into a vessel of the same, let up into it, in the same manner as in the above-mentioned experiments, a mixture of common air with $\frac{1}{78}$th of its bulk of fixed air, until it was half full. The event was the same as before; namely, the cloudiness produced in the lime-water was such that I could not possibly have overlooked. It must be observed, that in this experiment no fixed air could be generated, and a still greater proportion of the lime-water was turned into nitrous selenite than in the above-mentioned experiments; so that we may safely conclude, that if any fixed air is generated by the mixture of common and nitrous air, it must be less than $\frac{1}{75}$th of the bulk of the common air.

As for the nitrous selenite, it seems not to make the effect of the fixed air at all less sensible, as I found by filling two bottles with common air mixed with $\frac{1}{100}$dth of its bulk of fixed air, and pouring into each of them equal quantities of diluted lime-water; one of these portions of lime-water being previously diluted with an equal quantity of distilled water, and the other with the same quantity of a diluted solution of nitrous selenite, containing about $\frac{1}{400}$dth of its weight of calcareous earth; when I could not perceive that the latter portion of lime-water was rendered at all less cloudy than the former. Though the nitrous selenite, however, does not make the effect of the fixed air less sensible, yet the dilution of the lime-water, in consequence of some of the lime being absorbed by the acid, does; but, I believe, not in any remarkable degree.

There is an experiment mentioned by Mr. Kirwan which, though it cannot be considered as an argument in favour of the generation of fixed air, as he only supposes, without any proof, that fixed air is produced in it, does yet deserve to be taken notice of as a curious experiment. It is, that, if nitrous and common air be mixed over dry quicksilver, the common air is not at all diminished, that is, the bulk of the mixture will be not less than that of the common air employed, until water is admitted, and the mixture agitated for a few minutes. The reason of this in all probability is, that part of the phlogisticated nitrous acid, into which the nitrous air is converted, remains in the state of vapour until condensed by the addition of water. A proof that this is the real case is, that, in this manner of performing the experiment, the red fumes produced on mixing the airs remain visible for some hours, but immediately disappear on the addition of water and agitation.

The most material experiment alledged by Mr. Kirwan is one of Dr. Priestley's, in which he obtained fixed air from a mixture of red precipitate and iron filings. This at first seems really a strong argument in favour of the generation of fixed air; for though plumbago, which is known to consist chiefly of that substance, has lately been found to be contained in iron, yet one would not have expected it to be decompounded by the red precipitate, especially when the quantity of pure iron in the filings was much more than sufficient to supply the precipitate with phlogiston. The following experiment, however, shews that it was really decompounded; and that the fixed air obtained was not generated, but only separated by means of this decomposition.

500 grains of red precipitate mixed with 1000 of iron filings yielded, by

the assistance of heat, 7800 grain measures of fixed air, besides 2400 of a mixture of dephlogisticated and inflammable air, but chiefly the latter. The same quantity of iron filings, taken from the same parcel, was then dissolved in diluted oil of vitriol, so as to leave only the plumbago and other impurities. These mixed with 500 grains of the same red precipitate, and treated as before, yielded 9200 grain measures of fixed air, and 4200 of dephlogisticated air, of an indifferent quality, but without any sensible mixture of inflammable air. It appears, therefore, that less fixed air was produced when the red precipitate was mixed with the iron filings in substance, than when mixed only with the plumbago and other impurities; which shews, that its production was not owing to the iron itself, which seems to contain no fixed air, but to the plumbago, which contains a great deal. The reason, in all probability, why less fixed air was produced in the first case than [in] the latter is, that in the former more of the plumbago escaped being decompounded by the red precipitate than in the other. It must be observed, however, that the filings used in this experiment were mixed with about $\frac{1}{13}$th of their weight of brass, which was not discovered till they were dissolved in the acid, and which makes the experiment less decisive than it would otherwise be. The quantity of fixed air obtained is also much greater than, according to Mr. Bergman's experiment, could be yielded by the plumbago usually contained in 1000 grains of iron; so that though the experiment seems to shew that the fixed air was only produced by the decomposition of the impurities in the filings, yet it certainly ought to be repeated in a more accurate manner.

Before I conclude this paper, it may be proper to sum up the state of the argument on this subject. There are five methods of phlogistication considered by me in my paper on air; namely, first, the calcination of metals, either by themselves or when amalgamated with quicksilver; secondly, the burning of sulphur or phosphorus; thirdly, the mixture of nitrous air; fourthly, the explosion of inflammable air; and, fifthly, the electric spark; and Mr. Kirwan has not pointed out any other which he considers as unexceptionable. Now the last of these I by no means consider as unexceptionable, as it seems much most likely, that the phlogistication of the air in that experiment is owing to the burning or calcination of some substance contained in the apparatus[1]. It is true, that I have no proof of it; but there is so much probability in the opinion, that till it is proved to be erroneous, no conclusion can be drawn from such experiments in favour of the generation of fixed air. As to the first method, or the calcination of metals, there is not the least proof that any fixed air is generated, though we certainly have no direct proof of the contrary; nor

[1] In the experiment with the litmus I attribute the fixed air to the burning of the litmus, not decomposition, as Mr. Kirwan represents it, which is a sufficient reason why no fixed air should be found when the experiment is tried with air in which bodies will not burn.

did I in my paper insinuate that we had. The same thing may be said of the burning of sulphur and phosphorus. As to the mixture of nitrous air, and the combustion of inflammable air, it is proved, that if any fixed air is generated, it is so small as to elude the nicest test we have. It is certain too, that if it had been so much as $\frac{1}{70}$th of the bulk of the common air employed, it would have been perceived in the first of these methods, and would have been sensible in the second though still less. So that out of the five methods enumerated, it has been shewn, that in two no sensible quantity is generated, and not the least proof has been assigned that any is in two of the others; and as to the last, good reasons have been assigned for thinking it inconclusive; and therefore the conclusion drawn by me in the above-mentioned paper seems sufficiently justified; namely, that though it is not impossible that fixed air may be generated in some chemical processes, yet it seems certain, that it is not the general effect of phlogisticating air, and that the diminution of common air by phlogistication is by no means owing to the generation or separation of fixed air from it.

PHILOSOPHICAL TRANSACTIONS. VOL. 75, 1785, p. 372

XXIII. *Experiments on Air. By* Henry Cavendish, *Esq., F.R.S. and A.S.*

Read June 2, 1785

IN a Paper, printed in the last volume of the *Philosophical Transactions*, in which I gave my reasons for thinking that the diminution produced in atmospheric air by phlogistication is not owing to the generation of fixed air, I said it seemed most likely, that the phlogistication of air by the electric spark was owing to the burning of some inflammable matter in the apparatus; and that the fixed air, supposed to be produced in that process, was only separated from that inflammable matter by the burning. At that time, having made no experiments on the subject myself, I was obliged to form my opinion from those already published; but I now find, that though I was right in supposing the phlogistication of the air does not proceed from phlogiston communicated to it by the electric spark, and that no part of the air is converted into fixed air; yet that the real cause of the diminution is very different from what I suspected, and depends upon the conversion of phlogisticated air into nitrous acid.

The apparatus used in making the experiments was as follows. The air through which the spark was intended to be passed, was confined in a glass tube *M*, bent to an angle, as in fig. 1. (tab. XV.) which, after being filled with quicksilver, was inverted into two glasses of the same fluid, as in the figure. The air to be tried was then introduced by means of a small tube, such as is used for thermometers, bent in the manner represented by *ABC* (fig. 2.) the bent end of which, after being previously filled with quicksilver, was introduced, as in the figure, under the glass *DEF*, inverted into water, and filled with the proper kind of air, the end *C* of the tube being kept stopped by the finger; then, on removing the finger from *C*, the quicksilver in the tube descended in the leg *BC*, and its place was supplied with air from the glass *DEF*. Having thus got the proper quantity of air into the tube *ABC*, it was held with the end *C* uppermost, and stopped with the finger; and the end *A*, made smaller for that purpose, being introduced into one end of the bent tube *M*, (fig. 1.) the air, on removing the finger from *C*, was forced into that tube by the pressure of the quicksilver in the leg *BC*. By these means I was enabled to introduce the exact quantity I pleased of any kind of air into the tube *M*; and, by the same means, I could let up any quantity of soap-lees, or any other liquor which I wanted to be in contact with the air.

In one case, however, in which I wanted to introduce air into the tube many times in the same experiment, I used the apparatus represented in fig. 3. consisting of a tube *AB* of a small bore, a ball *C*, and a tube *DE* of a larger bore. This apparatus was first filled with quicksilver; and then the ball *C*, and the tube *AB*, were filled with air, by introducing the end *A* under a glass inverted into water, which contained the proper kind of air, and drawing out the quicksilver from the leg *ED* by a syphon. After being thus furnished with air, the apparatus was weighed, and the end *A* introduced into one end of the tube *M*, and kept there during the experiment; the way of forcing air out of this apparatus into the tube being by thrusting down the tube *ED* a wooden cylinder of such a size as almost to fill up the whole bore, and by occasionally pouring quicksilver into the same tube, to supply the place of that pushed into the ball *C*. After the experiment was finished, the apparatus was weighed again, which shewed exactly how much air had been forced into the tube *M* during the whole experiment; it being equal in bulk to a quantity of quicksilver, whose weight was equal to the increase of weight of the apparatus.

The bore of the tube *M* used in most of the following experiments, was about one-tenth of an inch; and the length of the column of air, occupying the upper part of the tube, was in general from $1\frac{1}{2}$ to $\frac{3}{4}$ of an inch.

It is scarcely necessary to inform any one used to electrical experiments, that in order to force an electrical spark through the tube, it was necessary, not to make a communication between the tube and the conductor, but to place an insulated ball at such a distance from the conductor as to receive a spark from it, and to make a communication between that ball and the quicksilver in one of the glasses, while the quicksilver in the other glass communicated with the ground.

I now proceed to the experiments.

When the electric spark was made to pass through common air, included between short columns of a solution of litmus, the solution acquired a red colour, and the air was diminished, conformably to what was observed by Dr. Priestley.

When lime-water was used instead of the solution of litmus, and the spark was continued till the air could be no further diminished, not the least cloud could be perceived in the lime-water; but the air was reduced to two-thirds of its original bulk; which is a greater diminution than it could have suffered by mere phlogistication, as that is very little more than one-fifth of the whole.

The experiment was next repeated with some impure dephlogisticated

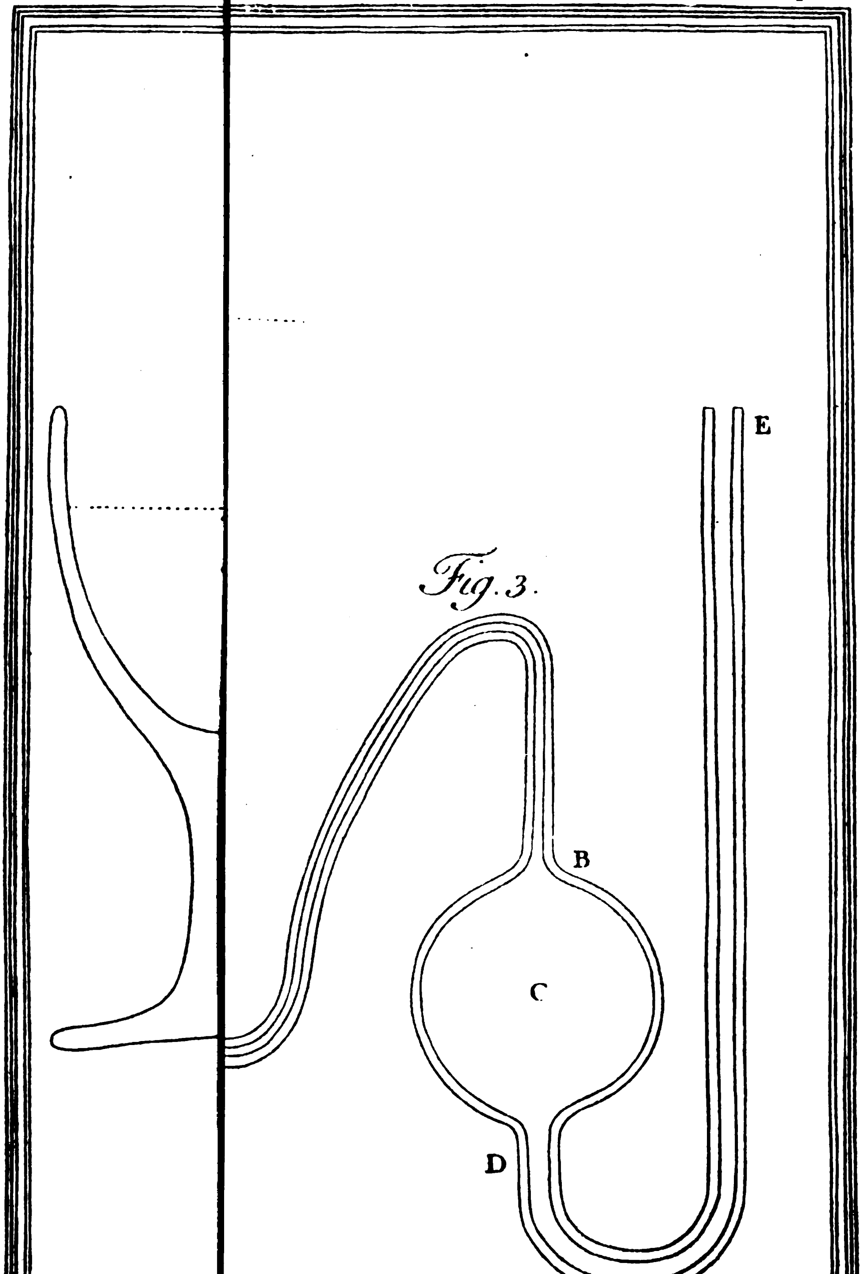
Fig. 3.
B
C
D
E

air. The air was very much diminished, but without the least cloud being produced in the lime-water. Neither was any cloud produced when fixed air was let up to it; but on the further addition of a little caustic volatile alkali, a brown sediment was immediately perceived.

Hence we may conclude, that the lime-water was saturated by some acid formed during the operation; as in this case it is evident, that no earth could be precipitated by the fixed air alone, but that caustic volatile alkali, on being added, would absorb the fixed air, and thus becoming mild, would immediately precipitate the earth; whereas, if the earth in the lime-water had not been saturated with an acid, it would have been precipitated by the fixed air. As to the brown colour of the sediment, it most likely proceeded from some of the quicksilver having been dissolved.

It must be observed, that if any fixed air, as well as acid, had been generated in these two experiments with the lime-water, a cloud must have been at first perceived in it, though that cloud would afterwards disappear by the earth being re-dissolved by the acid; for till the acid produced was sufficient to dissolve the whole of the earth, some of the remainder would be precipitated by the fixed air; so that we may safely conclude, that no fixed air was generated in the operation.

When the air is confined by soap-lees, the diminution proceeds rather faster than when it is confined by lime-water; for which reason, as well as on account of their containing so much more alkaline matter in proportion to their bulk, soap-lees seemed better adapted for experiments designed to investigate the nature of this acid, than lime-water. I accordingly made some experiments to determine what degree of purity the air should be of, in order to be diminished most readily, and to the greatest degree; and I found, that, when good dephlogisticated air was used, the diminution was but small; when perfectly phlogisticated air was used, no sensible diminution took place; but when five parts of pure dephlogisticated air were mixed with three parts of common air, almost the whole of the air was made to disappear.

It must be considered, that common air consists of one part of dephlogisticated air, mixed with four of phlogisticated; so that a mixture of five parts of pure dephlogisticated air, and three of common air, is the same thing as a mixture of seven parts of dephlogisticated air with three of phlogisticated.

Having made these previous trials, I introduced into the tube a little soap-lees, and then let up some dephlogisticated and common air, mixed in the above-mentioned proportions, which rising to the top of the tube *M*, divided the soap-lees into its two legs. As fast as the air was diminished by the electric spark, I continued adding more of the same kind, till no further diminution took place: after which a little pure dephlogisticated air, and after that a little common air, were added, in order to see whether the cessation of diminution was not owing to some imperfection in the proportion of the two kinds of air to each other; but without effect[1]. The soap-lees being then poured out of the tube, and separated from the quicksilver, seemed to be perfectly neutralized, as they did not at all discolour paper tinged with the juice of blue flowers. Being evaporated to dryness, they left a small quantity of salt, which was evidently nitre, as appeared by the manner in which paper, impregnated with a solution of it, burned.

For more satisfaction, I tried this experiment over again on a larger scale. About five times the former quantity of soap-lees were now let up into a tube of a larger bore; and a mixture of dephlogisticated and common air, in the same proportions as before, being introduced by the apparatus represented in fig. 3. the spark was continued till no more air could be made to disappear. The liquor, when poured out of the tube, smelled evidently of phlogisticated nitrous acid, and being evaporated to dryness, yielded $1\frac{4}{10}$ gr. of salt, which is pretty exactly equal in weight to the nitre which that quantity of soap-lees would have afforded if saturated with nitrous acid. This salt was found, by the manner in which paper dipped into a solution of it burned, to be true nitre. It appeared, by the test of *terra ponderosa salita*, to contain not more vitriolic acid than the soap-lees themselves contained, which was excessively little; and there is no reason to think that any other acid entered into it, except the nitrous.

A circumstance, however, occurred, which at first seemed to shew, that this salt contained some marine acid; namely, an evident precipitation took place when a solution of silver was added to some of it dissolved in water; though the soap-lees used in its formation were perfectly free from marine acid, and though, to prevent all danger of any precipitate being formed by an excess of alkali in it, some purified nitrous acid had been added to it, previous to the addition of the solution of silver. On consideration, however, I suspected, that this precipitation might arise from the nitrous acid in it being phlogisticated; and therefore I tried whether nitre, much phlogisticated, would precipitate silver from its solution. For this purpose I exposed some nitre to the fire, in an earthen retort, till it had yielded a good deal of dephlogisticated air; and then, having dissolved it in water, and added to it some well-purified spirit of nitre till it was sensibly acid, in order to be certain that the alkali did not predominate, I dropped into it some solution of silver, which immediately made a very copious precipitate. This solution, however, being deprived of some of its phlogiston by evaporation to dryness, and exposure for a few weeks to the

[1] From what follows it appears, that the reason why the air ceased to diminish was, that as the soap-lees were then become neutralized, no alkali remained to absorb the acid formed by the operation, and in consequence scarce any air was turned into acid. The spark, however, was not continued long enough after the apparent cessation of diminution, to determine with certainty, whether it was only that the diminution went on remarkably slower than before, or that it was almost come to a stand, and could not have been carried much further, though I had persisted in passing the sparks.

air, lost the property of precipitating silver from its solution; a proof that this property depended only on its phlogistication, and not on its having absorbed sea-salt from the retort, or by any other means.

Hence it is certain, that nitre, when much phlogisticated, is capable of making a precipitate with a solution of silver; and therefore there is no reason to think, that the precipitate, which our salt occasioned with a solution of silver, proceeded from any other cause than that of its being phlogisticated; especially as it appeared by the smell, both on first taking it out of the tube, and on the addition of the spirit of nitre, previous to dropping in the solution of silver, that the acid in it was much phlogisticated. This property of phlogisticated nitre is worth the attention of chemists; as otherwise they may sometimes be led into mistakes, in investigating the presence of marine acid by a solution of silver.

In the above-mentioned Paper I said, that when nitre is detonated with charcoal, the acid is converted into phlogisticated air; that is, into a substance which, as far as I could perceive, possesses all the properties of the phlogisticated air of our atmosphere; from which I concluded, that phlogisticated air is nothing else than nitrous acid united to phlogiston. According to this conclusion, phlogisticated air ought to be reduced to nitrous acid by being deprived of its phlogiston. But as dephlogisticated air is only water deprived of phlogiston, it is plain, that adding dephlogisticated air to a body, is equivalent to depriving it of phlogiston, and adding water to it; and therefore, phlogisticated air ought also to be reduced to nitrous acid, by being made to unite to, or form a chemical combination with, dephlogisticated air; only the acid formed this way will be more dilute, than if the phlogisticated air was simply deprived of phlogiston.

This being premised, we may safely conclude, that in the present experiments the phlogisticated air was enabled, by means of the electrical spark, to unite to, or form a chemical combination with, the dephlogisticated air, and was thereby reduced to nitrous acid, which united to the soap-lees, and formed a solution of nitre; for in these experiments those two airs actually disappeared, and nitrous acid was actually formed in their room; and as, moreover, it has just been shewn, from other circumstances, that phlogisticated air must form nitrous acid, when combined with dephlogisticated air, the above-mentioned opinion seems to be sufficiently established. A further confirmation of it is, that, as far as I can perceive, no diminution of air is produced when the electric spark is passed either through pure dephlogisticated air, or through perfectly phlogisticated air; which indicates the necessity of a combination of these two airs to produce the acid. Moreover, it was found in the last experiment, that the quantity of nitre procured was the same that the soap-lees would have produced if saturated with nitrous acid; which shews, that the production of the nitre was not owing to any decomposition of the soap-lees.

It may be worth remarking, that whereas in the detonation of nitre with inflammable substances, the acid unites to phlogiston, and forms phlogisticated air, in these experiments the reverse of this process was carried on; namely, the phlogisticated air united to the dephlogisticated air, which is equivalent to being deprived of its phlogiston, and was reduced to nitrous acid.

In the above-mentioned Paper I also gave my reasons for thinking, that the small quantity of nitrous acid, produced by the explosion of dephlogisticated and inflammable air, proceeded from a portion of phlogisticated air mixed with the dephlogisticated, which I supposed was deprived of its phlogiston, and turned into nitrous acid, by the action of the dephlogisticated air on it, assisted by the heat of the explosion. This opinion, as must appear to every one, is confirmed in a remarkable manner by the foregoing experiments; as from them it is evident, that dephlogisticated air is able to deprive phlogisticated air of its phlogiston, and reduce it into acid, when assisted by the electric spark; and therefore it is not extraordinary that it should do so, when assisted by the heat of the explosion.

The soap-lees used in the foregoing experiments were made from salt of tartar, prepared without nitre; and were of such a strength as to yield one-tenth of their weight of nitre when saturated with nitrous acid. The dephlogisticated air also was prepared without nitre, that used in the first experiment with the soap-lees being procured from the black powder formed by the agitation of quicksilver mixed with lead[1], and that used in the latter from turbith mineral. In the first experiment, the quantity of soap-lees used was 35 measures, each of which was equal in bulk to one grain of quicksilver; and that of the air absorbed was 416 such measures of phlogisticated air, and 914 of dephlogisticated. In the second experiment, 178 measures of soap-lees were used, and they absorbed 1920 of phlogisticated air, and 4860 of dephlogisticated. It must be observed, however, that in both experiments some air remained in the tube uncondensed, whose degree of purity I had no way of trying; so that the proportion of each species of air absorbed is not known with much exactness.

As far as the experiments hitherto published extend, we scarcely know more of the nature of the phlogisticated part of our atmosphere, than that it is not diminished by lime-water, caustic alkalies, or nitrous air; that it is unfit to support fire, or maintain life in animals; and that its specific gravity is not much less than that of common air: so that, though the nitrous acid, by being united to phlogiston, is converted into air possessed of these properties, and consequently, though it was reasonable to suppose, that part at least of the phlogisticated air of the atmosphere consists of this acid united to phlogiston, yet it might fairly be doubted whether the whole is of this kind, or whether there are not in reality many different

[1] This air was as pure as any that can be procured by most processes. I propose giving an account of the experiment, in which it was prepared, in a future Paper.

substances confounded together by us under the name of phlogisticated air. I therefore made an experiment to determine, whether the whole of a given portion of the phlogisticated air of the atmosphere could be reduced to nitrous acid, or whether there was not a part of a different nature from the rest, which would refuse to undergo that change. The foregoing experiments indeed in some measure decided this point, as much the greatest part of the air let up into the tube lost its elasticity; yet, as some remained unabsorbed, it did not appear for certain whether that was of the same nature as the rest or not. For this purpose I diminished a similar mixture of dephlogisticated and common air, in the same manner as before, till it was reduced to a small part of its original bulk. I then, in order to decompound as much as I could of the phlogisticated air which remained in the tube, added some dephlogisticated air to it, and continued the spark till no further diminution took place. Having by these means condensed as much as I could of the phlogisticated air, I let up some solution of liver of sulphur to absorb the dephlogisticated air; after which only a small bubble of air remained unabsorbed, which certainly was not more than $\frac{1}{120}$ of the bulk of the phlogisticated air let up into the tube; so that if there is any part of the phlogisticated air of our atmosphere which differs from the rest, and cannot be reduced to nitrous acid, we may safely conclude, that it is not more than $\frac{1}{120}$ part of the whole.

The foregoing experiments shew, that the chief cause of the diminution which common air, or a mixture of common and dephlogisticated air, suffers by the electric spark, is the conversion of the air into nitrous acid; but yet it seemed not unlikely, that when any liquor, containing inflammable matter, was in contact with the air in the tube, some of this matter might be burnt by the spark, and thereby diminish the air, as I supposed in the above-mentioned Paper to be the case. The best way which occurred to me of discovering whether this happened or not, was to pass the spark through dephlogisticated air, included between different liquors: for then, if the diminution proceeded solely from the conversion of air into nitrous acid, it is plain that, when the dephlogisticated air was perfectly pure, no diminution would take place; but when it contained any phlogisticated air, all this phlogisticated air, joined to as much of the dephlogisticated air as must unite to it in order to reduce it into acid, that is, two or three times its bulk, would disappear, and no more; so that the whole diminution could not exceed three or four times the bulk of the phlogisticated air: whereas, if the diminution proceeded from the burning of the inflammable matter, the purer the dephlogisticated air was, the greater and quicker would be the diminution.

The result of the experiments was, that when dephlogisticated air, containing only $\frac{1}{20}$ of its bulk of phlogisticated air (that being the purest air I then had), was confined between short columns of soap-lees, and the spark passed through it till no further diminution could be perceived, the air lost $\frac{43}{200}$ of its bulk; which is not a greater diminution than might very likely proceed from the first-mentioned cause; as the dephlogisticated air might easily be mixed with a little common air while introducing into the tube.

When the same dephlogisticated air was confined between columns of distilled water, the diminution was rather greater than before, and a white powder was formed on the surface of the quicksilver beneath; the reason of which, in all probability, was, that the acid produced in the operation corroded the quicksilver, and formed the white powder; and that the nitrous air, produced by that corrosion, united to the dephlogisticated air, and caused a greater diminution than would otherwise have taken place.

When a solution of litmus was used, instead of distilled water, the solution soon acquired a red colour, which grew paler and paler as the spark was continued, till at last it became quite colourless and transparent. The air was diminished by almost half, and I believe might have been still further diminished, had the spark been continued. When lime-water was let up into the tube, a cloud was formed, and the air was further diminished by about one-fifth. The remaining air was good dephlogisticated air. In this experiment, therefore, the litmus was, if not burnt, at least decompounded, so as to lose entirely its purple colour, and to yield fixed air; so that, though soap-lees cannot be decompounded by this process, yet the solution of litmus can, and so very likely might the solutions of many other combustible substances. But there is nothing, in any of these experiments, which favours the opinion of the air being at all diminished by means of phlogiston communicated to it by the electric spark.

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XIII. *An Account of Experiments made by Mr.* John M^cNab, *at* Henley House, Hudson's Bay, *relating to freezing Mixtures. By* Henry Cavendish, *Esq.*, *F.R.S. and A.S.*

Read February 23, 1786

IN my observations on Mr. Hutchins's Experiments, printed in the LXXIIId volume of the *Philosophical Transactions*, I gave my opinion concerning the cause of the cold produced by mixing snow with different liquors. As there were some circumstances, however, which seemed to form a difficulty in the way of this opinion, I was desirous of having further experiments made on the subject; and at the same time I thought that, by proper management, a greater degree of cold might be produced than had hitherto been done. On mentioning the experiments I wished to have made to Mr. Hutchins, he very obligingly desired Mr. M^cNab, Master at Henley-House, to try them; who was so good as to undertake the business, and has executed it in the most satisfactory manner; as he has not only taken great pains, but has shewn the utmost attention and accuracy, in observing and relating all the phænomena which occurred, and has manifested great judgement in frequently adapting the manner of trying the experiments to appearances which occurred in former ones, to which we are indebted for great part of the most curious facts in this paper. His endeavours have also been attended with much success, as he has not only shewn many remarkable circumstances relating to the freezing of the nitrous and vitriolic acids, and the phænomena of freezing mixtures; but has also produced degrees of cold greatly superior to any before known.

1. In the above-mentioned Paper I said, that the cold produced by mixing spirit of nitre with snow, is owing to the melting of the snow; and that in all probability there is a certain degree of cold, in which spirit of nitre is so far from dissolving snow, that it will yield out part of its own water, and suffer that to freeze, as is the case with solutions of common salt; so that if the cold of the materials, before mixing, is equal to this, no additional cold can be produced. A circumstance, however, which at first sight seems repugnant to this opinion, occurred in an experiment of

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Fahrenheit's for producing cold by a mixture of spirit of nitre and ice; namely, that the acid, which had been repeatedly cooled by different frigorific mixtures, was found frozen before it was mixed with the ice; notwithstanding which, cold was produced by the mixture. Professor Braun also found, that cold was produced by mixing frozen spirit of nitre with snow. On consideration, however, this appeared by no means inconsistent with the opinion there laid down, as there was great reason to think, that the freezing of the acid was of a different kind from that considered in the above-mentioned Paper, and that it did not proceed from the watery part separating from the rest and freezing; but that the whole acid, or perhaps the more concentrated part, froze; in which case it would not be extraordinary that the acid should dissolve more snow, and produce cold.

2. To clear up this point, I sent to Hudson's Bay a bottle of spirit of nitre, of nearly the same strength as Fahrenheit's; and desired Mr. M^c^Nab to expose it to the cold, and, if it froze, to ascertain the temperature, and decant the fluid part into another bottle, and send both home to be examined, as it would thereby be known, whether it was the whole acid, or only the watery part, which froze. For the same purpose also I sent some dephlogisticated spirit of nitre of the same strength, and also some strong oil of vitriol. I also sent some spirit of nitre and spirit of wine, both diluted with so much water, that it was expected, that with the cold of Hudson's Bay they would suffer the first kind of congelation; that is, their watery part would freeze, and thereby make the difference between the two kinds of freezing more apparent.

3. In the same Paper I say,

That on adding snow gradually to some of the spirit of nitre used by Mr. Hutchins, I found, that the addition of a small quantity produced heat instead of cold; and it was not until so much was added as to increase the heat from 28° to 51°, that the addition of more snow began to produce cold; the quantity of snow required for this purpose being pretty exactly one-quarter of the weight of the spirit of nitre, and the heat of the snow and air of the room, as well as the acid, being 28°. The reason of this is, that a great deal of heat is produced by mixing water with spirit of nitre, and the stronger the spirit is, the greater is the heat produced. Now it appears from this experiment, that before the acid was diluted, the heat produced by its union with the water formed from the melted snow was greater than the cold produced by the melting of the snow; and it was not till it was diluted by the addition of one-quarter of its weight of that substance, that the cold generated by the latter cause began to exceed the heat generated by the former. From what has been said, it is evident, that the cold of a freezing mixture, made with the undiluted acid, cannot be quite so great as that made with the same acid, diluted with a quarter of its weight of water, supposing the acid and snow to be both at 28° of heat; and there is no reason to think, that the event will be different if they are colder; for the undiluted acid will not begin to generate cold, until so much snow is dissolved as to increase its heat from 28° to 51°, so that no greater cold will be produced, than would be obtained by mixing the diluted acid heated to 51° with snow of the heat of 28°. This method of adding snow gradually to an acid, is much the best way I know of finding what strength it ought to be of, in order to produce the greatest effect possible.

As it seemed likely that, by following this method, a greater degree of cold might be produced than had been done hitherto, I sent three other bottles of spirit of nitre and oil of vitriol, all three diluted, but not so much so, but that I thought they would require a little further dilution, in order to reduce them to their properest degree of strength. I also sent a bottle of highly rectified spirit of wine, and a mixture of equal quantities of the above-mentioned common spirit of nitre and oil of vitriol; and desired Mr. McNab to find what degree of cold could be produced by mixing them with snow, after having first reduced them, in the above-mentioned manner, to their best degree of strength[1].

He was also desired to ascertain how much snow he added; for as their strength was determined before they were sent out, it would thereby be known what was the best strength of these liquors for frigorific mixtures.

All these bottles were numbered with a diamond; and as I shall sometimes distinguish them by these numbers, and as it may be of use to those who may consult the original, I have added the following list of these bottles, with their contents.

No.	Liquors mentioned in Art. 3	Weight of marble which they dissolve	Specific gravity at 60° of heat
168	Spirit of nitre	,582	1,4371
27	Dephlogisticated spirit of nitre	,53	1,4040
103	Diluted oil of vitriol	,654	1,5596
28	Equal weights of No. 168 and No. 103 ...	—	—
8	Very highly rectified spirit of wine	—	,8195
	Liquors mentioned in Art. 2		
151	Strong oil of vitriol	,98	1,8437
142	Spirit of nitre	,525	1,4043
139	Some of the same diluted with twice its weight of water	—	—
141	Dephlogisticated spirit of nitre	,53	1,4033
143	Some of the same spirit of wine as in No. 8 diluted with 1½ its weight of water ...	—	-
72	Diluted oil of vitriol for comparing the thermometers	,629	
171	Oil of vitriol of about the usual strength, but the exact strength not known, intended to refresh the former when too weak	—	—

[1] This might have been done at home; but I thought it not unlikely that the strength found this way might differ, in some measure, according to the heat in which the experiment was tried.

4. Professor Braun says, that by mixtures of snow and spirit of nitre he sunk thermometers filled with oil of sassafras, and some other essential oils, to – 100° or – 124°; and that, by the same means, he sunk thermometers filled with the highest rectified spirit of wine to – 148°. Though there seemed great reason to think, from Mr. Hutchins's experiments, that there must be some mistake in this; yet, as it was possible that the essential oils, and even spirit of wine of a strength much different from that with which Mr. Hutchins's thermometers were filled, might follow a considerably different progression in their contraction by great degrees of cold, I sent a thermometer filled with oil of sassafras, and two others with spirits of wine. One of these last was filled with the highest rectified spirits I could procure, its specific gravity at 60° of heat being ,8185; the other was intended to be filled with common spirits, though from circumstances I am inclined to suspect *that* also to have been filled with the best spirits. Besides these, there was sent a mercurial thermometer, accurately adjusted, according to the directions of the Committee of the Royal Society, printed in the LXVIIth volume of the *Transactions*; and also the two spirit thermometers used by Mr. Hutchins, which were filled with spirits whose specific gravity was ,8247.

5. These thermometers were compared together by exposing them to the cold, with their balls immersed in a glass vessel filled with diluted oil of vitriol. They were at times also compared in cold more violent than the natural cold of the climate, by adding snow to the acid in which they were tried, in which case care was taken to keep the mixture frequently stirred. Oil of vitriol was recommended for this purpose, as a fluid which would most likely bear any degree of cold without freezing, and whose natural cold might be much increased by the addition of snow. It seems to have answered the purpose very well, and not to have been attended with any inconvenience.

During the first comparison of these thermometers, a whitish globule, such as those which appear in frozen oil, was observed in the tube of the thermometer filled with oil of sassafras. This appearance of congelation did not much increase; but two days after a large air bubble was found in its ball, which prevented Mr. McNab from making further observations with it.

It is well known, that spirit of wine expands more by a given number of degrees of a mercurial thermometer in warm temperatures than in cold ones; and this inequality, as might be expected, was less in the stronger spirit than in the weaker, but the difference was inconsiderable. The oil of sassafras also had some of this inequality, but much less. It however appears to be by no means a proper fluid for filling thermometers with. No appearance was observed which indicates any considerable irregularity in the contraction of spirits of wine in intense cold, or which renders it probable, that thermometers filled therewith could be sunk by a mixture of snow and spirit of nitre to a degree near approaching to that mentioned by Professor Braun.

6. Mr. M^c^Nab in his experiments sometimes used one thermometer and sometimes another; but in the following pages I have reduced all the observations to the same standard; namely, in degrees of cold less than that of freezing mercury I have set down that degree which would have been shewn by the mercurial thermometer in the same circumstances; but as that could not have been done in greater degrees of cold, as the mercurial thermometer then becomes of no use, I found how much lower the mercurial thermometer stood at its freezing point, than each of the spirit thermometers, and increased the cold shewn by the latter by that difference.

On the common and dephlogisticated Acids of Nitre.

The following experiments shew, that both these acids are capable of a kind of congelation, in which the whole, and not merely the watery part, freezes. Their freezing point also differs greatly according to the strength, and varies according to a very unexpected law. Like water too they bear being cooled very much below their freezing point before the congelation begins, and as soon as that takes place, immediately rise up to the freezing point.

7. On the morning of Feb. 1 the common and dephlogisticated spirits of nitre, No. 142 and 141, whose specific gravities were 1,4043 and 1,4033, were found clear and fluid, the cold of the air at that time being $-47°$. They also bore being shook without any alteration; but on taking out their stoppers, both of them in a few minutes began to freeze, the congelation beginning by a white appearance at top, which gradually spread to the bottom; and they became so thick as not to move on inclining the phial. For want of a thermometer whose ball reached far enough below its scale, Mr. M^c^Nab was not able to determine their cold while in the bottle; but in somewhat more than an hour's time, the frozen acid had so much subsided as to admit of his pouring a little fluid matter out of each into a glass with a thermometer in it[1]; whereby the cold of the common spirit of nitre was found to be $-31°\frac{1}{2}$, and that of the dephlogisticated acid $-30°$, the temperature of the air being $-41°$. Each of these decanted liquors, at the time their temperature was tried, was full of small *spicula* of ice: they were then put into phials well stopped, and they, as well as the undecanted liquors, sent home to be examined. The decanted part of the common spirit of nitre dissolved ,535 of its weight of marble, and the un-

[1] It may be asked, why it was more possible to decant any liquor at this time than at first, as the acid was all the while exposed to a cold much below the freezing point? The reason in all probability is, not that any part of the ice first formed dissolved, but that the small filaments into which it shot collected together, and in some measure subsided to the bottom.

decanted part ,523; for which reason I shall call the strength of the former ,535, and that of the latter ,523; which mode of reckoning is observed in the remainder of this Paper. The strength of the decanted part of the dephlogisticated acid was ,56, and that of the undecanted part ,528; so that it appears that in each of these acids the unfrozen part was a little stronger than the frozen part. It is remarkable, that in the common spirit of nitre, the decanted part, though stronger than the other, was paler coloured and less fuming.

8. On Dec. 21, the temperature of the air being − 28°, some dephlogisticated spirit of nitre (No. 27), of nearly the same strength as the former acid, was poured into a jar, in order to be diluted with snow, as recommended in Art. 2. Immediately after it was decanted, it began to freeze, in the same manner as before described, except that a less portion of it seems to have congealed: its temperature, tried by dipping a thermometer into it, was − 19°, where it remained stationary for many minutes; it was then diluted with snow, as will be mentioned in Art. 14, whereby its strength was reduced to ,434.

9. On Dec. 29th, this diluted acid was completely melted, and half of it poured into a jar with a ground stopper, and both portions exposed to the air. In the morning they were perfectly fluid; but on taking the stopper out of the jar, and dipping in it a thermometer, the acid immediately froze, beginning by forming a white coat round the ball of the thermometer, which gradually spread through the whole fluid; and at the same time the thermometer rose till it stood stationary at − 5°. The cold of the acid before it began to freeze must have been about − 30°½, that being the temperature of a glass of vitriolic acid standing near it; but the thermometer which was dipped into it was five or six degrees colder, which seems to be the cause of the congelation beginning round the ball.

In the afternoon a thermometer was dipped into the other half of the acid, where, as the weather had grown less cold, it stood above a minute at − 25°, without freezing; then, however, the acid froze, with the same appearance as in the morning, and at the same time the thermometer rose to − 4°, and became stationary.

This acid, being left in the air with the thermometer in it, was found in the evening at − 45°; it however was not intirely frozen, being only thick as an unguent, which shews that the unfrozen part must have been of a different strength from the frozen part; but it does not appear whether stronger or weaker. The next morning it was frozen solid, though the cold was only half a degree greater. On Jan. 16th, this acid was again tried in the same manner; it then suffered a thermometer, whose ball had been previously warmed in the hand, to be dipped into it, and remain there several minutes without freezing, though its temperature was − 35°. But on lifting up the thermometer, a drop fell from its ball into the acid, which immediately set it a freezing, and it rose up to − 4°½.

10. On Dec. 22d, the spirit of nitre (No. 168) which a few days before had been diluted with snow, so as to be reduced to the strength of ,411, was divided into two equal parts, and exposed to the cold. On Dec. 29th, when the temperature of the air was $-17°\frac{1}{2}$, one of these parts was found beginning to freeze; the other was fluid, but began to freeze on dipping in a thermometer; the thermometer in both kept stationary at $-1°\frac{1}{2}$. The latter was twice re-melted and exposed to the cold, and both times the temperature of the frozen acid came out the same as before.

11. The white colour of the ice in these experiments seems owing only to its consisting of very slender filaments; for in some cases, where it froze slower, and where, in consequence, it shot into larger solid masses, they were transparent, and of the same colour as the acid itself. By the continuance of a sufficient cold, the acid, which by hasty freezing put on the white appearance, would become hard solid ice, but yet still retained its white appearance, owing perhaps to the filaments first shot consisting of an acid differing in strength from that which froze afterwards, and filled up the interstices.

In all these experiments, whether the ice was formed into minute filaments or solid masses, still, whenever there was a sufficient quantity of fluid matter to admit of it, they constantly subsided to the bottom; a proof that the frozen part was heavier than the unfrozen. The difference indeed is so great, that in one case where it froze into solid crystals on the surface, these crystals, when detached by agitation, fell with force enough to make a tinkling noise against the bottom of the glass.

These acids contract very much on freezing. Whenever the acid is frozen solid, the surface, instead of being elevated in ridges, like frozen water, is depressed and full of cracks. In one experiment Mr. McNab, after a glass almost full of acid was nearly frozen, filled it to the brim with fresh acid; and then, after it was completely frozen, the surface was visibly depressed, with fissures one-eighth of an inch broad, extending from top to bottom. It is this contraction of the acid in freezing which makes the frozen part subside in the fluid part; as it was found, in the undiluted acid, that the latter consisted of a stronger, and consequently heavier, acid than the former. But still the subsidence of the frozen part shews, that the ice is not mere water, or even a very dilute acid; which indeed was proved by the examination of the liquors sent home.

The ninth and tenth articles shew, that though the acids bear being cooled greatly below the freezing point, without any congelation taking place, yet as soon as they begin to freeze they immediately rise up to their freezing point; and this point is always very nearly, if not exactly, the same in the same acid; for those acids were frozen and melted again three or four times, and were cooled considerably more below the freezing point in one trial than another, and yet as soon as they began to freeze the thermometer immersed in them constantly rose nearly to the same point.

The quantity which these acids will bear being cooled below the freezing point, without freezing, is remarkable. The diluted spirit of nitre, whose freezing point is $-1°\frac{1}{2}$, once bore being cooled to near $-39°$, without freezing, that is, near 37 degrees below its freezing point. The diluted dephlogisticated spirit of nitre, whose freezing point is $-5°$, bore cooling to $-35°$; and the dephlogisticated spirit of nitre (141) whose true freezing point is most likely $-19°$ (*see next article*) bore being cooled to $-49°$: perhaps too they might have born to be cooled considerably lower without freezing, but how much does not appear. It must be observed, however, that the same diluted spirit which at one time bore being cooled to $-39°$, at another froze, without any apparent cause, when its cold was certainly less than $-30°$, and most likely not much below $-18°$.

12. The freezing point differs remarkably, according to the strength of the acid. In the diluted dephlogisticated and common spirit of Art. 7 and 8, the freezing point was $-5°$ and $-1°\frac{1}{2}$. In the dephlogisticated and common spirit of Art. 5 the decanted parts of which were stronger than the foregoing in scarcely so great a proportion as that of four to three, it seemed to be $-30°$ and $-31°\frac{1}{2}$. It may indeed be suspected, that as this point was determined only by pouring a small quantity of the acid into a glass, at a time when the air and glass were much colder than the acid, these decanted liquors might be cooled by the air and glass, and thereby make the freezing point appear lower than it really was: but I do not think this could be the case; for as the decanted liquors were full of small filaments of ice, they could hardly be cooled sensibly below their freezing points without freezing; and any cold, communicated to them by the air or glass, would serve only to convert more of them into ice, without sensibly increasing their cold: so that I think this experiment determines the true freezing point of their decanted part; but it must be observed, that as the decanted part was rather stronger than the rest, it is very possible that the freezing point of the undecanted part might be considerably less cold.

A circumstance which might incline one to think, that the way by which the freezing point was determined in this experiment is defective is, that the freezing point of the dephlogisticated acid No. 27, though nearly of the same strength as that last mentioned, but rather stronger, was much less low, being only $-19°$. But I have little doubt that the true reason of this is, that in the former acid the strength of the decanted part, which is the part whose freezing point was tried, was found to be at least $\frac{1}{20}$ greater than that of the whole mass; whereas in No. 27 the fluid part was in all probability not sensibly stronger than the whole mass; for as No. 27 was cooled only seven degrees below the freezing point, and its temperature was tried soon after its beginning to freeze, not much of the acid could have frozen; whereas the other was cooled 15 degrees below its freezing point, and was exposed for an hour or two to an air not much less cold, in consequence of which a considerable part of the acid must have frozen; so that in all probability the acid, whose freezing point was found to be -30°, was in reality $\frac{1}{20}$ part stronger than that whose freezing point was -19°.

If this reasoning be just, the freezing point of these acids is as follows:

		Freezing point
Dephlogisticated spirit of nitre, whose strength =	,56	-30°
	,53	-19
	,437	$-4\frac{1}{2}$
Common spirit of nitre, whose strength =	,54	$-31\frac{1}{2}$
	,411	$-1\frac{1}{2}$

On the Phænomena observed on mixing Snow with these Acids.

13. On Dec. 13, snow was added to the spirit of nitre No. 168, as recommended in Art. 2. The snow was put in very gradually, and time was taken to find what effect each addition had on the thermometer and mixture, before more was added. The temperature of the acid before the mixture was -27°, and each addition of snow raised the thermometer a little, till it rose to $-1^\circ\frac{1}{4}$; after which the next addition made it sink to -2°, which shewed that sufficient snow had then been added. The quantity of snow used was pretty exactly $\frac{4}{10}$ of the weight of the acid, the weight of the acid being 13 oz. so that the strength of the diluted acid was reduced to ,411.

The acid before the addition of snow had no signs of freezing, its temperature being in all probability much above its freezing point; yet the snow did not appear to dissolve, but formed thin white cakes, which however did not float on the surface, but fell to the bottom, and when broke by the spatula formed a gritty sediment; so that it appears, that these cakes are not simply undissolved snow, but that the adjoining acid absorbed so much of the snow in contact with it, as to become diluted sufficiently to freeze with that degree of cold, and then congealed into these cakes. The quantity of congealed matter seems to have kept increasing till the end of the experiment.

14. On Dec. 21, an experiment was made in the same manner with the dephlogisticated spirit of nitre No. 27. The acid began to freeze in pouring it into the jar in which the mixture was to be made, and stood stationary there at -19°, as related in Art. 6; so that the liquor at the beginning of the experiment was white and thick, which made the effect of the addition of the snow less sensible. However, the congealed matter constantly subsided to the bottom, and the quantity seems to have continued increasing to the end of the experiment. The heat of the mixture rose to -4° before cold began to be produced, and the quantity of snow added was $\frac{22}{100}$ of that of the acid, so that the strength of the acid was reduced to ,437 by the dilution.

A very remarkable circumstance in this experiment is, that the acid, while the snow was adding, first became of a yellowish, and afterwards of a greenish or bluish hue. This colour did not go off by standing, but continued at least ten days, during which time the acid constantly kept that colour, except when by hasty freezing it shot into small filaments, in which case it put on the white appearance which these acids always assumed under those circumstances; but once that by gradual freezing it shot into transparent ice, this ice was of a bluish colour.

It is difficult to conceive what this colour should proceed from. Spirit of nitre is well known to assume this colour when much phlogisticated and properly diluted; but one does not see why it should become phlogisticated by the addition of the snow, and still less why the dephlogisticated acid should become more phlogisticated thereby than the common acid did; for though it is not extraordinary, that a process not capable of producing any increase of phlogistication in the common acid, should make this as much phlogisticated as that, yet it is very extraordinary that it should make it more so. No notice is taken of any effervescence or discharge of air while it was assuming this colour, nor was it observed that it became more smoking thereby, or that the top of the phial in which it was kept became full of red fumes, as might naturally be expected if it was rendered much phlogisticated. These are circumstances which, considering Mr. M^{c}Nab's great attention to set down all the phænomena that occurred, I should think would hardly have been omitted if they had really happened.

15. It is remarkable, that in both these experiments the addition of snow produced heat, until it arrived pretty exactly at what was found to be the freezing point of the diluted acid; but that as soon as it arrived at that point, the addition of more snow began to produce cold. This can hardly be owing merely to accident, and to both acids having happened to be of that precise degree of heat before the experiment began, that their heat after dilution should coincide with the freezing point answering to their new strength. The true cause seems to be as follows. It will be shewn in Art. 16 and 17, that the freezing point of these acids, when diluted as in the foregoing experiments, is much less cold than when they are considerably more diluted; and it was before shewn to be much less cold than when not diluted; so that there must be a certain degree of strength, not very different from that to which these acids were reduced by dilution, at which they freeze with a less degree of cold than when they are either stronger or weaker. Now in these experiments, the temperature of the liquors before dilution was below this point of easiest freezing, and a great deal of the acid was in a state of congelation all the time of dilution; the consequence of which is, that when they were diluted to the strength of easiest freezing, they would also be at the heat of easiest freezing; for they could not be below that point, because, if they were, so much of the acid would immediately freeze as would raise them up to it; and they could not

be above it, for, if they were, so much of the congealed acid would dissolve as would sink them down to it. After they were arrived at this strength of easiest freezing, the addition of more snow would produce cold, unless this strength be greater than that at which the addition of a small quantity of snow begins to produce cold; but even were this the case, heat would not be produced, but the temperature of the acids would remain stationary until they were so much diluted that the addition of more snow should produce cold. So that, in either case, the heat of the acids, at the time that the addition of fresh snow began to produce cold, must be that of easiest freezing; and consequently, as this heat was found to coincide very nearly with the freezing point of these acids, after dilution, it follows that their strengths at that time could differ very little from the strength of easiest freezing.

If the temperature of the liquors at the beginning of the experiment had been above the point of easiest freezing, none of the acid would have congealed during the dilution, and nothing could have been learnt from the experiment relating to the point of easiest freezing; but the heat would have kept increasing, till the acid was diluted to that degree of strength at which the cold produced by the dissolving of the snow was just equal to the heat produced by the union of the melted snow with the acid[1]; after which the addition of more snow would begin to produce cold. When I recommended this method of finding the best strength of spirit of nitre for producing cold, by the addition of snow, I was not aware of any impediment from the freezing of the acid, in which case it would have been a very proper method; but on account of this circumstance it can hardly be considered as such, except when the cold of the acid at setting out is less than that of easiest freezing.

In the dephlogisticated spirit of nitre the freezing points answering to the strength of ,434, ,53, and ,56, were said to be $-4^\circ\frac{1}{2}$, -19°, and -30°; and the differences of -30° and -19° from $-4^\circ\frac{1}{2}$ are to each other very nearly in the duplicate ratio of ,126 and ,096, the differences of the corresponding strengths from ,434; which, as ,434 is the strength of easiest freezing, is the proportion that might naturally be expected, and consequently serves in some measure to confirm the reasoning in this and the 12th Article.

16. After Mr. M^c^Nab had diluted these acids as above-mentioned, he divided each of them into two parts, and tried what degree of cold could be produced by mixing them with snow. On January 15th, one of these parts of the common spirit of nitre was tried. It was fluid when the experiment began, though its temperature, as well as that of the snow, was $-21^\circ\frac{1}{2}$; but on adding snow it immediately began to freeze, and grew

[1] In the experiment related in my observations on Mr. Hutchins's Experiments, this strength was rather greater than that of easiest freezing; but whether it is so in degrees of cold exceeding that in which my experiment was tried, does not appear.

thick, and its heat increased to $-2°\frac{1}{2}$; but by the addition of more snow it quickly sunk again, and at last got to $-43°\frac{1}{4}$. During the addition of the snow, the mixture grew thinner, and by the time it arrived at nearly the greatest degree of cold, consisted visibly of three parts: the lowest part, which consisted of frozen acid, was white and felt gritty; the upper part, which occupied about an equal space, was also white, but felt soft, and must have consisted of unmelted snow; the other part, which occupied by much the smallest space, was clear and fluid. The quantity of snow added was about $\frac{9}{13}$ of the weight of the acid, and consequently its strength was reduced to ,243.

Though snow was added to the acid in this experiment as long as, and even longer than, it produced any increase of cold, yet some days after, on adding more snow to the mixture, while it was fluid, and of the temperature of $-40°\frac{3}{4}$, the cold was increased to $-44°\frac{1}{4}$, or 1 degree lower than before. Mr. M^c^Nab did not perceive the snow to melt, though in all probability some must have done so, or no cold would have been produced.

The cause of this seems to be, that in the preceding experiment the congealed part of the acid was stronger than the fluid part; so that, though the fluid part was not strong enough to dissolve snow in a cold greater than $-43°\frac{1}{4}$, yet the whole acid together was strong enough to do it in a cold one degree greater.

A circumstance occurred in the last experiment which I cannot at all see the reason of; namely, a small part of the acid being poured into a saucer, before the addition of the snow, it was in an hour's time changed into solid ice, though the cold of the air, at the time the acid was poured out, was only $-41°\frac{1}{4}$, and does not seem to have increased during the experiment.

17. On December 30, the other half of the same acid had been tried in the same manner; at the beginning of the experiment not more than one-ninth part of the acid was fluid, the rest solid clear ice; its temperature was $-34°\frac{1}{2}$, and that of the snow nearly the same; the greatest degree of cold produced was $-42°\frac{3}{4}$; and the quantity of snow employed was about one-eighteenth of the weight of the acid; so that the strength of the mixture was ,38. The freezing point of the acid thus diluted appears to be about $-45°\frac{1}{4}$; for by the increase of warmth during the day-time, most of the congealed matter dissolved; but in the evening it began to freeze again, so as to become thicker, its temperature being then $-45°\frac{1}{4}$; and the next morning it was frozen solid, its cold being one degree greater.

18. On December 12, the diluted spirit of nitre No. 139, whose strength was ,175, was found frozen, its temperature being $-17°$. The fluid part, which was full of thin flakes of clear ice, and was of the consistence of syrup, was decanted into another bottle, and sent back. Its strength was ,21, and was greater than that of the undecanted part in the proportion of ,21 to ,16; so that, as not much of the undecanted part was really con-

gealed, the frozen part of the acid must have been much weaker than the rest, if not mere water. Accordingly, during the melting of the undecanted part, the frozen particles swam at top. Mr. M^c^Nab added snow to a little of the decanted liquor, but it did not dissolve, and no increase of cold was produced.

19. From these experiments it appears, that spirit of nitre is subject to two kinds of congelation, which we may call the aqueous and spirituous; as in the first it is chiefly, if not intirely, the watery part which freezes, and in the latter the spirit itself. Accordingly, when the spirit is cooled to the point of aqueous congelation, it has no tendency to dissolve snow and produce cold thereby, but on the contrary is disposed to part with its own water; whereas its tendency to dissolve snow and produce cold, is by no means destroyed by being cooled to the point of spirituous congelation, or even by being actually congealed. When the acid is excessively dilute, the point of aqueous congelation must necessarily be very little below that of freezing water; when the strength is ,21, it is at $-17°$, and at the strength of ,243, it seems, from Art. 16 to be at $-44°\frac{1}{4}$. Spirit of nitre, of the foregoing degrees of strength, is liable only to the aqueous congelation, and it is only in greater strengths that the spirituous congelation can take place. This seems to be performed with the least degree of cold, when the strength is ,411, in which case the freezing point is at $-1°\frac{1}{2}$. When the acid is either stronger or weaker, it requires a greater degree of cold; and in both cases the frozen part seems to approach nearer to the strength of ,411 than the unfrozen part; it certainly does so, when the strength is greater than ,411, and there is little doubt but what it does so in the other case. At the strength of ,54 the point of spirituous congelation is $-31°\frac{1}{2}$, and at ,33 probably $-45°\frac{1}{4}$; at least one kind of congelation takes place at that point, and there is little doubt but that it is of the spirituous kind. In order to present this matter more at one view, I have added the following table of the freezing point of common spirit of nitre answering to different strengths.

Strength	Freezing point °	
,54	$-31\frac{1}{2}$	spirituous congelation
,411	$-1\frac{1}{2}$*	
,38	$-45\frac{1}{4}$	
,243	$-44\frac{1}{4}$	aqueous congelation
,21	-17	

* The point of easiest freezing.

20. In trying the first half of the dephlogisticated spirit of nitre, the cold produced was $-44°\frac{1}{2}$. The acid was fluid before the addition of the snow, and of the temperature of $-30°$, but froze on putting in the thermometer, and rose to $-5°$, as related in Art. 7.

In trying the second part, the acid was about 0° before the addition of the snow, and therefore had no disposition to freeze. The cold produced was $-42°\frac{1}{2}$.

As the quantity of snow added in these experiments was not observed, they do not determine any points of aqueous or spirituous congelation in this acid; but there is reason to think, that these points are nearly the same as those of common spirit of nitre of the same strength, as the cold produced in these experiments was nearly the same as that obtained by the common spirit of nitre.

On the Vitriolic Acid.

21. On December 12, the strong oil of vitriol, No. 151, was found frozen, and was nearly of the colour and consistence of hogs-lard. Its temperature, found by pressing the ball of a thermometer into it, was $-15°$, and that of the air nearly the same; but in the night it had been exposed to a cold of $-33°$. It dissolved but slowly on being brought into a warm room, and was not completely melted before it had risen to $+20°$, and even then was not very fluid, but of a syrupy consistence. During the progress of the melting, the congealed part sunk to the bottom, as in spirit of nitre: and many air bubbles separated from the acid, which, when it was completely melted, formed a little froth on the surface. As soon as it was sufficiently melted to admit of it, which was not till it had risen to the temperature of $+10°$, the fluid part was decanted, and both were sent home to be examined.

It is remarkable, that the frozen part did not intirely dissolve until the temperature was so much increased. This would incline one to think, that the frozen part must have differed in some respect from the rest, so as to require much less cold to make it freeze; but yet I could not find that the strength of the decanted part differed sensibly from the rest.

It appeared by another bottle of oil of vitriol, which also froze by the natural cold of the air, that this acid, as well as the nitrous, contracts in freezing.

22. On December 21, when the weather was at $-30°$, the vitriolic acid No. 103. was diluted with snow, as directed in Art. 3. The snow dissolved immediately, and no signs of congelation appeared during any part of the process. The temperature of the acid rose only one degree before it began to sink, and the weight of the snow added was only $\frac{10}{122}$ of that of the acid, so that its strength was reduced thereby to ,605; which is therefore the best degree of strength for producing cold by the addition of snow, when the degree of cold set out with is $-30°$. This strength is one-fifteenth part less than what I found myself, by a similar experiment, when the temperature of the acid was $+27°$; which shews, that the best degree of strength is rather less, when the degree of cold set out with is great than when small, but that it does not differ much.

23. The acid thus diluted was divided into two parts, and the next day Mr. M^c^Nab tried what degree of cold could be produced by adding snow to one of them. The temperature of the air at the time was $-39°$, and the mixture sunk by the process to $-55°\frac{1}{2}$. The snow dissolved readily, and the mixture did not lose much of its fluidity until it had acquired nearly its greatest degree of cold, nor did any congealed matter sink to the bottom in any part of the process. The quantity of snow added was about $\frac{86}{100}$ of the weight of the acid, so that the strength of the mixture was about ,325.

24. On January 1, thin crystals of ice were found diffused all through this mixture, the temperature of the air being $-51°\frac{1}{2}$, but that of the liquor was not tried. As this congelation must have been of the aqueous kind, and seems to have taken place at the temperature of $-51°\frac{1}{2}$, it should follow, that this acid had no power of dissolving snow in a cold of $-51°\frac{1}{2}$; so that it does not at first appear why a cold four degrees greater than that should have been produced in the foregoing experiment. The reason is, that at the time the mixture arrived at $-55°\frac{1}{2}$, it appeared by the diminution of its fluidity to have contained some undissolved snow, and some more was added to it after that time, which before the first of January dissolved and mixed with the acid; so that the acid in the mixture, at the time it sunk to $-55°\frac{1}{2}$, was not quite so much diluted as that which froze on January 1. This is the reverse of what happened in the trial of the nitrous acid in Art. 15. as in that experiment the fluid part, at the time of the greatest cold, was weaker than the whole mixture together; but it must be considered, that *that* mixture contained much congealed acid, as well as undissolved snow, whereas *this* contained only the latter.

25. On January 1, snow was added to the other half of the acid diluted on December 21. The cold produced was much greater than before, namely $-68°\frac{1}{2}$; this seems to have proceeded, partly from the air and materials having been 12 degrees colder in this than in the former experiment, and partly from the snow having been added faster, so that the mixture arrived at its greatest degree of cold in 20′, whereas it before took up 46′. Another reason is, that the former mixture was made in too small a jar, in consequence of which it was poured into a larger before the experiment was completed, whereby some cold was lost. The quantity of snow used in this experiment was less than in the former, so that the strength of the acid after the experiment was about ,343. The mixture also grew much thicker, and had a degree of elasticity resembling jelly; but whether this was owing only to more snow remaining undissolved, or to any other cause, I cannot tell.

26. Great as the foregoing degree of cold is, Mr. M^c^Nab, on February 2, produced one much greater. In hopes of obtaining a greater degree of cold by previously cooling the materials, he cooled about seven ounces of oil of vitriol, whose strength was ,629, that is, rather stronger than the fore-

going, by placing the jar in which it was contained in a freezing mixture of oil of vitriol and snow; the snow intended to be used was also cooled by placing it under the vessel in which the freezing mixture was made. As soon as the acid in the jar was cooled to the temperature of $-57°\frac{1}{2}$, a little of the snow was added, on which it immediately began to freeze, and rose to $-36°$; but in about 40 minutes, as the jar was still kept in the freezing mixture, it sunk to $-48°$; by which time it was grown very thick and gritty, especially at bottom. More of the cooled snow was then added, which in a short time made it sink to $-78°\frac{1}{2}$, and at the same time the thickness and tenacity of the mixture diminished; so that by the time it arrived at the greatest degree of cold, very little thickness remained.

It is worth inquiring, what was the reason of the greater degree of cold produced in this than in the preceding experiment? It could not be owing to the materials being colder; for at the time of the second addition of snow, at which time the experiment may be considered to have begun, the acid was not colder than at the beginning of the preceding experiment, and the snow in all probability not much colder. It could not be owing neither to the jar having been kept in the freezing mixture: for though that mixture was three or four degrees colder than the air in the preceding experiment, yet the acid in the jar, before it acquired much addition of cold, would be robbed of its cold faster by the mixture than it would by air of the same temperature as that in the preceding experiment. Neither could it proceed from any difference in the strength of the acid; for what difference there was must have done more hurt than good. The true reason is, that the acid was in a state of congelation: for as the congealed acid united to the snow and became fluid by the union, it is plain, that cold must have been produced both by the melting of the snow and by that of the acid; whereas, if the acid had been in a fluid state, cold would have been produced only by the first cause, and consequently a greater degree of cold should be produced in this experiment than in the former. The only inconvenience attending the acid being in a state of congelation is, that in all probability it does not unite to the snow so readily as when in a fluid state; but the difference seems not material, as the cold was produced, and the materials melted, in 5 minutes.

27. The day before, Mr. M^c^Nab, by adding snow to some of the same acid in the usual manner, when the cold of the materials was $-46°$, produced a cold of only $-66°$.

28. In these four last experiments the acid was reduced, by the addition of the snow, to the strengths of ,325, ,343, ,403, and ,334; and the cold produced in them was before said to be $-55°\frac{1}{2}$, $-68°\frac{1}{2}$, $-78°\frac{1}{2}$, and $-66°$; whence we may conclude, that these are nearly the points of aqueous congelation answering to the foregoing strengths; only it appears, from what was said in Art. 24. that the strengths here set down are all of them rather too small.

Though it is certain that oil of vitriol is capable of the spirituous congelation, and though it appears, both from the foregoing experiments and from some made by the Duc d'Ayen[1] and by M. de Morveau[2], that it freezes with a less degree of cold when strong than when much diluted, it is not certain whether it has any point of easiest freezing, like spirit of nitre, or whether the cold required to freeze it does not continually diminish as the strength increases, without limitation; but the latter opinion is the most probable. For the Duc d'Ayen's and M. de Morveau's acids, which, as they were concentrated on purpose, were most likely stronger than Mr. McNab's, froze with a cold less than zero of Fahrenheit; whereas the freezing point of Mr. McNab's undiluted acid, whose strength was ,98, was $-15°$, and that of the diluted acid, whose strength was ,629, was $-36°$; and when the acid was more diluted, it was found to bear a much greater cold without freezing. It appears also, both from Art. 21. and from M. de Morveau's experiment, that during the congelation of the oil of vitriol, some separation of its parts takes place, so that the congealed part differs in some respect from the rest, in consequence of which it freezes with a less degree of cold; and as there is reason to think from Art. 21. that these two parts do not differ much in strength, it seems as if the difference between them depended on some less obvious quality, and probably on that, whatever it is, which forms the difference between glacial and common oil of vitriol. The oil of vitriol prepared from green vitriol, has sometimes been obtained in such a state as to remain constantly congealed, except when exposed to a heat considerably greater than that of the atmosphere, whence it acquired its name of *glacial*[3]. It is not known indeed upon what this property depends, but it is certainly something else than its strength; for oil of vitriol of this kind is always smoking, and the fumes it emits are particularly oppressive and suffocating, though very different from those of the volatile sulphureous acid. On rectification likewise it yields, with the gentlest heat, a peculiar concrete substance, in the form of saline crystals; and after this volatile part has been driven off, the remainder is no longer smoking, and has lost its glacial quality[4].

On the Mixture of Oil of Vitriol and Spirit of Nitre.

29. This mixture is not so fit for producing cold by the addition of snow, as oil of vitriol alone; for the cold obtained did not exceed $-54°\frac{1}{2}$, in either of the experiments tried with it. The point of spirituous congelation of this mixture, when diluted with somewhat more than one-tenth of

[1] *Diction. de Chym.* par Macquer, 2de édit.

[2] *Nouv. Mém. de l'Academ. de Dijon*, 1782, 1er semestre, p. 68.

[3] *Mém. de l'Academ. des Sc.* 1738, p. 288.

[4] Crell's *Neu. Entdeck. in der Chemie*, Th. 11, p. 100, Th. 12, p. 241, etc., and *Annalen*, 1785, St. 5, p. 438, etc.

its weight of water, is about -- 20°, and is much lower when the acid is considerably more diluted; but as the Society will most likely have less curiosity about the disposition to freeze of this mixture than of the simple acids, I shall spare the particulars.

On the Spirit of Wine.

30. The rectified spirits No. 8. were diluted with snow, in the same manner as the other liquors; but were found not to want any, as the first and only addition of snow produced cold. The quantity added was about $\frac{1}{20}$ of the weight of the spirit.

31. The spirit thus diluted was divided, like the other liquors, into two parts, and each tried separately. The first was at – 45°, before the addition of the snow, and was sunk by the process to – 56°. The snow, even at the first addition, did not dissolve well, so that the spirit immediately became full of white spots[1], and grew thick by the time it arrived at its greatest degree of cold. After standing some hours, the mixture rose to the temperature of – 39°, and was grown clear, but yet was not limpid, but of the consistence of syrup. No cold was produced by adding snow to it in that state, though it appeared that its point of aqueous congelation was at least 6 degrees lower than its temperature at that time[2]; which seems to shew that spirit of wine has scarce any power of dissolving snow when it wants even 6 degrees of its point of aqueous congelation, and therefore is another instance that snow is dissolved much less readily by spirit of wine than by the nitrous and vitriolic acids.

32. In trying the other part of the diluted spirits, the cold produced was only – 47°$\frac{1}{2}$, the cold set out with being – 37°.

33. It appeared by the diluted spirit of wine No. 143. which on December 12 froze by the natural cold of the atmosphere, and was treated in the same manner as the diluted spirit of nitre, that when highly rectified spirit of wine, such as No. 8. is diluted with 1$\frac{4}{10}$ its weight of water, its point of aqueous congelation will be at – 21°. The congealed part of the spirit was white like diluted milk, and even the decanted part, which was full of thin films of ice, had a milky hue. The fluid part was stronger than the rest, and no increase of cold was produced by adding snow to some of it, both of which are marks of aqueous congelation.

Though the foregoing experiments confirm the truth of what I said, in the account of Mr. Hutchins's experiments, concerning the cause of the

[1] This was not the case during the above-mentioned dilution of the spirits; but the cold was 16 degrees less in that experiment than in this.

[2] On account of the dilution which the spirits suffered by the melting of the snow which remained undissolved at the time of the greatest cold, its point of aqueous congelation was no longer so low as - 56°; but it still was not less than - 45°$\frac{1}{2}$, as in the evening it was found at that temperature, without much congealed matter in it.

cold produced by mixing snow with different liquors, and intirely clear up the difficulty relating to it which I mentioned in Art. 1., yet several questions may naturally occur; such as, why the cold produced by the oil of vitriol was so much greater than that obtained by the spirit of nitre, notwithstanding that in warmer climates the nitrous acid seems to produce more cold? and why the cold produced by the nitrous acid, notwithstanding its previous dilution, which might naturally be expected to be of service, was not greater than has been obtained by other persons without that precaution? But as this would lead me into disquisitions of considerable length, without my being able to say any thing very satisfactory on the subject, I shall forbear entering into it. I will only observe, that in most of the foregoing experiments, Mr. M^cNab would probably have produced more cold, if he had added the snow faster. We ought not, however, to regret that he did not, as its effects on the acids would then have been less sensible.

The natural cold, when these experiments were made, is remarkable; as there were at least nine mornings in which the cold was not less than that of freezing mercury; four in which it was at least eight degrees below that point, or $-47°$; and one in which it was $-50°$. Whereas out of nine winters, during which Mr. Hutchins observed the thermometer at Albany Fort, there were only twelve days in which the cold was equal to that of freezing mercury, and the greatest cold seems to have been $-45°$. I cannot learn whether the last winter was more severe than usual at Hudson's Bay; or whether Henley-House is a colder situation than Albany, which may perhaps be the case; for though it is only 130 miles distant from it, yet it stands inland, and to the W. or S.W. of it, which is the quarter from which the coldest winds blow.

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Mr. M^cNab's original account of the experiments which furnished the materials of this Paper, having been thought too long to be printed in detail, is deposited in the Archives of the Society.

PHILOSOPHICAL TRANSACTIONS. VOL. 78, 1788, p. 166

XIII. *An Account of Experiments made by Mr.* John M^cNab, *at* Albany Fort, Hudson's Bay, *relative to the Freezing of Nitrous and Vitriolic Acids. By* Henry Cavendish, *Esq., F.R.S. and A.S.*

Read February 28, 1788

FROM the experiments made by Mr. M^cNab, of which I gave an account in the LXXVIth Volume of the *Philosophical Transactions*, p. 241. it appeared, that spirit of nitre was subject, not only to what I call the aqueous congelation, namely, that in which it is chiefly, and perhaps intirely, the watery part which freezes, but also to another kind, in which the acid itself freezes, and which I call the spirituous congelation. When its strength is such as not to dissolve so much as $\frac{243}{1000}$ of its weight of marble, or when its strength is less than ,243, as I call it for shortness, it is liable to the aqueous congelation solely; and it is only in greater strengths that the spirituous congelation can take place. This seems to be performed with the least degree of cold when the strength is ,411, in which case the freezing point is at $-1°\frac{1}{2}$. When the acid is either stronger or weaker, it requires a greater degree of cold; and in both cases the frozen part seems to approach nearer to the strength of ,411 than the unfrozen part. The freezing points, answering to different degrees of strength, seemed to be as follows.

Strength	Freezing point °	
,54	$-31\frac{1}{2}$	spirituous congelation
,411	$-1\frac{1}{2}$	
,38	$-45\frac{1}{2}$	
,243	$-44\frac{1}{2}$	aqueous congelation
,21	-17	

As some of these properties, however, were deduced from reasoning not sufficiently easy to strike the generality of readers with much conviction, Mr. M^cNab was desired to try some more experiments to ascertain the truth of it; which he was so good as to undertake, and has executed them with the same care and accuracy as the former.

For this purpose, I sent him some bottles of spirit of nitre of different strengths, and he was desired to expose each of these liquors to the cold till they froze; then to try their temperature by a thermometer; afterwards to keep them in a warm room till the ice was almost melted, and then again expose them to the cold, and, when a considerable part of the acid had frozen, to try the temperature a second time; then to decant the unfrozen part into another bottle, and send both parts back to England, that their strength might be examined.

The intent of this second exposure to the cold was as follows. Spirit of nitre bears, like other liquors, to be cooled greatly below its freezing point without freezing: then the congelation begins suddenly; the liquor is filled with fine spicula of frozen matter, and the ice becomes so loose and porous, that, if the process be continued long enough for a considerable portion of the acid to congeal, scarce any of the fluid part can be decanted: whereas, if it be heated in this state till the frozen part is almost, but not intirely, melted, and be again exposed to the cold, as the liquor is then in contact with the congealed matter, it begins to freeze as soon as it arrives at the freezing point, and the ice becomes much more solid and compact.

The intent of decanting the fluid part, and sending both parts back, that their strength might be determined, was partly to examine the truth of the supposition laid down in my former Paper, that the strength of the frozen part approaches nearer to ,411 than that of the unfrozen; but it is also a necessary step towards determining the freezing point answering to a given strength of the acid; for as the frozen part is commonly of a different strength from the unfrozen, the strength of the fluid part, and the cold necessary to make it freeze, is continually altering during the progress of the congelation. In consequence of this, the temperature of the liquor is not that with which the frozen part congealed; but it is that necessary to make the remainder, or the fluid part, begin to freeze, or, in other words, it is the freezing point of the fluid part. This is the reason that a thermometer, placed in spirit of nitre, continually sinks during the progress of congelation; which is contrary to what is observed in pure water, and other fluids in which no separation of parts is produced by freezing.

Moreover, from the above-mentioned experiments of Mr. M^{c}Nab it appeared, that oil of vitriol, as well as spirit of nitre, is subject to the spirituous congelation; but it seemed uncertain, whether, like the latter, it had any point of easiest freezing, or whether it did not uniformly freeze with less cold as the strength increased. For this reason, some bottles of oil of vitriol, of different strengths, were sent, which he was desired to try in the same manner as the former. This point, indeed, has since been determined by Mr. Keir, who has shewn that oil of vitriol has a strength of easiest freezing; and that at that point a remarkably slight degree of cold is sufficient for its congelation.

The result of Mr. M^c^Nab's experiments on the nitrous acid is given in the following table.

No.	Decanted part Quantity	Decanted part Strength	Undecanted part Quantity	Undecanted part Strength	Strength of the whole mass	Strength before sent	Freezing point by first method °	Freezing point by second method °
6	—	—	—	—	—	,561	− 41,6	—
7	1410	,445	2137	,435	,439	,437	+ *1,7	− 3,8
8	1658	,390	1940	,422	,407	,408	− 3,5	− 4
9	1368	,353	2438	,416	,393	,391	− 4,5	− 11
10	2206	,343	1920	,373	,357	,357	− 12,5	− 13,8
11	3620	,310	602	,381	,320	,320	− 22,5	− 23
12	2155	,276	1494	,293	,283	,280	− 39,1	− 40,3
13	1618	,241	1961	,235	,238	,238	− 34	− 32

The first column contains the numbers by which Mr. M^c^Nab has distinguished the different bottles. The second and third columns contain the quantity and strength of the decanted part of the liquor; and the fourth and fifth shew the quantity and strength of the undecanted part of the liquor. The sixth column gives the strength of both parts put together, or the strength of the whole mass; and the seventh is the strength of the same acid, as it was determined before it was sent to Hudson's Bay. The strengths of the decanted and undecanted parts were found by saturating the liquor returned home with marble; and that of the whole mass was inferred by computation from the quantity and strength of the decanted and undecanted parts; and as the strength thus inferred never differs from that determined before the liquors were sent to Hudson's Bay by more than $\frac{1}{100}$ part of the whole, it is not likely that the strengths of the decanted and undecanted parts here set down should differ from the truth by much more than that quantity.

The eighth column contains the freezing points found in the first method, or the temperature of the liquors after the hasty congelation which took place on exposing them to the cold without any frozen matter in them; and the ninth contains their temperature after the more gradual congelation which took place when they were cooled with some frozen matter in them; and as the unfrozen part of the acid was decanted immediately after the temperature had been observed, it follows, that this column shews the true freezing points of the decanted liquors. In like manner the eighth column shews the freezing points of that part of the liquor which remained fluid in the first manner of trying the experiment; but as the strength of this part was not determined, the precise strengths to which these freezing points correspond are unknown. Thus much, however, is certain, that these points must be below those of the whole mass, and in all probability must be above those of the decanted liquor; as there is great reason to think, that the quantity of frozen matter was always less, and consequently the strength of the fluid part differed less from that of the whole mass, in the first way of trying the experiment than in the second.

Before I draw any conclusions from these experiments, it will be proper to take notice of some particularities which occurred in trying them.

No. 6 was made to congeal by a freezing mixture of snow and diluted oil of vitriol. By the time the acid was cooled to $-42°$, icy filaments were formed on the inside of the phial above the acid. Ten minutes after, the acid being cooled one degree more, the phial was taken out and agitated. This mixed the icy filaments with the acid, and made it freeze, which it seems not to have done before, in consequence of which its temperature rose to $-41°\frac{1}{2}$. After having melted the greatest part of these filaments, and again exposed it to the freezing mixture, some snow accidentally fell into the acid, and made an uncertainty in the freezing point, for which reason it is not set down. But as it is evident, that the quantity of congealed matter in the first experiment was excessively small, the strength of the unfrozen part could not differ sensibly from that of the whole mass, and therefore $-41°\frac{1}{2}$ is the true freezing point that answers to the strength of ,561.

It is remarkable, that No. 8 acquired by congelation a bluish colour, not unlike that which the dephlogisticated nitrous acid, in Mr. M^cNab's former experiments, acquired by dilution with snow. It is not said, how long the acid retained this colour, but it was intirely gone when the phial arrived in England. I am quite at a loss to account for this phænomenon, and why it happened to this bottle only.

No. 12 when cooled to $-17°$ seemed to contain many icy particles; but as it afterwards bore to be cooled to $-48°$, without their increasing, we may conclude, that they were not frozen spirit of nitre, but only some heterogeneous matter separated from it. A little of the congealed part of No. 8 dropped into it while at this point, made it freeze, and it rose to $-39°$.

In all the foregoing acids the ice was heavier than the fluid part, and in consequence subsided to the bottom; a proof that it was the spirituous congelation which had taken place in them: but in No. 13 the frozen part swam at top, which shews, that the congelation was of the aqueous kind.

It may appear remarkable to those who read Mr. McNab's experiments, that these acids bore to be heated so much above their freezing points before the ice intirely dissolved. No. 6. bore to be heated 18 degrees, No. 7. 13 degrees, and No. 12. 17 degrees above their freezing points, before all the congealed acid had disappeared. But as, in order to dissolve this congealed matter, they were brought into a room in all probability a great many degrees warmer than the points to which they were heated, so that the liquors heated fast; and as during the dissolution the ice would subside to the bottom; it is not extraordinary, that the fluid part in the phial might be many degrees warmer than the frozen part, unless the phials were much agitated during the time, which nothing shews them to have been; especially if we consider the great quantity of heat which, in all probability, must be communicated to the frozen acid in order to melt it; and that, perhaps, the frozen acid may receive and part with its heat but slowly. It must be observed that in No. 6. and 12. the frozen part might very likely be of a considerably different strength, and in consequence its freezing point might be several degrees different from that of the whole mass, so that the temperature to which the fluid was heated, in order to melt the ice, might very likely not differ so much from the freezing point of the ice itself as is here set down. But this could not be the case with No. 7.

It must be observed, that when Mr. McNab wanted to try the temperature of No. 7. after it had frozen in the first manner, the stopper stuck so tight that he was not able to remove it without warming it before the fire. The thermometer was then introduced, and stood several minutes therein at + 1°½, or + 2°. As the thermometer remained so long at this point, one might naturally suppose, that this was the true freezing point of the unfrozen acid. But yet, from what has been just said, it seems not improbable that it may be otherwise, and that the true freezing point may be sensibly lower; for which reason it is marked in the table with an asterisk (*) as doubtful.

It was before said, that the temperatures in the ninth column of the foregoing table, are the freezing points answering to the strengths expressed in the third column, and that − 41°½ is the freezing point answering to the strength of ,561; whence the freezing points determined by these experiments, and their respective strengths, are as follows:

Strength	Freezing point
	°
,561	− 41,6
,445	− 3,8
,390	− 4
,353	− 11
,343	− 13,8
,310	− 23
,276	− 40,3

By interpolation from these *data*, according to Newton's method[1], it appears, that the strength at which the acid freezes with the least cold is ,418, and that the freezing point answering to that strength is − 2°$\frac{4}{10}$.

In order to shew more readily the freezing point answering to any given strength, I have computed, by the same method, the following table, in which the strengths increase in arithmetical progression.

It was before shewn, that the freezing points, found by the first method, ought to be below those of the whole mass, and must, in all probability, be

[1] *Princip. Math.* Lib. III. prop. 40, lem. 5.

above those of the decanted liquor. In order to see how this agrees with observation, I computed in the above-mentioned manner the freezing

Strength	Freezing point °	Difference °
,568	− 45,5	
		+ 15,4
,538	− 30,1	
		+ 12
,508	− 18,1	
		+ 8,7
,478	− 9,4	
		+ 5,3
,448	− 4,1	
		+ 1,7
,418	− 2,4	
		− 1,8
,388	− 4,2	
		− 5,5
,358	− 9,7	
		− 8
,328	− 17,7	
		− 10
,298	− 27,7	

points answering to the strength of the whole mass, and compared them with the observed freezing points. The result is given in the following table.

No.	Strength of the whole mass	Strength of the decanted liquor	Computed freezing point of the whole mass °	Observed freezing point: In first method °	Observed freezing point: In second method °
7	,439	,445	− 3,2	+ 1,7	− 3,8
8	,407	,390	− 2,6	− 3,5	− 4,
9	,393	,353	− 3,7	− 4,5	− 11,
10	,357	,343	− 10,	− 12,5	− 13,8
11	,320	,310	− 19,9	− 22,5	− 23,
12	,283	,276	− 35,6	− 39,1	− 40,3

It may be observed, that the freezing point of No. 7. tried in the first way, is considerably above that corresponding to the strength of the whole mass; but as this experiment was shewn [p. 218] to be doubtful, and not unlikely to exceed the truth, we may safely reject it as erroneous. All the others, as might be expected, are lower than those corresponding to the strength of the whole mass, and above those observed in the second manner, and therefore serve to confirm the truth of the above determination of the freezing points of spirit of nitre; and also shew, that in this acid the point of spirituous congelation is pretty regular, and does not depend much, if at all, on the rapidity with which the congelation is performed.

The point of aqueous congelation, however, seems liable to considerable irregularity; for No. 13, after having been exposed to the cold, froze on agitation, the congelation, as was before said, being of the aqueous kind, and the thermometer stood stationary therein at $-34°$. The ice being then almost melted, it was again exposed to the cold, till a good deal was frozen; but yet its temperature was then no lower than $-32°\frac{1}{4}$, though the quantity of frozen matter must certainly have been much more than in the first trial. The fluid part being then decanted, and the frozen part melted, both were again exposed to the cold. They both were made to congeal by agitation, and the temperature of the undecanted was then found to be $-35°$, and that of the decanted part $-37°$: so that it should seem as if the freezing point found by the hasty congelation was always lower than that found the other way, which may, perhaps, proceed from this cause; namely, that when sufficient time is allowed, the watery part will separate from the rest, and freeze in a degree of cold much less than what is required to produce that effect, when it is performed in a more rapid manner.

These experiments confirm the truth of the conclusions I drew from Mr. McNab's former experiments; for, first, there is a certain degree of strength at which spirit of nitre freezes with a less degree of cold than when it is either stronger or weaker; and when spirit of nitre, of a different strength from that, is made to congeal, the frozen part approaches nearer to the foregoing degree of strength than the unfrozen. Likewise this strength, as well as the freezing point corresponding thereto, and the freezing point answering to the strength of ,54, come out very nearly the same as I concluded from those experiments; for by the present experiments they come out ,418, $-2°\frac{4}{10}$ and $-31°$, and by the former, ,411, $-1°\frac{1}{2}$, and $-31°$. But the freezing point answering to the strength of ,38 is totally different from what I there supposed. This must have been owing to the strength of that acid having been very different from what I thought it; which is not improbable, as its strength was inferred only from the quantity of snow which was added to it in finding the degree of cold produced by its mixture with snow.

After the foregoing experiments were finished, Mr. McNab made some more for determining the freezing points both of the decanted and undecanted part; but for want of a sufficient explanation of the manner in which they were executed, I have not been able to make any use of them. In their present state they shew much appearance of irregularity; but this would very likely have been cleared up, if the circumstances had been more fully detailed.

On the Vitriolic Acid.

An irregularity of a remarkable kind occurred in trying two of these acids; namely, when the undecanted part was melted and again made to congeal, its freezing point was found to be much less cold than that of the decanted part, and the difference was much greater than could be attributed to the difference of strength. This seems to have happened only in the two strongest acids, namely, No. 1. and 2. and in great measure confirms the supposition which I formed from Mr. McNab's former experiments, that the congealed part of oil of vitriol differs from the rest, not merely in strength, but also in some other respect, which I am not acquainted with. It should seem, however, that this property does not extend to weak oil of vitriol.

It perhaps may be suspected, that this property takes place in the nitrous acid also, and was the cause of the slow melting of the ice taken notice of in [p. 217]. But I think it more likely, that that phænomenon proceeded from the causes there assigned.

Some smaller irregularities occurred in trying the vitriolic acid, the cause of which I believe was, that when this acid has been cooled below the freezing point, and begins to freeze, the congelation proceeds but slowly; so that a considerable time elapses before it rises to the true freezing point. Something of the same kind seems to take place in the nitrous acid also, though in a less degree; for the decanted liquors usually continued to freeze and deposit a small quantity of ice, for a few minutes after they were poured off, though their cold, at least in some instances, was found rather to diminish during that time. It must be observed, that small spicula of ice always came over along with the decanted liquor; and to this, in all probability, the new-formed ice attached itself; for otherwise it is likely, that no ice would have been produced.

The following table contains the strength of the acids as determined before they were sent to Hudson's Bay, and the quantity and strength of the decanted and undecanted parts when they arrived at London, and the strength of the whole mass as computed from thence. For the sake of uniformity, I have expressed their strengths, like those of the nitrous acid, by the quantity of marble necessary to saturate them, though I did not find their strength by actually trying how much marble they would dissolve; as that method is too uncertain, on account of the selenite formed in the operation, and which in good measure defends the marble from the action of the acid. The method I used was, to find the weight of the plumbum vitriolatum formed by the addition of sugar of lead, and from thence to compute the strength, on the supposition that a quantity of oil of vitriol, sufficient to produce 100 parts of plumbum vitriolatum, will dissolve 33 of marble; as I found by experiment that so much oil of vitriol would saturate as much fixed alkali as a quantity of nitrous acid sufficient to dissolve 33 of marble. It may be observed, that the quantity of alkali, necessary to saturate a given quantity of acid, can hardly be determined with much accuracy, for which reason the foregoing less direct method was adopted; especially as the precipitation of plumbum vitriolatum shews the proportional strengths, which is the thing principally wanted, with as great accuracy as any method I know.

No.	Strength before sent	Decanted part		Undecanted part		Strength of whole mass
		Quantity	Strength	Quantity	Strength	
1	,977	1375	,967	3460	,963	,964
2	,918	3915	,919	1876	,905	,914
3	,846	88	,777	4915	,850	,849
4	,758	389	,710	3795	,753	,755
				547	,803	

The undecanted part of No. 4 was divided into two parts; namely, the less and the more congealable part; and it is the latter whose quantity and strength is given in the last line.

It is well known, that oil of vitriol attracts moisture with great avidity; and some of these acids were much exposed to the air during the experiments made with them, and may therefore be supposed to have attracted so much moisture from the air, as might sensibly diminish their strength; and this seems actually to have been the case with some of them. But as the bottles were well stopped, and as, except in one acid which was the most exposed to the air, the strength of the whole mass comes out not much less than that determined before the liquors were sent to Hudson's Bay, I imagine their strength could not sensibly alter during their voyage home; and consequently their strength, at the time the last observations were made with them, could not differ much from that here set down.

It would be tedious to give the experiments for determining their freezing points in detail; but the result is as follows. The freezing point of No. 1 tried in the first method, was somewhat above $+ 1°$, but it is uncertain how much; that tried in the second manner seemed $- 6°\frac{1}{2}$. But the freezing point of the undecanted part, after having been intirely melted, and again exposed to the cold, was $+ 9°$. It must be observed, that though this part was in all probability at first stronger than the decanted part, yet at the time its freezing point was tried, it seems to have become rather weaker than that, owing to its exposure to the air. It was before said, that the freezing point tried in the second manner is that of the decanted liquor; so that the freezing point of the decanted part seems to have been 13 or 14 degrees colder than that of the undecanted part; though the difference of strength, if there was any, must in all probability have tended to produce the contrary effect.

The freezing point of No. 2. tried in the first way, was $- 26°$; and that tried in the second was $- 30°$, or $- 26°$; but yet the freezing point of the undecanted part was 26 or 30 degrees higher, namely, at zero; a difference which could scarcely have proceeded from the difference of strength.

The freezing point of No. 3 could hardly differ much from $+ 42°$; and that of No. 4 was about $- 45°$.

It should be remarked, that when this last acid, as well as No. 1. and 2. were exposed to a great cold, a sediment formed in them. This must have been of a very different nature from frozen acid, as appeared both from its texture, which was soft and mucilaginous to the feel, instead of being gritty as the frozen acid always was; and also from its being not much increased by an increase of cold; and therefore seems to have been some impurity separated from the acid. The quantity was greatest in No. 4.; but even in this, though it appeared great, it is likely that the real quantity was very small.

Another bottle of acid, whose strength was ,659, was sent; but Mr. McNab was not able to make this freeze.

From these experiments it should seem, that the freezing point of oil of vitriol, answering to different strengths, is nearly as follows:

Strength	Freezing point °
,977	+ 1
,918	− 26
,846	+ 42
,758	− 45

From hence we may cônclude, that oil of vitriol has not only a strength of easiest freezing, as Mr. Keir has shewn; but that, at a strength superior to this, it has another point of contrary flexure, beyond which, if the strength be increased, the cold necessary to freeze it again begins to diminish.

The strength answering to this latter point of contrary flexure must, in all probability, be rather more than ,918, as the decanted or unfrozen part of No. 2. seemed rather stronger than the undecanted part; and for a like reason the strength of easiest freezing is rather more than ,846.

Mr. Keir found that oil of vitriol froze, with the least degree of cold, when its specific gravity at 60° of heat was 1,780, and that the freezing point answering to that degree of strength was + 46°; which agrees pretty nearly with these experiments, as the strength of oil of vitriol of that specific gravity is ,848, that is, nearly the same as that of No. 3.

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XVII. *On the Conversion of a Mixture of dephlogisticated and phlogisticated Air into nitrous Acid, by the electric Spark. By* Henry Cavendish, *Esq., F.R.S. and A.S.*

Read April 17, 1788

IN Volume LXXV. of the *Philosophical Transactions*, p. 372. I related an experiment, which shewed, that by passing repeated electric sparks through a mixture of atmospheric and dephlogisticated air, confined in a bent glass tube by columns of soap-lees and quicksilver, the air was converted into nitrous acid, which united to the soap-lees and formed nitre. But as this experiment has since been tried by some persons of distinguished ability in such pursuits without success, I thought it right to take some measures to authenticate the truth of it. For this purpose, I requested Mr. Gilpin, Clerk of the Royal Society, to repeat the experiment, and desired some of the Gentlemen most conversant with these subjects to be present at putting the materials together, and at the examination of the produce.

This laborious experiment Mr. Gilpin was so good as to undertake. It was performed in the same manner, and with the same apparatus, which was used in my own experiments, and which is described in the beginning of the above-mentioned Paper, and is accompanied with a drawing. The method used for introducing air into the bent tube, was that described in the last paragraph of p. [194] in that Paper, by means of the apparatus represented in fig. 3. or the reservoir, as I shall call it. The soap-lees, like those of my own experiments, were prepared from salt of tartar, and were of such strength as to yield $\frac{1}{10}$ of their weight of nitre when saturated with nitrous acid. The dephlogisticated air was prepared from turbith mineral, and seemed by the nitrous test to contain about $\frac{1}{18}$ part of phlogisticated air.

On December 6, 1787, in the presence of Sir Joseph Banks, Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. J. Hunter, and Mr. Macie, the materials were put together. The quantity of soap-lees, introduced into the bent tube, was 180 measures, each of which contained one grain of quicksilver; and, as the bore of the tube was rather more than one-third of an inch in diameter, it formed a column of five or six-tenths of an inch in length, which, by the introduction of the air, was divided into two parts, one resting on the quicksilver in one leg of the tube, and the other on that in the other leg. The dephlogisticated air was mixed with one-third part of its bulk of atmospheric air of the room in a separate jar, and the reservoir was filled with the mixture; and from thence Mr. Gilpin, as occasion required, forced air into the bent tube, to supply the place of that absorbed by means of the electric spark.

From what has been said, it appears, that the mixture employed contained a less proportion of common air than that used in either of my experiments. This made it necessary for Mr. Gilpin now and then to introduce some common air by means of the bent tube represented in fig. 3. of the above-mentioned paper, whenever from the slowness of the absorption he thought there was too small a proportion of phlogisticated air in the tube.

My reason for this manner of proceeding was, that as my first experiment seemed to shew, that the dephlogisticated air ought to be in a rather greater proportion to the phlogisticated than the latter did, I was somewhat uncertain as to the proper quantities, and doubted whether I could proportion them in such manner as that it should not be necessary, during the course of the experiment, to add either dephlogisticated or common air. I therefore mixed the airs in such proportion, that I was sure there could be no occasion to add the former; since it was much easier, as well as more unexceptionable, to add common air than dephlogisticated air.

On December 24, as the air in the reservoir was almost all used, this apparatus was again filled in the presence of most of the above-mentioned Gentlemen, with a mixture of the same dephlogisticated air and common air, in the same proportions as before; and the same thing was repeated on January 19.

On January 23, the bent tube was, by accident, raised out of one of the glasses of mercury into which it was inverted, by which it was filled with air, and a good deal of the soap-lees were lost; there, however, was enough remaining for examination.

On January 28, and 29, the produce of this experiment was examined in the presence of Sir Joseph Banks, Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. Heberden, Dr. J. Hunter, Mr. Macie, and Dr. Watson. It appeared that 9290 measures of the mixed air had been forced into the bent tube from the reservoir[1]. Besides this, Mr. Gilpin had at different times introduced 872 measures of common air, which makes in all 10,162 of air, consisting of 6968 of dephlogisticated air, and 3194 of common air. But as there were 900 measures of air remaining in the tube when the accident happened, the quantity absorbed was only 9262; but this is a much greater

[1] The method of ascertaining the quantity of air forced in was by weighing the reservoir, as mentioned in the above-mentioned Paper, p. 374 [p. 188 of this volume].

quantity that [*sic*] what from my own experiments seemed necessary for this quantity of soap-lees.

The soap-lees were poured into a small glass cup, and the tube washed with a little distilled water, in order that as little as possible might be lost. As they were by this means considerably diluted, they were evaporated to dryness; but it was difficult to estimate the quantity of the saline residuum, as it was mixed with a few particles of mercury.

Some vitriolic acid, dropped on a little of this residuum, yielded a smell of nitrous acid, the same as when dropped on nitre phlogisticated by exposure to the fire in a covered crucible; but it was thought less strong. The remainder was dissolved in a small quantity of distilled water, and the following experiments were tried with the solution.

It did not at all discolour paper tinged with the juice of blue flowers.

It left a nauseous taste in the mouth like solutions of mercury, and most other metallic substances.

Paper dipped into it, and dried, burnt with some appearance of deflagration, but not so strongly or uniformly as when dipped in a solution of nitre. The marks of deflagration, however, were stronger than when the Paper was dipped into a solution of mercury in spirit of nitre, but not so strong as when equal parts of this solution and solution of nitre were used.

A solution of fixed vegetable alkali, dropped into some of it diluted, produced a slight reddish-brown precipitate, which afterwards assumed a greenish colour.

A bit of bright copper being dipped into it, acquired an evident whitish colour, though not so white as when dipped into the solution of mercury in spirit of nitre.

From these experiments it appears, that the mixture of the two airs was actually converted into nitrous acid, only the experiment was continued too long, so that the quantity of air absorbed was greater than in my experiments, and the acid produced was sufficient, not only to saturate the soap-lees, but also to dissolve some of the mercury. The truth of the latter part is proved by the metallic taste of the residuum, its not discolouring the blue paper, the precipitate formed by the addition of fixed alkali, and the white colour given to the copper; and the nitrous fumes produced by the addition of oil of vitriol, as well as the manner in which paper impregnated with the residuum burnt, shew as plainly, that the acid produced was of the nitrous kind. It is remarkable, however, that during this experiment there were no signs which shewed when the soap-lees became saturated. The only time when the diminution proceeded much slower than usual was on January 4. It then seemed to go on very slowly; but as the air absorbed at that time was only 4830 measures, which is much less than what seems requisite to saturate the alkali, and as the diminution immediately went on again upon adding more common air, it seems not likely, that the soap-lees were saturated at that time.

On January 10, Mr. Gilpin observed a small quantity of whitish sediment on the surface of the mercury; which seems to shew, that the soap-lees were then saturated, and that the acid was beginning to corrode the mercury. The quantity of air absorbed was also 6840 measures, which is about as much as I expected would be required. However, as I was persuaded, from the event of my own experiments, that the diminution would either intirely cease, or go on very slowly, as soon as the soap-lees were saturated; and as I was unwilling to stop the experiments before that happened, I thought it best to continue the electrification.

On the same morning Mr. Gilpin found, that about 120 measures of the air in the bent tube had been spontaneously absorbed during the night, the quantity therein being so much less than it was the preceding evening, though the electrical machine had not been worked, or anything done to it during the intermediate time. The reason of this in all probability is, that as the acid was then corroding the mercury, the soap-lees became impregnated with nitrous air, which, during the night, united to the dephlogisticated air, and caused the diminution.

Though in reality the event of this experiment was such as to establish the truth of my position, that the mixture of dephlogisticated and phlogisticated air is converted by the electric spark into nitrous acid, as fully as if the experiment had been stopped in proper time; yet, as the event was in some measure different from that of my own experiments, and might afford room for cavil, I was desirous of having it repeated; and as Mr. Gilpin was so obliging as to undertake it again, the materials were, on February 11. put together for a fresh experiment, in the presence of most of the above-mentioned Gentlemen. The soap-lees employed were the same as before, but 183 measures were now introduced. The dephlogisticated air was different, the former parcel being all used. It was prepared, like the former, from turbith mineral, but was rather purer, as it seemed to contain only $\frac{1}{32}$ of phlogisticated air. The proportion in which it was mixed with common air was that of 22 to 10; so that a greater proportion of common air was now used, in consequence of which it was not necessary for Mr. Gilpin to introduce common air so often.

On February 29, the reservoir was again filled with air of the same kind, in presence of some of the same Gentlemen. As it was found by the last experiment that we must not depend on the saturation of the soap-lees being made known by any alteration in the rate of diminution, the process was stopped as soon as the air absorbed was such as from my own experiments I judged sufficient to neutralize the soap-lees. This was effected on the 15th of March. The air remaining in the tube, when Mr. Gilpin left off working, was 600 measures; but at the time the produce was examined, it was reduced to about 120, so much having been absorbed without the help of any electrification, which is a still more remarkable instance of spontaneous absorption than what occurred in the former experiment.

A few days after the experiment began, a black film was formed in one of the legs, which, I suppose, must have been a mercurial *ethiops*; but whether owing to some small degree of foulness in the mercury or tube, or to any other cause, I cannot tell. This foulness seemed not to increase; but on March 10, when the air absorbed was about 5200, a whitish sediment began to appear on the surface of the mercury.

On March 19, the produce was examined in the presence of Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. Heberden, Dr. J. Hunter, Mr. Macie, and Dr. Watson. The mixed air forced into the bent tube from the reservoir was 6650 measures, besides which Mr. Gilpin had at different times introduced 630 of common air, which makes in all 7280, containing 4570 of dephlogisticated, and 2710 of common air.

The soap-lees were evaporated to dryness as before. The residuum weighed two grains, but there were two or three globules of mercury mixed with it, which might very likely weigh half a grain. This being dissolved in a small quantity of water, the following experiments were made with it.

It did not at all discolour paper tinged with blue flowers.

Slips of paper were dipped into it, and dried; and, by way of comparison, other slips of paper were dipped into a solution both of common nitre and phlogisticated nitre, and also dried. The former burnt in the same manner, and with as strong marks of deflagration, as the latter.

It had a strong taste of nitre, but left also a slight metallic taste on the tongue.

It did not give any white colour to a piece of clean copper put into it.

In order to see whether the whitish sediment, which was before said to be formed in the bent tube, contained any mercury, the remainder of this solution was diluted with some more distilled water, and suffered to stand till the white sediment had subsided. The clear liquor being then poured off, the remainder, containing the sediment, which seemed to amount only to a very small quantity, was put on a piece of bright copper, and dried upon it; a piece of clean gold was then laid over it, and both were exposed to heat. Both metals acquired a whitish colour, especially the gold, but which was very indeterminate.

In order to discover how nice a test of alcalinity the paper tinged with blue flowers was, a saturated solution of common nitre was mixed with $\frac{1}{120}$ of its bulk of the soap-lees; and this mixture was found to turn the paper evidently green; so that, as the solution of nitre contains about twice as much alkali as the soap-lees, it appears, that if the residuum had wanted only $\frac{1}{240}$ part of being saturated, it would have discoloured the paper.

From the foregoing trials it appears, that the mixture of dephlogisticated and common air in this experiment was actually converted into nitrous acid, and was sufficient not only to saturate the soap-lees, but also to dissolve some of the mercury. The quantity dissolved, however, was very small, and not sufficient to diminish sensibly the deflagrating quality of the nitre; so that the proof of the air being converted into nitrous acid was as evident as if no mercury had been dissolved.

In this experiment, as well as the former, no indication of the soap-lees becoming saturated was afforded by any cessation in the diminution of the air; whereas, in my experiments, it was very manifest. I do not know what this difference should be owing to, except to Mr. Gilpin's giving much stronger electrical sparks than I did. In his experiments the metallic knob which received the spark, and conveyed it to the bent tube, was usually placed at about $2\frac{1}{2}$ inches from the conductor, so that the spark jumped through $2\frac{1}{2}$ inches of air, in passing from the conductor to the knob, besides from $1\frac{1}{2}$ to $2\frac{1}{2}$ inches of air in the tube; whereas in my experiments, I believe, the knob was never placed at the distance of more than $1\frac{1}{4}$ inch from the conductor, and the quantity of air in the tube was much less; but the conductor and electrical machine were the same.

Except this, the only difference I know in the manner of conducting the experiment is, first, that Mr. Gilpin usually continued working the machine for half an hour at a time, whereas I seldom worked it more than ten minutes; and, secondly, that in Mr. Gilpin's Experiments the common air in the reservoir bore a less proportion to the dephlogisticated air than in mine; in consequence of which it was necessary for him frequently to introduce common air. On this account, the proportion of the two airs in the bent tube would be considerably different at different times; but on the whole, the common air absorbed bore a greater proportion to the dephlogisticated than in mine.

Though the whole quantity of air absorbed in these experiments is known with considerable precision, yet it is impossible to determine, with any accuracy, how much of each kind was absorbed, on account of our uncertainty about the nature of the air which remained at the end of the experiment. But if in the last experiment we suppose that the air absorbed spontaneously between the 15th and 19th of March was intirely dephlogisticated, and that what remained at the end of that time was of the purity of common air, it will appear, that 4090 of dephlogisticated and 2588 of common air, which is equivalent to 4480 of pure dephlogisticated air and 2198 of phlogisticated air, were absorbed at the time the electrification was stopped, and consequently the dephlogisticated air is $\frac{204}{100}$ of the phlogisticated air; whereas in my first experiment it seemed to be $\frac{220}{100}$, and in my last $\frac{253}{100}$.

But the quantity of acid produced, and consequently, I suppose, the saturation of the soap-lees, depends only on the quantity of phlogisticated air absorbed; and the effect of the greater or less quantity of dephlogisticated air is only to make the nitre produced more or less phlogisticated. Now, in this experiment, the bulk of the phlogisticated air was $12\frac{2}{10}$ that of the soap-lees. In my first experiment it was $11\frac{9}{10}$, and in my last $10\frac{8}{10}$.

As many persons seem to have supposed that the diminution of the air in these experiments is much quicker than it really is, though I do not know any thing in my Paper which should lead to suppose that it was not very slow, it may be proper to say something on this head. As the quickness of the diminution depends so much on the power of the electrical machine, I can only speak as to what happens with the machine used in these experiments. This was one of Mr. Nairne's patent machines, the cylinder of which is 12½ inches long, and 7 in diameter. A conductor of 5 feet long, and 6 inches in diameter, was adapted to it, and the ball which received the spark was placed at two or three inches from another ball, fixed to the end of the conductor. Now, when the machine worked well, Mr. Gilpin supposes he got about two or three hundred sparks a minute, and the diminution of the air during the half hour which he continued working at a time, varied in general from 40 to 120 measures, but was usually greatest when there was most air in the tube, provided the quantity was not so great as to prevent the spark from passing readily.

The only persons I know of, who have endeavoured to repeat this experiment, are, M. Van Marum, assisted by M. Paets Van Trootswyk; M. Lavoisier, in conjunction with M. Hassenfratz; and M. Monge. I am not acquainted with the method which the three latter Gentlemen employed, and am at a loss to conceive what could prevent such able philosophers from succeeding, except want of patience. But M. Van Marum, in his *Premiere Continuation des Expériences, faites par le moyen de la Machine électrique Teylerienne*, p. 182. has described the method employed by him and M. Van Trootswyk. They used a glass tube, the upper end of which was stopped by cork, through which an iron wire was passed, and secured by cement, and the lower end was immersed into mercury; so that the electric spark passed from the iron wire to the soap-lees. After so much of a mixture of five parts of dephlogisticated and three of common air as was equal to twenty-one times the bulk of the soap-lees[1] was absorbed, some paper was moistened with the alkali, which by its burning appeared to contain nitre, but shewed that the alkali was not near saturated. The experiment was then continued with the same soap-lees till more of the air, equal to fifty-six times the bulk of the soap-lees, was absorbed, which is near double the quantity required to saturate them; but yet the diminution went on as fast as ever. It was then tried, by the burning of paper dipped into them, how nearly they were saturated; but they still seemed far from being so.

The circumstance of using the iron wire appears evidently objectionable, on account of the danger of the iron wire being calcined by the electric spark, and absorbing the dephlogisticated air; and when I first read the account, I thought this the most probable cause of the difference in the result of our experiments; but I am now inclined to think that the case was otherwise. From the manner in which M. Van Marum expresses

[1] This is rather more than half of that requisite to saturate the soap-lees.

himself, it seems that the only circumstance, from which they concluded that the alkali was not saturated, was the imperfect marks of deflagration, that the paper dipped into it exhibited in burning; which, as we have seen, might proceed as well from some of the mercury having been dissolved as from the alkali not being saturated. I am much inclined to think, therefore, that, so far from the soap-lees not having been saturated, the quantity of acid produced was in reality much more than sufficient for this purpose, and had dissolved a good deal of the mercury; for the quantity of air absorbed favours this opinion, and the phænomena agree well with Mr. Gilpin's first experiment, in which this was certainly the case; whereas, if the diminution had proceeded chiefly from the dephlogisticated air being absorbed by the iron, the tube towards the end of the experiment would have been filled chiefly with phlogisticated air, which would have made the diminution proceed much slower than before; but we are told, that it went on as fast as ever. It is most likely, therefore, that the apparent disagreement between their experiment and mine proceeded only from their having continued the process too long, and from their not having properly examined the produce.

M. Van Marum then proceeds to say:

Surpris de cette différence de résultat j'envoyai une description exacte de nos expériences à M. Cavendish, le priant en même tems de m'instruire s'il pourroit trouver la cause de cette différence; et comme la seule différence essentielle, par laquelle notre expérience différoit de celle de M. Cavendish, consistoit en ce que nous avons employé de l'air pur produit du précipité rouge ou du minium, au lieu de l'air pur produit de la poudre noire formée par l'agitation du mercure avec le plomb, dont M. Cavendish ne donne pas la maniere de le produire[1], je le priai de me communiquer de quelle maniere il étoit venu a cet air, parceque je desirois de répéter l'expérience avec ce même air: mais comme il ne m'a fourni aucune élucidation sur la cause vraisemblable de la différence du resultat de nos expériences, et qu'il ne lui a pas plu de me communiquer sa maniere de produire l'air pur qu'il avoit employé pour ses expériences, m'écrivant, qu'il s'étoit proposé d'en parler dans un écrit public, la longueur ennuyante de ces expériences nous a fait prendre la resolution de différer leur continuation, pour obtenir une parfaite saturation de la lessive, jusqu'à ce que M. Cavendish ait publié sa maniere de produire l'air pur, dont il s'est servi, nous contentant pour le present d'avoir vu, que l'union du principe d'air pur et de la mofette produit de l'acide nitreux, suivant la découverte de M. Cavendish.

[1] The using the iron wire formed a material difference in our manner of conducting the experiment, and one which may, perhaps, have had great influence on the result; but I do not see how the using some other kind of dephlogisticated air, instead of that prepared from Dr. Priestley's black powder, can in the least degree form an essential difference, as in the same paragraph in which I mention my having used this kind of air in my first experiment, I say, that in my second experiment I used air prepared from turbith mineral.

As I should be sorry to be thought to have refused any necessary information to a Gentleman who was desirous to repeat one of my experiments, and who by his situation was able to do it with less trouble than any one else, I hope the Society will indulge me in adding a copy of my answer, that they may judge whether this is in any degree a fair representation of it.

To M. Van Marum.

Sir,

I received the honour of your letter, in which you inform me of your ill success in trying my experiment on the conversion of air into nitrous acid by the electric spark. It is very difficult to guess why an experiment does not succeed, unless one is present and sees it tried; but if you intend to repeat the experiment, your best way will be to try it with the same kind of apparatus that I described in that Paper. If you do so, and observe the precautions there mentioned, I flatter myself you will find it succeed. The apparatus you used seems objectionable, on account of the danger of the iron being corroded by absorbing the dephlogisticated air.

As to the dephlogisticated air procured from the black powder formed by agitating mercury mixed with lead, as it was foreign to the subject of the Paper, and as I proposed to speak of it in another place, I did not describe my method of procuring it. As far as I can perceive, the success depends intirely on carefully avoiding every thing by which the powder can absorb fixed air, or become mixed with particles of an animal or vegetable nature, or any other inflammable matter: for which reason care should be taken not to change the air in the bottle in which the mercury is shaken, by breathing into it, as Dr. Priestley did, or even by blowing into it with a bellows, as thereby some of the dust from the bellows may be blown into it. The method which I used to change the air was, to suck it out by means of an air-pump, through a tube which entered into the bottle, and did not fill up the mouth so close but what air could enter in from without, to supply the place of that drawn out through the tube.

I am, &c.

With regard to the main experiment, it was not in my power to give him further information than I did; as I pointed out the only circumstance to which, at that time, I could attribute the difference in our results. And with regard to the manner of preparing the dephlogisticated air from the black powder, I have mentioned all the particulars in which my manner of proceeding differed from Dr. Priestley's, and have also explained on what I imagine the success intirely depends; so that, I believe, no one at all conversant in this kind of experiments will think that I did not communicate to him my method of procuring that air.

PHILOSOPHICAL TRANSACTIONS. VOL. 80, 1790, p. 101

X. *On the Height of the Luminous Arch which was seen on* Feb. 23, 1784. *By* Henry Cavendish, *Esq.*, *F.R.S. and A.S.*

Read February 25, 1790

THIS arch was observed, at the same time, at Cambridge by Mr. Wollaston; at Kimbolton in Huntingdonshire, by the Rev. Mr. Hutchinson; and at Blockley near Campden in Gloucestershire, by Mr. Franklin; and is described in letters from those gentlemen read to the Royal Society in December 1786[1].

It has been remarked, that as the arches of the kind described in these Papers have usually but a very slow motion, their height above the surface of the earth may readily be determined, provided they are observed about the same time, at places sufficiently distant; and they seem to be the only meteors of the aurora kind whose height we have any means of ascertaining.

The three places at which this phænomenon was seen are not so well suited for this purpose as might at first be expected from their distance, because they lie too much in the direction of the arch; they however seem sufficient to determine its height within certain limits, and perhaps are as well adapted for it as any observations we are likely to have of such phænomena.

The latitude of Cambridge is 52° 12′ 36″: that of Kimbolton is said by Mr. Hutchinson to be 52° 20′, and, according to the survey of Huntingdonshire, published by Jefferies, is 52° 19′ 50″; so that we may suppose it to be seven geographical miles north of Cambridge, and by the maps it seems to be about 18 such miles west of it: and Blockley is by the map 12 geographical miles south and 72 west of Cambridge.

At Cambridge the observations of its track seem to have been made at about 9 h. 15′ P.M. or 8 h. sidereal time. At Kimbolton, allowing for the difference of meridians, they could hardly have been made more than 5′ sooner; and at Blockley they were most likely made nearly at the same times as at Cambridge.

[1] See pp. 43–46, of this Volume [i.e. Vol. 80 of the *Phil. Trans.*].

At Blockley the arch passed about 7° south of the zenith; but it is unnecessary to determine this point with precision. At Kimbolton it was found by a quadrant to pass 11° to the south of it; and at Cambridge it was observed to pass through δ and ϵ Tauri, β Aurigæ, θ Ursæ majoris, Cor Caroli, and Arcturus. Now, if an arch was drawn through these stars, it must, I think, have appeared sensibly waved to the eye; whereas Mr. Wollaston did not take notice of any crookedness in this part of its course. It is most likely, therefore, that the middle of the arch must have passed to the south of β Aurigæ, and to the north of θ Ursæ; and if a circle is drawn through δ Tauri, Arcturus, and a point one degree north of the zenith, it will differ but little from a great circle, will agree as well with the positions of these stars as any regular line which can be drawn, and will pass $2\frac{1}{2}$ degrees below β Aurigæ, and as much above θ Ursæ; which is not a greater difference from observation than may well have taken place, considering how much care and acquaintance with the fixed stars are required to determine a path by them so nearly.

The direction of the arch here described in that part near the zenith is W. 18° S.; and if a line is drawn through Cambridge in this direction, Kimbolton is 12,8 geographical miles north of it; and therefore, as the arch appeared 12° more south at Kimbolton than at Cambridge, the height of the arch above the surface of the earth must be $61\frac{1}{2}$ geographical or 71 statute miles. If we suppose that the middle of the arch really passed through β Aurigæ, the height comes out 52 statute miles. On the whole, I should think, the height could hardly be less than 52 miles, and is not likely to have much exceeded 71.

The common aurora borealis has been supposed, with great reason, to consist of parallel streams of light shooting upwards, which, by the laws of perspective, appear to converge towards a point; and when any of these streams are over our heads, they appear actually to come to a point, and form a corona. Hence, from analogy, it seems not unlikely, that these luminous arches may consist of parallel streams of light, disposed so as to form a long thin band, pretty broad in its upright direction, and stretched out horizontally to a great length one way, but thin in the opposite direction. If this is the case, they will appear narrow and well-defined to an observer placed in the plane of the band; but to one placed at a little distance from it, they will appear broader, fainter, and less well-defined; and when the observer is removed to a great distance from the plane, they will vanish, or appear only as an obscure ill-defined light in the sky.

There are two circumstances which rather confirm this conjecture: first, that though we have an account of another arch besides this[1] having been seen at great distances in the direction of the arch, we have none of any having been seen in places much distant from each other in the contrary direction; and, secondly, that most of them have passed near the

[1] That of Feb. 15, 1750. *Phil. Trans.* XLVI. pp. 472 and 647.

zenith, whereas otherwise they ought frequently to appear in other situations; for if they appeared near the zenith to an observer in one latitude, they should appear in a very different situation in a latitude much different from that.

I wish it to be understood, however, that I do not offer this as a theory of which I am convinced; but only as an hypothesis which has some probability in it, in hopes that by encouraging people to attend to these arches, it may in time appear whether it is true or not. If it should hereafter be found, that these arches are never seen at places much distant from each other in a direction perpendicular to the arch, it would amount almost to a proof of the truth of the hypothesis; but if they ever are seen at the same time at such places, it would shew that the hypothesis is not true.

Supposing the hypothesis to be well-founded, the height above determined will answer to the middle part of the band, provided the breadth of it was small in respect of its distance from the earth, but otherwise will be considerably below the middle. If the breadth of the band was equal to the distance of its lower edge from the earth, the height of the lower edge would be three-fourths of that above found; and if the breadth was many times greater, would be half of it.

In the common aurora borealis, an arch is frequently seen low down in the northern part of the sky, forming part of a small circle. What this is owing to, I cannot pretend to say; but it is likely that it proceeds from streams of light which appear more condensed when seen in that direction than in any other, and consequently that the streams which form the arch to an observer in one place are different from those which form it to one at a distant place, and consequently that no conclusion as to its height can be drawn from observations of it in different places. Attempts, however, have been made to determine the height of the aurora from such observations, and even from those of the Corona[1]; though the latter method must surely be perfectly fallacious, and most likely the former is so too.

[1] Bergman. *Opusc.* Vol. v.

PHILOSOPHICAL TRANSACTIONS. VOL. 82, 1792, p. 383

XX. *On the Civil Year of the* Hindoos, *and its Divisions; with an Account of three* Hindoo *Almanacs belonging to* Charles Wilkins, *Esq. By* Henry Cavendish, *Esq.*

Read June 21, 1792

THOUGH we have received much information concerning the astronomy of the Hindoos, we know but little of their civil year, and its divisions; and what accounts of it we have received vary much from each other, owing partly, as will be seen, to different methods being used in different parts of India. As it occurred to me, that the best way by which a person in Europe could clear up the difficulties in this subject, would be to examine the *patras*, or almanacs, published by the Hindoos themselves, I applied to Mr. Wilkins, well known for his skill in the Sanskreet language, who was so good as to lend me three such, and assist me in finding out their meaning.

One of them was procured by Mr. Wilkins at Benares, and is computed for that place. The second came from Tanna, in the island of Salsette, near Bombay; but it appears to be the copy of a Benares patra, as it is disposed in the same form as the first, and is adapted to the same latitude and longitude. The third is computed for Nadeea, a town of Bengal, about 50 miles N. of Calcutta, almost as noted for learned men as Benares, and much frequented by students from the coast of Coromandel. The language of all three of them is a corrupt Sanskreet; but the last is written in the common Bengal character.

It appears from these almanacs that the civil year is regulated very differently in different parts of India; but before I speak of this year, it will be proper to mention a few words of the astronomical, which in all parts serves to regulate the civil year.

The astronomical year begins at the instant when the sun comes to the first point of the Hindoo zodiac. In the present year, 1792, it began, according to the principles delivered in the *Surya Siddhanta*[1], on April 9, at $22^h\ 14'$ after midnight of their first meridian, which is about $41'$ of time west of Calcutta; but according to Mr. Gentil's account of the Indian

[1] See an account of this in the 2d volume of the *Asiatic Researches*.

astronomy, it began $3^h\ 24'$ earlier. As this year, however, is longer than ours, its commencement falls continually later in respect of the Julian year by $50'\ 26''$ in four years.

This year is divided into 12 months, each of which corresponds to the time of the sun's stay in some sign, so that they are of different lengths, and seldom begin at the beginning of a day.

The civil day, in all parts of India, begins at sun-rise, and is divided into 60 parts, called dandas, which are again divided into 60 palas.

The only parts of the Benares patras which are of any material use for my purpose, are the names of the months which are set down at the top of each page, and the three first columns, the first of which contains the day of the month, according to the civil account, the next the day of the week, and the third the time at which the lunar *teethee* ends; but as many may like to be informed of the nature of an Hindoo almanac, I shall give an account of the remaining parts at the end of this paper.

In those parts of India in which this almanac is used, the civil year is lunisolar, consisting of 12 lunar months, with an intercalary month inserted between them occasionally. It begins at the day after the new moon next before the beginning of the solar year[1].

The lunar month is divided into thirty parts, called teethees; these are not strictly of the same length, but are equal to the time in which the moon's true motion from the sun is 12°. From the new moon till the moon arrives at 12° distance from the sun, is called the first teethee. From thence till it comes to 24°, is called the second teethee; and so on till the full moon; after which the teethees return in the same order as before.

The civil day is constantly called by the number of that teethee which expires during the course of the day.

[1] My reasons for saying that the civil year begins at the day after the new moon next before the beginning of the solar year, are as follow: 1st. These almanacs begin at this time, and, moreover, the year of Veekramādeetya and Sālavāhana, which is set down at the top of each page, is the same in the first page as in all the following, which would be improper, unless the year began at this time. 2dly. In the calculation of the eclipse of the sun, in Père Patouillet's Memoir, given in Bailly's *Astronomie Indienne*, the computation is made for the new moon preceding the beginning of the solar year, and yet the year of Sālavāhana, and of the cycle of 60, set down in the Memoir, is the same as if the solar year was already begun. 3dly. Père du Champ, in his table of the names of the years of the cycle of 60, given in the same book, had added to some of them the corresponding year of Christ, together with a day of the month. This day, in all of them, is the day next after the new moon, preceding the beginning of the solar year: and though no explanation is given, must evidently be intended for the day on which the year begins. And, 4thly. It is said in the *Ayeen Akbery*, by Abraham Roger, and, I believe, some other authors, that the year begins at this time. To the three last authorities, indeed, it may be objected, that they are taken from places in which we do not know that the Benares almanac is used; but they shew, that in some parts of India the year begins at that time, and if it does so in any place, it most likely does at Benares.

As the teethee is sometimes longer than one day, a day sometimes occurs in which no teethee ends. When this is the case, the day is called by the same number as the following day; so that two successive days go by the same name.

It oftener happens that two teethees end on the same day, in which case the number of the first of them gives name to the day, and there is no day called by the number of the last; so that a gap is made in the order of the days.

In the latter part of the month the days are counted from the full moon, in the same manner as in the former part they are counted from the new moon; only the last day, or that on which the new moon happens, is called the 30th instead of the 15th.

It follows from what has been said, that each half of the month constantly begins on the day after that on which the new or full moon falls; only sometimes the half month begins with the second day, the first being wanting.

The manner of counting the days, as we have seen, is sufficiently intricate; but that of counting the months, is still more so.

The civil year, as was before said, begins at the day after the new moon; and moreover, in the years which have an intercalary month, this month begins at the day after the new moon; but notwithstanding this, the ordinary civil month begins at the day after the full moon. To make their method more intelligible, I will call the time from new moon to new moon, the natural month. The civil month Visākha begins at the day after the full moon of that natural month which commences at the beginning of the civil year, or, in other words, at the day after the full moon of that natural month during which the sun enters the first Hindoo sign. Jyēshtha begins on the day after the full moon of that natural month during which the sun enters the second sign, and so on. The names of the civil months, with the names of the signs which the sun enters during the natural month at the full moon of which the civil month begins, are given in the following table, [p. 239] to which I have also added the day of our month when the sun entered that sign in the latter part of the year 1784, and beginning of 1785, taken from the Benares almanac, the time of the day being counted from sun-rise, and expressed in the Hindoo manner.

It may be observed, that in general, Visākha begins at the day after that full moon which is nearest to the instant at which the sun enters Mesha, whether before or after; however, it is not always accurately the nearest.

The two parts of each month are distinguished in these almanacs by the addition of the syllables *vadee* and *soodha* to the name; thus the first half of Visākha, or that from the day after the full, to the day after the new moon is called Visākha-vadee, and the remainder Visākha-soodha[1];

[1] *Soodha* signifies clear, pure, or complete; but the word *Vadee* is not to be found in any of Mr. Wilkins's dictionaries.

Civil Month	Sign	Day on which the ⊙ enters it			
			1784		
			day.	dan.	pa.
Visākha	Mesha	April	9,	37,	7
Jyēshtha	Vreesha	May	10,	34,	8
Āshāra	Meetoona	June	11,	0,	8
Srāvana	Karkata	July	12,	37,	58
Bhādra	Seengha	August	13,	7,	11
Aswēena	Kanyā	Sept.	13,	7,	36
Kārteeka	Toolā	Octob.	13,	32,	55
Mārgaseersha	Vreescheeka	Nov.	12,	25,	38
Powsha	Dhanoo	Decem.	11,	54,	18
			1785		
Māgha	Makara	Jan.	10,	13,	11
Phālgoona	Koombha	Feb.	8,	40,	21
Chitra	Meena	March	10,	30,	38

but, I believe, the more usual way of distinguishing them is by the words *kreeshna paksha*, or the dark side, and *sookla paksha*, the bright side.

A consequence of this way of counting the months is, that the first half of Chitra falls in one year, and the latter half in the following year.

Whenever the sun enters no sign during a natural month, this month is intercalary, and makes an irregularity, which may best be explained by an example.

In the year 1779, the sun entered into no sign during the natural month which began at the end of the first fortnight of Srāvana; accordingly the whole of this month was intercalary, and the fortnight which preceded it was called Neeja Srāvana vadee, instead of simply Srāvana vadee, as it would otherwise have been named. The first half of the intercalary month was called Adheeka Srāvana soodha, and the latter half Adheeka Srāvana vadee, and the fortnight after the intercalary month, Neeja Srāvana soodha[1].

It appears, therefore, that the two parts of the month where the intercalation takes place, are separated from each other by the interval of the whole intercalary month, and have the word Neeja prefixed to them; and the two parts of the intercalary month are called by the same name, but have the word Adheeka prefixed[2].

[1] *Adheeka* signifies over and above, or intercalary. *Neeja* prefixed to the name of the month signifies that month itself.

[2] What has been here said, agrees perfectly with Mr. Wilkins's almanacs; the only doubt is, whether there may not be some different method of regulating the month, which may also agree with these almanacs, and may be the true one. It is proper, therefore, that I should state my reasons for the account here given. Du Champ, who seems a very accurate writer, says (see Bailly, p. 320) that he was informed by a Hindoo calculator, that whenever the sun enters no sign during a lunar month, that month is doubled. This passage agrees very well with these almanacs, if by month we mean the time between two new moons; but disagrees entirely with

In these almanacs no notice is taken of the solar months, notwithstanding that a column is allotted to the day of the Mahometan calendar, which seems to shew that, in the countries which use the Benares patra, it is not customary to date by the solar month; for it is very unlikely that the computers of these almanacs should have given the days of the Mahometan calendar, and yet have omitted days used in their own.

In those parts of India which use the Nadeea patra, the case is quite different. This almanac contains the name of the solar and lunar month, with the corresponding days of the week and solar month, and the number of the lunar teethee which ends on those days. It begins with the day after that on which the astronomical year commences. This is marked as the first of the month, the next day is called the second, and so on, regularly to the end of the month. In like manner, all the other months begin on the day after the astronomical commencement, and the days are continued regularly to the end, so that the number of days in the month varies from 29 to 32[1].

them if we mean by it the time between two full moons; and moreover, in Mr. Wilkins's almanac it is the period from one new moon to another, which is called Adheeka. It seems certain, therefore, that in this passage the word month must mean what I have called the natural month; and that the rule for intercalation is such as I have mentioned, namely, that it shall take place whenever the sun enters no sign during the natural month. It is certain also that the ordinary civil month begins at the day after the full moon; and granting these two points, I cannot see any way in which the months can be regulated so as to differ in substance from what I have said.

[1] Perhaps I do not express myself accurately in saying that the civil month begins at the day after the commencement of the astronomical. It is true, that in this almanac it is the day after the commencement of the astronomical month, which is marked by the number one; but it must be observed that the Hindoos count by years complete, not by years current: for example, the year 1000 of the Kalee Yug begins at the time when 1000 years are completed from the Kalee Yug; and it is likely that the same manner of counting is adopted with regard to days, so that the day of the month marked one, does not signify the first day, but the day which begins at the expiration of the first day, and consequently that the civil month begins at the sun-rise of the day on which the astronomical month begins. I, however, have chosen to say that it begins at the day after, partly because I am not sure that the foregoing is the true meaning of the Hindoos, and partly because it would have been difficult to express myself in such manner as not to run great risk of being misunderstood, if I had done otherwise. What is here said applies equally to the lunar month in this and the Benares almanacs.

Though it is foreign to the subject of this paper, I cannot refrain from taking notice of an error, which I apprehend many European astronomers have fallen into, from not distinguishing between days current and days complete. It is common to say that the astronomical day begins twelve hours later than the civil day, and the nautical day twelve hours sooner; and it is true that the hour, which, according to the civil account is called one in the afternoon of the first of January, is written by astronomers January $1^d 1^h$, but this, I apprehend, ought not to be read 1^h on the first of January, but 1^d and 1^h from the beginning of January, so that in reality the astronomical and nautical day both begin 12^h before the civil. A proof of the truth

The names of the months are the same as those of the lunar months in the Benares patra, Visākha being the first, or that which corresponds with the sign Mesha.

The lunar months begin, not at the full, as in the Benares patra, but at the new moon, and are called by the name of that solar month which ends during the course of them; for example, the lunar month, during which the solar month Visākha ends, is called Chandra (or lunar) Visākha, so that each month begins a fortnight later than by the Benares patra.

The teethees do not recommence at the full moon, but are continued to the end of the month, or to the 30th. In other respects they are counted as in the Benares patra; that is, the same notation is used whenever a day occurs in which no teethee ends, or when two teethees end on the same day.

Unluckily no intercalary month occurred in the year for which this almanac was computed, so that it gives us no information about the method of intercalation; but from analogy we may conclude, that those lunar months in which the sun enters no sign are intercalary, and are called by the name of either the preceding or following month, with the addition of some word to denote that they are intercalary[1].

As the Nadeea almanac begins with the day after the commencement of the solar year, and gives the day of the solar month, which the Benares patra does not, it affords reason to think that the custom of that part of India in which it is used, is to date by the solar month, and begin the year on the next day to the astronomical year; and accordingly Mr. Wilkins informs me, that the Hindoos of Bengal, in all their common transactions, date according to solar time, and use what is commonly called the Bengal era, but in the correspondence of the Brahmins, dating books, and regulating feasts and fasts, they generally note the teethee; and if the year is mentioned, it is often that of Veekramādeetya, sometimes that of Sālavāhana, but more frequently the vulgar Bengal year.

From what has been said, it appears, that the Hindoo civil months, both solar and lunar, consist, neither of a determinate number of days, nor are regulated by any cycle, but depend solely on the motions of the sun and moon, so that a Hindoo has no way of knowing what day of the month it is, but by consulting his almanac; and what is more, the month ought sometimes to begin on different days, in different places, on account of the difference in latitude and longitude, not to mention the difference

of this is, that in astronomical tables the place of the heavenly bodies set down for the beginning of the year, is the place for noon of the last civil day of the preceding year; and moreover, in Halley's tables this place is said to be *annis Julianis ineuntibus*, which shews that he thought that this was not merely a practice used for the sake of convenience, but that the year actually begins at this time.

[1] The Chinese, who, like the Hindoos, consider that lunar month as intercalary in which the sun enters no sign, call it by the same name as the preceding month; and it is likely that the Bengalese do so too.

which may arise from errors in computation. The inconvenience with which this must be attended seemed so great to me, that two or three years ago, by the assistance of Sir Joseph Banks, I proposed a query on the subject to Mr. Davis, author of the very valuable paper, in the Asiatic Researches, on the Hindoo astronomy, inquiring whether any method was taken to avoid the ambiguity, and was favoured with the following answer.

My Pundit, and others with whom I have conversed on the subject, although well aware of the circumstance (that the month may begin on different days in different places) do not think the ambiguity thence arising of much consequence, nor is there any method they know of taken to avoid it. The almanacs in common use are computed at Benares, Tirhut[1], and Nadeea, and three principal seminaries of Hindoo learning in the Company's provinces, whence they are annually dispersed throughout the adjacent country. Every Brahmin in charge of a temple, or whose duty it is to announce the times for the observance of religious ceremonies, is furnished with one of these almanacs; and if he be an astronomer, he makes such corrections in it as the difference of the latitude and longitude render necessary.

The beginning of the solar month falling on different days of the week, is not, as I have observed, regarded; but a disagreement in the computation of the teethee, which sometimes also happens, occasions no small perplexity, because by the teethees, or lunar days, are regulated most of their religious festivals: and I am assured that an instance of this kind, which occurred in Cossim Ally's time, obliged the Rajah of Nadeea to settle by proclamation which of the disputed computations should be regarded as the true one.

To the best of Mr. Wilkins's knowledge, the Nadeea almanac is used all over Bengal, and the Benares all over the upper part of India: and it is likely, therefore, that the Tirhut is used all over Bahar; but of the nature of this almanac I have no information; only to judge from the date of the inscription found at Mongueer[2], it is more likely to agree with the Nadeea than Benares patra.

As one of Mr. Wilkins's Benares patras came from Salsette, we may conclude that this almanac is in use in that part of India. The inscriptions too, found at Salsette and Delhi[3], confirm the opinions that this manner of dating is in use in both those places, as both are dated by the day of the bright side of the moon.

It appears from P. du Champ, and P. Patouillet, and I believe I may add Abraham Roger, that in the part of India from which they write, the civil year begins at the new moon before the beginning of the astronomical year[4]; which seems to shew that the Benares manner of dating is in use in

[1] A district in North Bahar.

[2] *Asiatic Researches*, vol. I. p. 127.

[3] *Asiatic Researches*, vol. I. pp. 363 and 379.

[4] Narsapour, from which P. Patouillet writes, is near the coast, and in the latitude of 16°½ N. Chrisnabouram, from which P. du Champ's Memoir is sent, is in

great part of the coast of Coromandel; but there is some reason to think, that in the neighbourhood of Madras and Pondicherry, they date in a manner different from that used either at Benares or Nadeea: for Mr. Gentil makes the month Chitra or Sitterey, as he spells it, correspond with the sign Mesha, in which he agrees with an alamanac published by an European at Madras, which seems to shew that in those places they date by solar months, but make Chitra correspond with the first sign.

Mr. Wilkins thinks he has heard of one or two places on the east coast of the Peninsula, and in particular Orissa, at which almanacs are computed; but he is not acquainted with the nature of them.

I shall now give a more particular account of the three almanacs. The two Benares patras are preceded by a preface, which begins with an invocation to the Deity, and then gives a whimsical account of the four Yoogas, or ages, and of the inferiority of each succeeding age to that preceding it, and concludes with astrological remarks.

There are no titles to any of the columns of which the almanacs are composed, nor is any explanation of them given in any part of the work; but by a careful examination of the numbers, a person acquainted with astronomical computations may, without much difficulty, find out their meaning.

The calendar part contains one page for each half of the lunar month. At the top of each page is given the year of the eras of Veekramādeetya and Sālavāhana. After this comes the name of the month, and in one almanac is given also the name and number of the month used by the Mahometans.

The part below this consists of eleven columns. The first gives the day of the month, according to the civil reckoning; the next the day of the week; and the two following contain the time of the day, that is the danda and pala at which the lunar teethee ends. The fifth column contains the name of the nakshatra[1] which the moon quits during the course of the day; and the two next shew the time at which she quits it.

The next three columns are very odd; they serve to shew the moon's place in what may be called a moveable zodiac, the first point of which moves backwards with the same velocity with which the sun moves forwards, and coincides with the sun at the beginning and middle of the Hindoo year. This zodiac is divided into twenty-seven equal parts, and the first of these three columns gives the name of the 27th part which the moon quits during the course of the day, and the two others the time at which she quits it. I do not know what use these columns can be applied

nearly the same latitude, but about 2° inland, and Paliacat, where Abraham Roger resided, is on the coast, in the latitude of 13°½, or near ½ a degree N. of Madras. This author, however, has expressed himself so inaccurately, that I am not sure whether they begin the year at that time or not.

[1] Otherwise called the 27 lunar mansions.

to, unless that of astrology. No trace of any thing of the kind has occurred to me in any account of the Hindoo astronomy[1].

In these columns the names of the days of the week, and nakshatras, are expressed by the first syllable of the word.

The last column is the day of the month used by the Mahometans.

As no explanation of these columns is given in the almanacs, it will be proper to mention my reasons for supposing them to be such as I have asserted.

The numbers in the third and fourth columns increase while the moon is near her apogee, and diminish during the rest of the month, which shews that it must be the time at which the moon completes some part of a revolution; and by examining these numbers during twelve revolutions of the moon in anomaly, it appears that the moon moves over 336 of these parts in $330^{d}\ 41^{dan.}\ 43^{pal.}$ which differs very little from the time answering to 336 teethees, so that there can be no doubt but that these columns shew the time at which the teethee ends. But a further proof of the truth of it is, that the time given in these columns for the end of the last teethee of each half month, agrees pretty nearly with the time of the new and full moon given in the nautical almanac, after allowing for the difference of longitude between Greenwich and Benares, and the time between sun-rise, at the latter place, and noon; which shews also that the time in these columns is reckoned from sun-rise, as might naturally be expected.

In regard to the moon's place in the nakshatras and moveable zodiac, it appears, by examining the fifth and eighth columns, that in each of them are 27 characters, which return constantly in order, except when the regularity is broken, either by the moon quitting two spaces in the same day, or by not quitting any one space in the day. The numbers also, both in the sixth and seventh, and in the ninth and tenth columns, increase when the moon is near the apogee, and diminish when she is near the perigee, which shews that they must be the time at which the moon finishes some 27th part of a revolution of one kind or other; and by examining the alteration of the numbers during twelve revolutions of the moon in anomaly, it appears first, that the moon describes 326 of the spaces given in the fifth column, in $329^{d}\ 57^{dan.}\ 38^{pal.}$ which is the time in which the moon moves over that number of nakshatras; and secondly, that the moon describes 350 of the spaces given in the eighth column in $329^{d}\ 36^{dan.}\ 48^{pal.}$ which is the time in which the sum of the mean motions of the moon and sun are equal to 350 27ths of a circle; or in other words, is the time in which the moon's motion in the moveable zodiac is 350 of these 27th parts; and moreover, I cannot find any other 27th of a revolution of the moon which will agree with this time; which is a sufficient proof that the numbers in the ninth and tenth columns are the times at which

[1] From a circumstance not worth mentioning, I find that the place of the moon in this moveable zodiac, is called the Yug.

the moon quits one of these 27th parts in the moveable zodiac. But a thing which more strongly proves the truth of this, and which also shews that the first point of this moveable zodiac coincides with the first point of the fixed zodiac, when the sun also coincides with it, is this: according to my supposition it is evident, that whenever the sun quits a nakshatra at the same time that the moon quits some other nakshatra, the moon must at the same time quit some 27th part of the moveable zodiac; and consequently that the numbers in the ninth and tenth columns should agree with those in the sixth and seventh; and accordingly we find, that on all the days of the year, in which the sun quits a nakshatra, the numbers in these two pairs of columns are nearly alike.

Underneath these eleven columns are tables of the diurnal motion and places of the sun and five planets, and of the moon's node in the Hindoo zodiac, for each week of the year; and between these tables and the eleven columns is set down the day of the month and week, and number of the week for which these places are given, and also the interval at that time between sun-rise and midnight, and the length of the day. The day of the week for which these places are given, is that which is the first in the current solar year, and the number of the week is also counted from the beginning of the solar year. The places are given for midnight.

On the right hand of the eleven principal columns is a space allotted for miscellaneous occurrences. In this is set down the time at which the sun enters each sign, and the beginning and end of eclipses. In these two years no solar eclipses were visible, but the end of the lunar eclipse is denoted by a Sanskreet word, signifying delivery; the meaning of the term used for the beginning is not so clear. The number of digits eclipsed is not set down. The other articles in this space consist chiefly of the time at which the moon and planets come to certain situations. Of this there is not a great deal which I understand, and what I do, is not worth taking notice of. There are also some figures and tables between the preface and calendar, which, as far as I can find, relate only to astrology.

The Nadeea almanac contains, besides the articles above-mentioned, the time of the day at which the lunar teethee ends, the number of the nakshatra and yug (place in the moveable zodiac) which the moon quits on that day, and the time at which she quits them, besides a few occasional remarks. It is disposed in a much coarser manner than the Benares patra, as each page contains as many days as it will hold, so that the month seldom begins at the beginning of a page. It contains no preface, and no explanation of the columns. The days of the week are not denoted by the first syllables of the name, but only by a number, expressing their order in the week, which caused some trouble in finding what day was meant by these numbers; but, by a variety of circumstances, I think it certain that the number 1 must denote Sunday.

PHILOSOPHICAL TRANSACTIONS. VOL. 87, 1797, p. 119

Extract of a Letter from Henry Cavendish, *Esq. to Mr.* Mendoza y Rios, *January*, 1795

[Addition to a Paper by Joseph de Mendoza y Rios, entitled "Recherches sur les principaux Problèmes de l'Astronomie Nautique."]

[Read December 22, 1796]

THE methods in which the whole distance of the moon and star is computed, particularly yours, require fewer operations than those in which the difference of the true and apparent places is found; but yet, as in the former methods, it is necessary either to take proportional parts, or to use very voluminous tables; I am much inclined to prefer the latter. This induced me to try whether a convenient method of the latter kind might not be deduced from the fundamental proposition used in your paper, and I have obtained the following, which has the advantage of requiring only short tables, and wanting only one proportional part to be taken, and I think seems shorter than any of the kind I have met with.

Let h and H be the apparent and true altitude of the star; l and L the apparent and true altitude of the moon, g and G the apparent and true distance of the moon and star. Let the sine and cosine of $g = d$ and δ, the sine and cosine of $l = a$ and α, the sine and cosine of $h = b$ and β; and the sine of the actual and mean horizontal parallax $= p$ and π; and let the sine of $L = a - m + pe$, and its cosine $= \alpha\,(1 + \mu - p\epsilon)$ and let the sine of $H = b - n$, and its cosine $= \beta\,(1 + \nu)$.

Then the cosine of G

$$= \delta\,(1 + \mu - p\epsilon)\,(1 + \nu) + (a - m + pe)\,(b - n) - ab\,(1 + \mu - p\epsilon)\,(1 + \nu),$$

which equals

$$\begin{aligned}\delta &+ \delta\mu + \delta\nu - \delta p\epsilon + \delta\mu\nu - \delta p\epsilon\nu + ab - bm + bpe - an + nm - npe \\ &- ab - ab\mu + abp\epsilon - ab\nu - ab\mu\nu + ab\nu p\epsilon = \delta + \delta\mu + \delta\nu - \delta p\epsilon \\ &- bm - ba\mu + bpe + bap\epsilon - an - ab\nu + nm - npe - ab\mu\nu \\ &+ ab\nu p\epsilon + \delta\mu\nu - \delta\pi\epsilon\nu.\end{aligned}$$

To make use of this rule, it must be considered that the quantity $\delta\mu\nu - \delta p\epsilon\nu$ is so small that it may safely be disregarded; but

$$nm - npe - ab\mu\nu + ab\nu p\epsilon,$$

if the altitudes are not more than 5°, may amount to about 12″, and therefore ought not to be neglected. The quantity $e + a\epsilon$ also differs very little from one, but it is not quite equal to it. Let therefore a table be made under a double argument, namely, the altitudes of the moon and star, giving the value of

$$nm - n\pi e - ab\mu\nu + ab\nu\pi\epsilon + b\pi e + ba\pi\epsilon - b\pi,$$

answering to different values of these altitudes, which call A. Let a second table be made under a double argument, namely, the altitude of the star and the apparent distance of the moon and star, giving the value of $\delta\nu$, which call D. Let a third table be made with the observed altitude for argument, giving the logarithm of $am + a^2\mu$; and let this quantity, answering to the moon's altitude, be called M, and that answering to the star's altitude, N; observing that the same table will do for the moon and star; but a fourth table should be made for the sun, so as to include its parallax; and, lastly, let a fifth table be made, with the moon's altitude for argument, giving the logarithm of $\frac{\epsilon}{a} - \frac{\mu}{\pi a}$, which call C. Then will

$$\cos . G = \delta - \delta apC - \frac{bM}{a} - \frac{aN}{b} + bp + D - A.$$

It must be observed that $\delta apC = \delta p\epsilon - \frac{\delta\mu p}{\pi}$, whereas it ought to equal $\delta p\epsilon - \delta\mu$; but μ cannot exceed 57″, and the horizontal parallax cannot differ from the mean by more than $\frac{1}{18}$ part of the whole; so that the error arising from thence cannot exceed 3″ or 4″. This small error however may be diminished by giving the quantity C for more than one horizontal parallax.

Addition to the foregoing Letter.

I have procured tables of the above-mentioned kind to be computed, which are intended to be inserted in a work now printing by Mr. Mendoza y Rios. Allowance is made in them for the alteration of the refractive power of the atmosphere, which is done by two new tables, one giving the correction of the logarithms M and N, and the other the sum of the corrections of $\delta\mu$ and $\delta\nu$. Now it must be observed, that the quantities μ and ν vary only from 57″ to 51″; and therefore the corrections of $\delta\mu$ and $\delta\nu$, may, without any material error, be considered as the same at all altitudes; and therefore the sum of the corrections may be comprehended in a table, under a double argument, namely, the refractive power of the atmosphere and the apparent distance.

In order to avoid as much as possible the inconvenience arising from using negative quantities, or giving different cases, the table *D* is continued to 125° of apparent distance, and the numbers in the table *A* are increased by 0,0003, so as to make them always positive; and to compensate this, the numbers in *D* are increased by 0,0002, and those in the correction of $\delta\mu + \delta\nu$ by 0,0001. It was found proper also to give the table *C* for four different values of horizontal parallax.

The above tables are short, and do not require proportional parts to be taken. The only part of the work in which this is wanted, is in finding the angle answering to the natural cosine of the true distance. In finding the natural cosine of the apparent distance this is avoided, by neglecting the odd seconds in working the problem, and adding them to the result.

PHILOSOPHICAL TRANSACTIONS. VOL. 88, 1798, p. 469

XXI. *Experiments to determine the Density of the Earth. By* Henry Cavendish, *Esq., F.R.S. and A.S.*

Read June 21, 1798

MANY years ago, the late Rev. John Michell, of this Society, contrived a method of determining the density of the earth, by rendering sensible the attraction of small quantities of matter; but, as he was engaged in other pursuits, he did not complete the apparatus till a short time before his death, and did not live to make any experiments with it. After his death, the apparatus came to the Rev. Francis John Hyde Wollaston, Jacksonian Professor at Cambridge, who, not having conveniences for making experiments with it, in the manner he could wish, was so good as to give it to me.

The apparatus is very simple; it consists of a wooden arm, 6 feet long, made so as to unite great strength with little weight. This arm is suspended in an horizontal position, by a slender wire 40 inches long, and to each extremity is hung a leaden ball, about 2 inches in diameter; and the whole is inclosed in a narrow wooden case, to defend it from the wind.

As no more force is required to make this arm turn round on its centre, than what is necessary to twist the suspending wire, it is plain, that if the wire is sufficiently slender, the most minute force, such as the attraction of a leaden weight a few inches in diameter, will be sufficient to draw the arm sensibly aside. The weights which Mr. Michell intended to use were 8 inches diameter. One of these was to be placed on one side the case, opposite to one of the balls, and as near it as could conveniently be done, and the other on the other side, opposite to the other ball, so that the attraction of both these weights would conspire in drawing the arm aside; and, when its position, as affected by these weights, was ascertained, the weights were to be removed to the other side of the case, so as to draw the arm the contrary way, and the position of the arm was to be again determined; and, consequently, half the difference of these positions would shew how much the arm was drawn aside by the attraction of the weights.

In order to determine from hence the density of the earth, it is necessary to ascertain what force is required to draw the arm aside through a given space. This Mr. Michell intended to do, by putting the arm in motion, and observing the time of its vibrations, from which it may easily be computed[1].

Mr. Michell had prepared two wooden stands, on which the leaden weights were to be supported, and pushed forwards, till they came almost in contact with the case; but he seems to have intended to move them by hand.

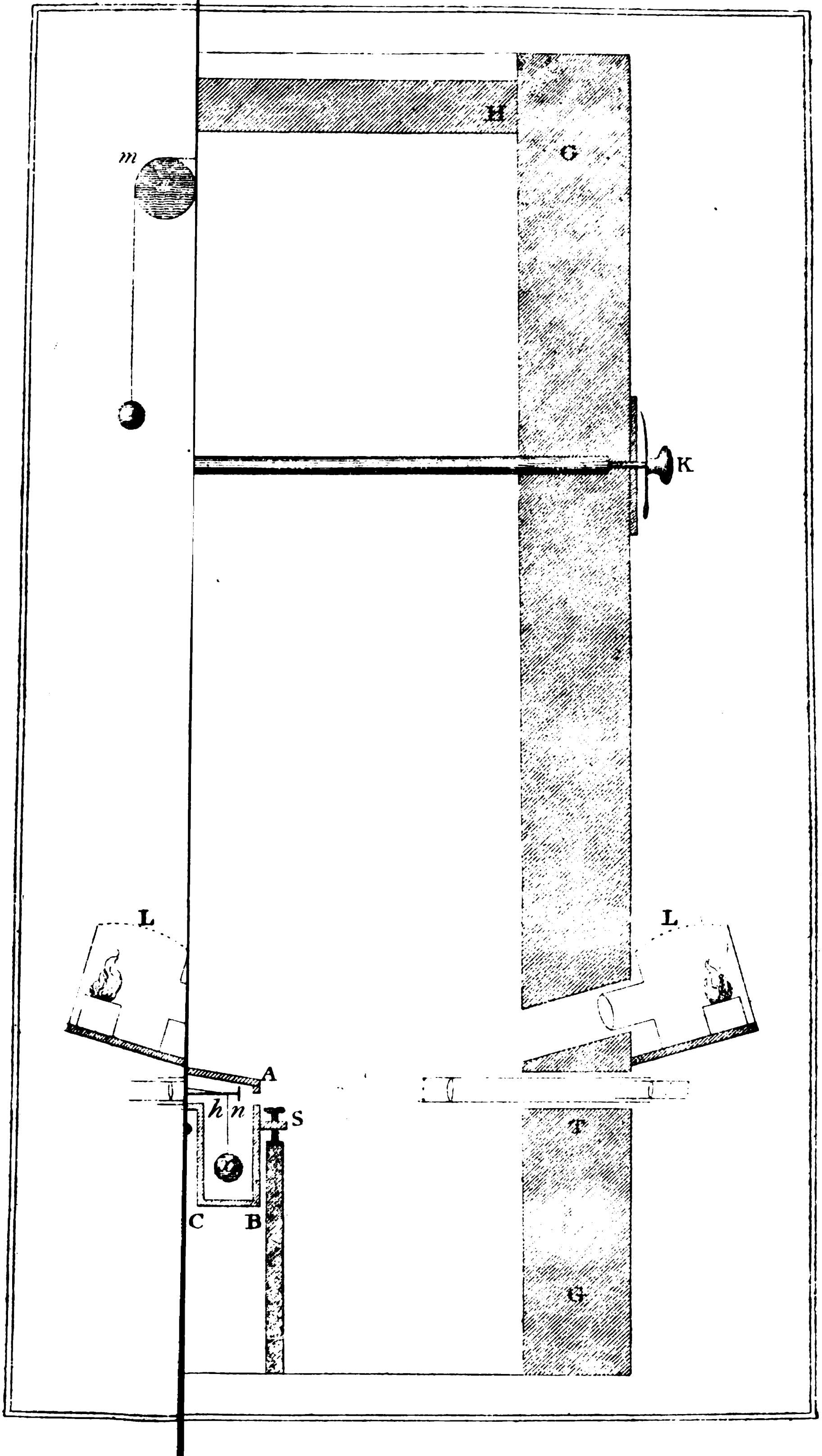
As the force with which the balls are attracted by these weights is excessively minute, not more than $\frac{1}{50,000,000}$ of their weight, it is plain, that a very minute disturbing force will be sufficient to destroy the success of the experiment; and, from the following experiments it will appear, that the disturbing force most difficult to guard against, is that arising from the variations of heat and cold; for, if one side of the case is warmer than the other, the air in contact with it will be rarefied, and, in consequence, will ascend, while that on the other side will descend, and produce a current which will draw the arm sensibly aside[2].

As I was convinced of the necessity of guarding against this source of error, I resolved to place the apparatus in a room which should remain constantly shut, and to observe the motion of the arm from without, by means of a telescope; and to suspend the leaden weights in such manner, that I could move them without entering into the room. This difference in the manner of observing, rendered it necessary to make some alteration in Mr. Michell's apparatus; and, as there were some parts of it which I thought not so convenient as could be wished, I chose to make the greatest part of it afresh.

Fig. 1. (Tab. XXIII.) is a longitudinal vertical section through the instrument, and the building in which it is placed. *ABCDDCBAEFFE*, is the case; *x* and *x* are the two balls, which are suspended by the wires *hx* from the arm *ghmh*, which is itself suspended by the slender wire *gl*. This arm consists of a slender deal rod *hmh*, strengthened by a silver wire

[1] Mr. Coulomb has, in a variety of cases, used a contrivance of this kind for trying small attractions; but Mr. Michell informed me of his intention of making this experiment, and of the method he intended to use, before the publication of any of Mr. Coulomb's experiments.

[2] M. Cassini, in observing the variation compass placed by him in the Observatory, (which was constructed so as to make very minute changes of position visible, and in which the needle was suspended by a silk thread,) found that standing near the box, in order to observe, drew the needle sensibly aside; which I have no doubt was caused by this current of air. It must be observed, that his compass-box was of metal, which transmits heat faster than wood, and also was many inches deep; both which causes served to increase the current of air. To diminish the effect of this current, it is by all means advisable to make the box, in which the needle plays, not much deeper than is necessary to prevent the needle from striking against the top and bottom.


H
G
m
K
L
L
A
h
n
S
T
C
B
G

hgh; by which means it is made strong enough to support the balls, though very light[1].

The case is supported, and set horizontal, by four screws, resting on posts fixed firmly into the ground: two of them are represented in the figure, by *S* and *S*; the two others are not represented, to avoid confusion. *GG* and *GG* are the end walls of the building. *W* and *W* are the leaden weights; which are suspended by the copper rods *RrPrR*, and the wooden bar *rr*, from the centre pin *Pp*. This pin passes through a hole in the beam *HH*, perpendicularly over the centre of the instrument, and turns round in it, being prevented from falling by the plate *p*. *MM* is a pulley, fastened to this pin; and *Mm*, a cord wound round the pulley, and passing through the end wall; by which the observer may turn it round, and thereby move the weights from one situation to the other.

Fig. 2. (Tab. XXIV.) is a plan of the instrument. *AAAA* is the case. *SSSS*, the four screws for supporting it. *hh*, the arm and balls. *W* and *W*, the weights. *MM*, the pulley for moving them. When the weights are in this position, both conspire in drawing the arm in the direction *hW*; but, when they are removed to the situation *w* and *w*, represented by the dotted lines, both conspire in drawing the arm in the contrary direction *hw*. These weights are prevented from striking the instrument, by pieces of wood, which stop them as soon as they come within $\frac{1}{8}$ of an inch of the case. The pieces of wood are fastened to the wall of the building; and I find, that the weights may strike against them with considerable force, without sensibly shaking the instrument.

In order to determine the situation of the arm, slips of ivory are placed within the case, as near to each end of the arm as can be done without danger of touching it, and are divided to 20ths of an inch. Another small slip of ivory is placed at each end of the arm, serving as a vernier, and subdividing these divisions into 5 parts; so that the position of the arm may be observed with ease to 100ths of an inch, and may be estimated to less. These divisions are viewed, by means of the short telescopes *T* and *T*, (fig. 1.) through slits cut in the end of the case, and stopped with glass; they are enlightened by the lamps *L* and *L*, with convex glasses, placed so as to throw the light on the divisions; no other light being admitted into the room.

The divisions on the slips of ivory run in the direction *Ww*, (fig. 2.) so that, when the weights are placed in the positions *w* and *w*, represented by the dotted circles, the arm is drawn aside, in such direction as to make the

[1] Mr. Michell's rod was entirely of wood, and was much stronger and stiffer than this, though not much heavier; but, as it had warped when it came to me, I chose to make another, and preferred this form, partly as being easier to construct and meeting with less resistance from the air, and partly because, from its being of a less complicated form, I could more easily compute how much it was attracted by the weights.

index point to a higher number on the slips of ivory; for which reason, I call this the positive position of the weights.

FK, (fig. 1.) is a wooden rod, which, by means of an endless screw, turns round the support to which the wire *gl* is fastened, and thereby enables the observer to turn round the wire, till the arm settles in the middle of the case, without danger of touching either side. The wire *gl* is fastened to its support at top, and to the centre of the arm at bottom, by brass clips, in which it is pinched by screws.

In these two figures, the different parts are drawn nearly in the proper proportion to each other, and on a scale of one to thirteen.

Before I proceed to the account of the experiments, it will be proper to say something of the manner of observing. Suppose the arm to be at rest, and its position to be observed, let the weights be then moved, the arm will not only be drawn aside thereby, but it will be made to vibrate, and its vibrations will continue a great while; so that, in order to determine how much the arm is drawn aside, it is necessary to observe the extreme points of the vibrations, and from thence to determine the point which it would rest at if its motion was destroyed, or the point of rest, as I shall call it. To do this, I observe three successive extreme points of a vibration, and take the mean between the first and third of these points, as the extreme point of vibration in one direction, and then assume the mean between this and the second extreme, as the point of rest; for, as the vibrations are continually diminishing, it is evident, that the mean between two extreme points will not give the true point of rest.

It may be thought more exact, to observe many extreme points of vibration, so as to find the point of rest by different sets of three extremes, and to take the mean result; but it must be observed, that notwithstanding the pains taken to prevent any disturbing force, the arm will seldom remain perfectly at rest for an hour together; for which reason, it is best to determine the point of rest, from observations made as soon after the motion of the weights as possible.

The next thing to be determined is the time of vibration, which I find in this manner: I observe the two extreme points of a vibration, and also the times at which the arm arrives at two given divisions between these extremes, taking care, as well as I can guess, that these divisions shall be on different sides of the middle point, and not very far from it. I then compute the middle point of the vibration, and, by proportion, find the time at which the arm comes to this middle point. I then, after a number of vibrations, repeat this operation, and divide the interval of time, between the coming of the arm to these two middle points, by the number of vibrations, which gives the time of one vibration. The following example will explain what is here said more clearly. [See p. 253.]

The first column contains the extreme points of the vibrations. The second, the intermediate divisions. The third, the time at which the arm

Extreme points.	Division.	Time.	Point of rest.	Time of middle of vibration.
27,2		h. ′ ″		h. ′ ″
	25	10 23 4	—	10 23 23
	24	57		
22,1	—	—	24,6	
27	—	—	24,7	
22,6	—	—	24,75	
26,8	—	—	24,8	
23	—	—	24,85	
26,6	—	—	24,9	
	25	11 5 22	—	11 5 22
	24	6 48		
23,4				

came to these divisions; and the fourth, the point of rest, which is thus found: the mean between the first and third extreme points is 27,1, and the mean between this and the second extreme point is 24,6, which is the point of rest, as found by the three first extremes. In like manner, the point of rest found by the second, third, and 4th extremes, is 24,7, and so on. The fifth column is the time at which the arm came to the middle point of the vibration, which is thus found: the mean between 27,2 and 22,1 is 24,65, and is the middle point of the first vibration; and, as the arm came to 25 at $10^h\ 23'\ 4''$, and to 24 at $10^h\ 23'\ 57''$, we find, by proportion, that it came to 24,65 at $10^h\ 23'\ 23''$. In like manner, the arm came to the middle of the seventh vibration at $11^h\ 5'\ 22''$; and, therefore, six vibrations were performed in $41'\ 59''$, or one vibration in $7'\ 0''$.

To judge of the propriety of this method, we must consider in what manner the vibration is affected by the resistance of the air, and by the motion of the point of rest.

Let the arm, during the first vibration, move from D to B, (Tab. XXIV. fig. 3.) and, during the second, from B to d; Bd being less than DB, on account of the resistance. Bisect DB in M, and Bd in m, and bisect Mm in n, and let x be any point in the vibration; then, if the resistance is proportional to the square of the velocity, the whole time of a vibration is verylittle altered; but, if T is taken to be the time of one vibration, as the diameter of a circle to its semicircumference, the time of moving from B to n exceeds ½ a vibration, by $\frac{T \times Dd}{8Bn}$ nearly; and the time of moving from B to m falls short of ½ a vibration, by as much; and the time of moving from B to x, in the second vibration, exceeds that of moving from x to B, in the first, by $\frac{T \times Dd \times Bx^2}{4Bn^2 \times \sqrt{Bx \times x\delta}}$, supposing Dd to be bisected in δ; so that, if a mean is taken, between the time of the first arrival of the arm at x and its returning back to the same point, this mean will be earlier than the true time of its coming to B, by

$$\frac{T \times Dd \times Bx^2}{8Bn^2 \times \sqrt{Bx} \times x\delta}.$$

The effect of motion in the point of rest is, that when the arm is moving in the same direction as the point of rest, the time of moving from one extreme point of vibration to the other is increased, and it is diminished when they are moving in contrary directions; but, if the point of rest moves uniformly, the time of moving from one extreme to the middle point of the vibration, will be equal to that of moving from the middle point to the other extreme, and moreover, the time of two successive vibrations will be very little altered; and, therefore, the time of moving from the middle point of one vibration to the middle point of the next, will also be very little altered.

It appears, therefore, that on account of the resistance of the air, the time at which the arm comes to the middle point of the vibration, is not exactly the mean between the times of its coming to the extreme points, which causes some inaccuracy in my method of finding the time of a vibration. It must be observed, however, that as the time of coming to the middle point is before the middle of the vibration, both in the first and last vibration, and in general is nearly equally so, the error produced from this cause must be inconsiderable; and, on the whole, I see no method of finding the time of a vibration which is liable to less objection.

The time of a vibration may be determined, either by previous trials, or it may be done at each experiment, by ascertaining the time of the vibrations which the arm is actually put into by the motion of the weights; but there is one advantage in the latter method, namely, that if there should be any accidental attraction, such as electricity, in the glass plates through which the motion of the arm is seen, which should increase the force necessary to draw the arm aside, it would also diminish the time of vibration; and, consequently, the error in the result would be much less, when the force required to draw the arm aside was deduced from experiments made at the time, than when it was taken from previous experiments.

Account of the Experiments.

In my first experiments, the wire by which the arm was suspended was $39\frac{1}{4}$ inches long, and was of copper silvered, one foot of which weighed $2\frac{4}{10}$ grains: its stiffness was such, as to make the arm perform a vibration in about 15 minutes. I immediately found, indeed, that it was not stiff enough, as the attraction of the weights drew the balls so much aside, as to make them touch the sides of the case; I, however, chose to make some experiments with it, before I changed it.

In this trial, the rods by which the leaden weights were suspended were of iron; for, as I had taken care that there should be nothing magnetical in the arm, it seemed of no signification whether the rods were magnetical or not; but, for greater security, I took off the leaden weights, and tried what effect the rods would have by themselves. Now I find, by computation, that the attraction of gravity of these rods on the balls, is to that of the weights, nearly as 17 to 2500; so that, as the attraction of the weights appeared, by the foregoing trial, to be sufficient to draw the arm aside by about 15 divisions, the attraction of the rods alone should draw it aside about $\frac{1}{10}$ of a division; and, therefore, the motion of the rods from one near position to the other, should move it about $\frac{1}{5}$ of a division.

The result of the experiment was, that for the first 15 minutes after the rods were removed from one near position to the other, very little motion was produced in the arm, and hardly more than ought to be produced by the action of gravity; but the motion then increased, so that, in about a quarter or half an hour more, it was found to have moved $\frac{1}{2}$ or $1\frac{1}{2}$ division, in the same direction that it ought to have done by the action of gravity. On returning the irons back to their former position, the arm moved backward, in the same manner that it before moved forward.

It must be observed, that the motion of the arm, in these experiments, was hardly more than would sometimes take place without any apparent cause; but yet, as in three experiments which were made with these rods, the motion was constantly of the same kind, though differing in quantity from $\frac{1}{2}$ to $1\frac{1}{2}$ division, there seems great reason to think that it was produced by the rods.

As this effect seemed to me to be owing to magnetism, though it was not such as I should have expected from that cause, I changed the iron rods for copper, and tried them as before; the result was, that there still seemed to be some effect of the same kind, but more irregular, so that I attributed it to some accidental cause, and therefore hung on the leaden weights, and proceeded with the experiments.

It must be observed, that the effect which seemed to be produced by moving the iron rods from one near position to the other, was, at a medium, not more than one division; whereas the effect produced by moving the weight from the midway to the near position, was about 15 divisions; so that, if I had continued to use the iron rods, the error in the result caused thereby, could hardly have exceeded $\frac{1}{30}$ of the whole. [See Tables on p. 256 *et seq.*]

It must be observed, that in this experiment, the attraction of the weights drew the arm from 11,5 to 25,8, so that, if no contrivance had been used to prevent it, the momentum acquired thereby would have carried it to near 40, and would, therefore, have made the balls to strike against the case. To prevent this, after the arm had moved near 15 divisions, I returned the weights to the midway position, and let them remain there, till the arm came nearly to the extent of its vibration, and then again moved them to the positive position, whereby the vibrations were so much

EXPERIMENT I. Aug. 5.

Weights in midway position.

Extreme points.	Divisions.	Time. h. ′ ″	Point of rest.	Time of mid. of vibration. h. ′ ″	Difference. ′ ″
	11,4	9 42 0			
	11,5	55 0			
	11,5	10 5 0	11,5		

At 10^h 5′, weights moved to positive position.

23,4					
27,6	—	—	25,82		
24,7	—	—	26,07		
27,3	—	—	26,1		
25,1	—	—			

At 11^h 6′, weights returned back to midway position.

5,					
	11	0 0 48	—	0 1 13	
	12	1 30			
18,2	—	—	12	—	14 56
	12	16 29	—	16 9	
	11	17 20			
6,6	—	—	11,92	—	14 36
	11	30 24	—	30 45	
	12	31 11			
16,3	—	—	11,72	—	15 13
	12	45 58	—	45 58	
	11	47 4			
7,7					

Motion on moving from midway to pos. = 14,32
pos. to midway = 14,1
Time of one vibration = 14′ 55″

diminished, that the balls did not touch the sides; and it was this which prevented my observing the first extremity of the vibration. A like method was used, when the weights were returned to the midway position, and in the two following experiments.

The vibrations, in moving the weights from the midway to the positive position, were so small, that it was thought not worth while to observe the time of the vibration. When the weights were returned to the midway position, I determined the time of the arm's coming to the middle point of each vibration, in order to see how nearly the times of the different vibrations agreed together. In great part of the following experiments, I contented myself with observing the time of its coming to the middle point of only the first and last vibration.

Experiment II. Aug. 6.

Weights in midway position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
	11	10 4 0			
	11	11 0			
	11	17 0			
	11	25 0	11,		
		Weights moved to positive position.			
29,3					
24,1	—	—	26,87		
30	—	—	27,57		
26,2	—	—	28,02		
29,7	—	—	28,12		
26,9	—	—	28,05		
28,7	—	—	27,85		
27,1	—	—	27,82		
28,4	—	—			
		Weights returned to midway position.			
6					
	12	1 3 50	—	1 4 1	
	13	4 34			
18,5	—	—	12,37	—	14 52
	13	18 29	—	18 53	
	12	19 18			
6,5	—	—	11,67	—	14 46
	11	33 48	—	33 39	
	12	34 51			
15,2	—	—	11	—	13 46
	13	45 8	—	47 25	
	12	46 22			
7,1	—	—	10,75	—	15 25
	11	2 3 48	—	2 2 50	
	12	5 18			
13,6					

Motion of arm on moving weights from midway to pos. = 15,87

pos. to midway = 15,45

Time of one vibration = 14′ 42″

EXPERIMENT III. Aug. 7.

The weights being in the positive position, and the arm a little in motion.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
31,5					
29	—	—	30,12		
31	—	—	30,02		
29,1					
		Weights moved to midway position.			
9					
	14	10 34 18	—	10 34 55	
	15	35 8			
20,5	—	—	14,8	—	14 44
	15	49 31	—	49 39	
	14	50 27			
9,2	—	—	14,07	—	14 38
	14	11 5 7	—	11 4 17	
	15	6 18			
17,4	—	—	13,52	—	14 47
	14	11 18 46	—	11 19 4	
	13	19 58			
10,1	—	—	13,3	—	14 27
	13	33 46	—	33 31	
	14	35 26			
15,6					
		Weights moved to positive position.			
32					
	28	0 2 48	—	0 2 59	
	27	3 56			
23,7	—	—	27,8		
31,8	—	—	28,27		
25,8	—	—	28,62		
	27	44 58	—	47 40	
	28	46 50			
31,1					

Motion of the arm on moving weights from pos. to mid. = 15,22
mid. to pos. = 14,5

Time of one vibration, when in mid. position = 14′ 39″
pos. position = 14 54

These experiments are sufficient to shew, that the attraction of the weights on the balls is very sensible, and are also sufficiently regular to determine the quantity of this attraction pretty nearly, as the extreme results do not differ from each other by more than $\frac{1}{10}$ part. But there is a circumstance in them, the reason of which does not readily appear, namely, that the effect of the attraction seems to increase, for half an hour, or an hour, after the motion of the weights; as it may be observed, that in all three experiments, the mean position kept increasing for that time, after moving the weights to the positive position; and kept decreasing, after moving them from the positive to the midway position.

The first cause which occurred to me was, that possibly there might be a want of elasticity, either in the suspending wire, or something it was fastened to, which might make it yield more to a given pressure, after a long continuance of that pressure, than it did at first.

To put this to the trial, I moved the index so much, that the arm, if not prevented by the sides of the case, would have stood at about 50 divisions, so that, as it could not move farther than to 35 divisions, it was kept in a position 15 divisions distant from that which it would naturally have assumed from the stiffness of the wire; or, in other words, the wire was twisted 15 divisions. After having remained two or three hours in this position, the index was moved back, so as to leave the arm at liberty to assume its natural position.

It must be observed, that if a wire is twisted only a little more than its elasticity admits of, then, instead of setting, as it is called, or acquiring a permanent twist all at once, it sets gradually, and, when it is left at liberty, it gradually loses part of that set which it acquired; so that if, in this experiment, the wire, by having been kept twisted for two or three hours, had gradually yielded to this pressure, or had begun to set, it would gradually restore itself, when left at liberty, and the point of rest would gradually move backwards; but, though the experiment was twice repeated, I could not perceive any such effect.

The arm was next suspended by a stiffer wire.

EXPERIMENT IV. Aug. 12.

Weights in midway position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
	21,6	9 30 0			
	21,5	52 0			
	21,5	10 13 0	21,5		
Weights moved from midway to positive position.					
27,2					
22,1	—	—	24,6		

EXPERIMENT IV. Aug. 12 (*cont.*)

Extreme points.	Divisions.	Time. h. ′ ″	Point of rest.	Time of mid. of vibration. h. ′ ″	Difference. ′ ″
27	—	—	24,67		
22,6	—	—	24,75		
26,8	—	—	24,8		
23,0	—	—	24,85		
26,6	—	—	24,9		
23,4					
Weights moved to negative position.					
15					
	17	19 25	—	10 20 31	
	19	20 41			
22,4	—	—	18,72	—	7 0
	20	26 45	—	27 31	
	19	27 22			
15,1	—	—	18,52	—	6 57
	19	35 1	—	34 28	
	20	48			
21,5	—	—	18,35	—	7 23
	20	40 23	—	41 51	
	19	41 18			
15,3	—	—	18,22	—	6 48
	18	48 36	—	48 39	
	19	49 24			
20,8	—	—	18,1	—	6 58
	19	54 45	—	55 37	
	18	55 45			
15,5					
Weights moved to positive position.					
31,3					
	25	11 10 25	—	11 10 40	
	23	11 3			
17,1	—	—	24,02	—	7 3
	22	17 6	—	17 43	
	23	26			
30,6	—	—	24,17	—	7 1
	25	24 33	—	24 44	
	23	25 17			
18,4	—	—	24,32	—	7 5
	23	31 21	—	31 49	
	25	32 9			
29,9	—	—	24,4	—	6 59
	25	38 39	—	38 48	
	23	39 31			

EXPERIMENT IV. Aug. 12 (*cont.*)

Extreme points.	Divisions.	Time. h. ′ ″	Point of rest.	Time of mid. of vibration. h. ′ ″	Difference. ′ ″
19,4	—	—	24,5	—	7 6
	23	45 16	—	45 54	
	25	46 12			
29,3					

Motion of arm on moving weights from midway to pos. = 3,1
pos. to neg. = 6,18
neg. to pos. = 5,92

Time of one vibration in neg. position = 7′ 1″
pos. position = 7 3

EXPERIMENT V. Aug. 20.

The weights being in the positive position, the arm was made to vibrate, by moving the index.

29,6					
21,1	—	—	25,2		
29,	—	—	25,17		
21,6					

Weights moved to negative position.

22,6					
	20	10 22 47	—	10 23 11	
	19	23 30			
16,3	—	—	19,27		
21,9	—	—	19,15		
16,5	—	—	19,1		
21,5	—	—	19,07		
16,8	—	—	19,07		
21,2	—	—	19,07		
17,1	—	—	19,05		
20,8	—	—	19,02		
17,4	—	—	19,05		
20,6	—	—	19,02		
	20	11 32 16	—	11 33 53	
	19	33 58			
17,5	—	—	18,97		
					7 13
	19	41 16	—	41 6	
	20	43 0			
20,3					

Weights moved to positive position.

20,2					
	24	11 49 10	—	11 49 37	
	26	50 19			

EXPERIMENT V. Aug. 20 (*cont.*)

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
29,4	—	—	24,95	—	7 7
	26	56 15	—	56 44	
	25	47			
20,8	—	—	24,92		
28,7	—	—	24,87		
21,3	—	—	24,85		
28,1	—	—	24,75		
21,5	—	—	24,67		
27,6	—	—	24,67		
22	—	—	24,7		
	24	0 45 48	—	0 46 21	
	25	46 43			
27,2	—	—	24,7	—	7 1
	25	53 11	—	53 22	
	24	54 9			
22,4					

Motion of arm on moving weights from pos. to neg. = 5,9
neg. to pos. = 5,98
Time of one vibration, when weights are in neg. position = 7′ 5″
pos. position = 7 5

In the fourth experiment, the effect of the weights seemed to increase on standing, in all three motions of the weights, conformably to what was observed with the former wire; but, in the last experiment, the case was different; for though, on moving the weights from positive to negative, the effect seemed to increase on standing, yet, on moving them from negative to positive, it diminished.

My next trials were, to see whether this effect was owing to magnetism. Now, as it happened, the case in which the arm was inclosed, was placed nearly parallel to the magnetic east and west, and therefore, if there was any thing magnetic in the balls and weights, the balls would acquire polarity from the earth; and the weights also, after having remained some time, either in the positive or negative position, would acquire polarity in the same direction, and would attract the balls; but, when the weights were moved to the contrary position, that pole which before pointed to the north, would point to the south, and would repel the ball it was approached to; but yet, as repelling one ball towards the south has the same effect on the arm as attracting the other towards the north, this would have no effect on the position of the arm. After some time, however, the poles of the weight would be reversed, and would begin to attract the balls, and would therefore produce the same kind of effect as was actually observed.

To try whether this was the case, I detached the weights from the upper part of the copper rods by which they were suspended, but still retained the lower joint, namely, that which passed through them; I then fixed them in their positive position, in such manner, that they could turn round on this joint, as a vertical axis. I also made an apparatus, by which I could turn them half way round, on these vertical axes, without opening the door of the room.

Having suffered the apparatus to remain in this manner for a day, I next morning observed the arm, and, having found it to be stationary, turned the weights half way round on their axes, but could not perceive any motion in the arm. Having suffered the weights to remain in this position for about an hour, I turned them back into their former position, but without its having any effect on the arm. This experiment was repeated on two other days, with the same result.

We may be sure, therefore, that the effect in question could not be produced by magnetism in the weights; for, if it was, turning them half round on their axes, would immediately have changed their magnetic attraction into repulsion, and have produced a motion in the arm.

As a further proof of this, I took off the leaden weights, and in their room placed two 10-inch magnets; the apparatus for turning them round being left as it was, and the magnets being placed horizontal, and pointing to the balls, and with their north poles turned to the north; but I could not find that any alteration was produced in the place of the arm, by turning them half round; which not only confirms the deduction drawn from the former experiment, but also seems to shew, that in the experiments with the iron rods, the effect produced could not be owing to magnetism.

The next thing which suggested itself to me was, that possibly the effect might be owing to a difference of temperature between the weights and the case; for it is evident, that if the weights were much warmer than the case, they would warm that side which was next to them, and produce a current of air, which would make the balls approach nearer to the weights. Though I thought it not likely that there should be sufficient difference, between the heat of the weights and case, to have any sensible effect, and though it seemed improbable that, in all the foregoing experiments, the weights should happen to be warmer than the case, I resolved to examine into it, and for this purpose removed the apparatus used in the last experiments, and supported the weights by the copper rods, as before; and, having placed them in the midway position, I put a lamp under each, and placed a thermometer with its ball close to the outside of the case, near that part which one of the weights approached to in its positive position, and in such manner that I could distinguish the divisions by the telescope. Having done this, I shut the door, and some time after moved the weights to the positive position. At first, the arm was drawn aside only in its usual manner; but, in half an hour, the effect was so much increased, that the arm was drawn 14 divisions aside, instead of about three, as it would otherwise have been, and the thermometer was raised near 1°½; namely, from 61° to 62°½. On opening the door, the weights were found to be no more heated, than just to prevent their feeling cool to my fingers.

As the effect of a difference of temperature appeared to be so great, I bored a small hole in one of the weights, about three-quarters of an inch deep, and inserted the ball of a small thermometer, and then covered up the opening with cement. Another small thermometer was placed with its ball close to the case, and as near to that part to which the weight was approached as could be done with safety; the thermometers being so placed, that when the weights were in the negative position, both could be seen through one of the telescopes, by means of light reflected from a concave mirror.

EXPERIMENT VI. Sept. 6.

Weights in midway position.

Extreme points.	Divisions.	Time. h. ′	Point of rest.	Thermometer In air.	Thermometer In weight.
	18,9	9 43	—	55,5	
	18,85	10 3	18,85		
Weights moved to negative position.					
13,1	—	10 12	—	55,5	55,8
18,4	—	18	15 82		
13,4	—	25			
missed					
13,6	—	39	—	55,5	55,8
17,6	—	46	15,65		
13,8	—	53	15,65		
17,4	—	11 0	15,65		
14,0	—	7	15,65		
17,2	—	14	—	55,5	
Weights moved to positive position.					
25,8	—	23			
17,5	—	30	21,55		
25,4	—	37	21,6		
18,1	—	44	21,65		
25,0	—	51			
missed					
24,7	—	0 5			
19,	—	12	21,77		
24,4	—	19			

Motion of arm on moving weights from midway to − = 3,03

− to + = 5,9

EXPERIMENT VII. Sept. 18.

Weights in midway position.

Extreme points.	Divisions.	Time.	Point of rest.	Thermometer In air.	Thermometer In weight.
		h. ′			
	19,4	8 30	—	56,7	
	19,4	9 32	—	56,6	
Weights moved to negative position.					
13,6	—	40	—	—	57,2
18,8	—	47	16,25		
13,8	—	54			
Eight extreme points missed.					
16,9	—	10 58			
14,5	—	11 5	15,62		
16,6	—	12			
Weights moved to positive position.					
26,4	—	20	—	56,5	
17,2	—	28	21,72		
26,1	—	35			
Four extreme points missed.					
19,3	—	0 10			
25,1	—	17	22,3		
19,7	—	24			

Motion of arm on moving weights from midway to − = 3,15
− to + = 6,1

EXPERIMENT VIII. Sept. 23.

Weights in midway position.

	19,3	9 46	—	53,1	
	19,2	10 45	19,2	53,1	
Weights moved to negative position.					
13,5	—	56	—	—	53,6
18,6	—	11 3	16,07		
13,6	—	10			
Four extreme points missed.					
17,4	—	44			
14,1	—	51	15,7		
17,2	—	58	—	—	53,6
Weights moved to positive position.					
15,7	—	0 1			
26,7	—	8	21,42		
16,6	—	15	—	53,15	

EXPERIMENT VIII. Sept. 23 (*cont.*)

Two extreme points missed.

Extreme points.	Divisions.	Time.	Point of rest.	Thermometer In air.	Thermometer In weight.
		h. ′			
25,9	—	36			
18,1	—	43	21,9		
25,5	—	50			

Motion of arm on moving weights from midway to − = 3,13
− to + = 5,72

In these three experiments, the effect of the weight appeared to increase from two to five tenths of a division, on standing an hour; and the thermometers shewed, that the weights were three or five tenths of a degree warmer than the air close to the case. In the two last experiments, I put a lamp into the room, over night, in hopes of making the air warmer than the weights, but without effect, as the heat of the weights exceeded that of the air more in these two experiments than in the former.

On the evening of October 17, the weights being placed in the midway position, lamps were put under them, in order to warm them; the door was then shut, and the lamps suffered to burn out. The next morning it was found, on moving the weights to the negative position, that they were $7°\frac{1}{2}$ warmer than the air near the case. After they had continued an hour in that position, they were found to have cooled $1°\frac{1}{2}$, so as to be only 6° warmer than the air. They were then moved to the positive position; and in both positions the arm was drawn aside about four divisions more, after the weights had remained an hour in that position, than it was at first.

May 22, 1798. The experiment was repeated in the same manner, except that the lamps were made so as to burn only a short time, and only two hours were suffered to elapse before the weights were moved. The weights were now found to be scarcely 2° warmer than the case; and the arm was drawn aside about two divisions more, after the weights had remained an hour in the position they were moved to, than it was at first.

On May 23, the experiment was tried in the same manner, except that the weights were cooled by laying ice on them; the ice being confined in its place by tin plates, which, on moving the weights, fell to the ground, so as not to be in the way. On moving the weights to the negative position, they were found to be about 8° colder than the air, and their effect on the arm seemed now to diminish on standing, instead of increasing, as it did before: as the arm was drawn aside about $2\frac{1}{2}$ divisions less, at the end of an hour after the motion of the weights, than it was at first.

It seems sufficiently proved, therefore, that the effect in question is produced, as above explained, by the difference of temperature between the weights and case; for, in the 6th, 8th, and 9th experiments, in which the weights were not much warmer than the case, their effect increased but little on standing; whereas, it increased much, when they were much warmer than the case, and decreased much, when they were much cooler.

It must be observed, that in this apparatus, the box in which the balls play is pretty deep, and the balls hang near the bottom of it, which makes the effect of the current of air more sensible than it would otherwise be, and is a defect which I intend to rectify in some future experiments.

EXPERIMENT IX. April 29.

Weights in positive position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibrations.
		h. ′ ″		h. ′ ″
34,7				
35	—	—	34,84	
34,65				
Weights moved to negative position.				
23,8				
	28	11 18 29	—	11 18 43
	29	58		
33,2	—	—	28,52	
	29	25 27	—	25 40
	28	57		
23,9	—	—	28,25	
32	—	—	28,01	
24,15	—	—	27,82	
31	—	—	27,63	
24,4	—	—	27,55	
30,4	—	—	27,47	
	28	0 7 4	—	0 7 26
	27	53		
24,7				

Motion of arm = 6,32

Time of vibration = 6′ 58″

EXPERIMENT X. May 5.

Weights in positive position.

34,5				
33,5	—	—	33,97	
34,4				
Weights moved to negative position.				
22,3				
	28	10 43 42	—	10 43 36
	29	44 6		

EXPERIMENT X. May 5 (*cont.*)

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
33,2	—	—	27,82	—	7 0
	28	50 33	—	50 36	
	27	51 0			
22,6	—	—	27,72		
32,5	—	—	27,7		
23,2	—	—	27,58		
31,45	—	—	27,4		
23,5	—	—	27,28		
	27	11 25 20	—	11 25 24	
	28	58			
30,7	—	—	27,21	—	7 3
	28	32 0	—	32 27	
	27	32 40			
23,95	—	—	27,21	—	6 56
	27	39 19	—	39 23	
	28	40 2			
30,25					

Motion of arm = 6,15
Time of vibration = 6′ 59″

EXPERIMENT XI. May 6.

Weights in positive position.

34,9					
34,1	—	—	34,47		
34,8	—	—	34,49		
34,25					

Weights moved to negative position.

23,3					
	28	9 59 59	—	10 0 8	
	29	10 0 27			
33,3	—	—	28,42	7 5	
	29	6 52			
	27	7 51			
23,8	—	—	28,35		
32,5	—	—	28,3		
24,4					
missed					
24,8					
31,3	—	—	28,17		
	29	10 48 37	—	10 49 8	
	28	49 21			

EXPERIMENT XI. May 6 (*cont.*)

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
		h. ′ ″		h. ′ ″
25,3	—	—	28,2	
	28	56 8	—	56 13
	29	56 56		
30,9				

Motion of arm = 6,07
Time of vibration = 7′ 1″

In the three foregoing experiments, the index was purposely moved so that, before the beginning of the experiment, the balls rested as near the sides of the case as they could, without danger of touching it; for it must be observed, that when the arm is at 35, they begin to touch. In the two following experiments, the index was in its usual position.

EXPERIMENT XII. May 9.

Weights in negative position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
		h. ′ ″		h. ′ ″
	17,4	9 45 0		
	17,4	58 0		
	17,4	10 8 0		
	17,4	10 0	17,4	
28,85				
Weights moved to positive position.				
	24	10 20 50	—	10 20 59
	22	21 46		
18,4	—	—	23,49	
28,3	—	—	23,57	
19,3	—	—	23,67	
27,8	—	—	23,72	
20	—	—	23,8	
27,4	—	—	23,83	
	24	11 3 13	—	11 3 14
	23	54		
20,55	—	—	23,87	
	23	9 45	—	10 18
	24	10 28		
27				

Motion of arm = 6,09
Time of vibration = 7′ 3″

EXPERIMENT XIII. May 25.

Weights in negative position.

Extreme points.	Divisions.	Time. h. ′ ″	Point of rest.	Time of mid. of vibrations. h. ′ ″
16				
18,3	—	—	17,2	
16,2				
	Weights moved to positive position.			
29,6				
	25	10 22 22	—	10 22 56
	24	0 45		
17,4	—	—	23,32	
	23	29 59	—	30 3
	24	30 23		
28,9	—	—	23,4	
	24	36 58	—	37 7
	23	37 24		
18,4	—	—	23,52	
	23	10 44 3	—	10 44 14
	24	31		
28,4	—	—	23,62	
19,3	—	—	23,7	
27,8	—	—	23,7	
	24	11 5 26	—	11 5 31
	23	6 1		
19,9	—	—	23,72	
	23	12 12	—	12 35
	24	50		
27,3				
	Weights moved to negative position.			
13,5				
21,8	—	—	17,75	
	18	37 34	—	37 39
	17	38 10		
13,9	—	—	17,67	
	17	44 26	—	44 45
	18	45 4		
21,1	—	—	17,62	
14,4	—	—	17,6	
20,5	—	—	17,52	
14,7	—	—	17,47	
20	—	—	17,42	
	18	0 19 57	—	0 20 24
	17	20 52		

EXPERIMENT XIII. May 25 (*cont.*)

Extreme points.	Divisions.	Time. h. ′ ″	Point of rest.	Time of mid. of vibration. h. ′ ″
15	—	—	17,37	
	17	27 15	—	27 30
	18	28 15		
19,5				

Motion of the arm on moving weights from − to + = 6,12
+ to − = 5,97

Time of vibration at + = 7′ 6″
− = 7 7

EXPERIMENT XIV. May 26.

Weights in negative position.

Extreme points.	Divisions.	Time. h. ′ ″	Point of rest.	Time of mid. of vibration. h. ′ ″
	16,1	9 18 0		
	16,1	24 0		
	16,1	46 0		
	16,1	49 0	16,1	
Weights moved to positive position.				
27,7				
	23	10 0 46	—	10 1 1
	22	1 16		
17,3	—	—	22,37	
	22	7 58	—	8 5
	23	8 27		
27,2	—	—	22,5	
	23	15 2	—	15 9
	22	32		
18,3	—	—	22,65	
26,8	—	—	22,75	
19,1	—	—	22,85	
26,4	—	—	22,97	
	23	43 40	—	43 32
	22	44 22		
20	—	—	23,15	
	22	49 53	—	50 41
	23	50 37		
26,2				
Weights moved to negative position.				
12,4				
	16	11 7 53	—	11 8 25
	17	8 27		
21,5	—	—	17,02	
	17	15 30	—	15 27
	16	16 3		

EXPERIMENT XIV. May 26 (*cont.*)

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
		h. ′ ″		h. ′ ″
12,7	—	—	16,9	
20,7	—	—	16,85	
13,3	—	—	16,82	
20	—	—	16,72	
13,6	—	—	16,67	
	16	11 50 33	—	11 50 58
	17	51 19		
19,5	—	—	16,65	
	17	57 53	—	58 6
	16	58 44		
14				

Motion of arm by moving weights from − to + = 6,27
+ to − = 6,13
Time vibration at + = 7′ 6″
− = 7 6

In the next experiment, the balls, before the motion of the weights, were made to rest as near as possible to the sides of the case, but on the contrary side from what they did in the 9th, 10th and 11th experiments.

EXPERIMENT XV. May 27.

Weights in negative position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
		h. ′ ″		h. ′ ″
3,9				
3,35	—	—	3,61	
3,85	—	—	3,61	
3,4				
Weights moved to positive position.				
15,4				
	10	10 5 59	—	10 5 56
	9	6 27		
4,8	—	—	9,95	
	9	12 43	—	13 5
	10	13 11		
14,8	—	—	10,07	
	10	20 24	—	20 13
	9	56		
5,9	—	—	10,23	
14,35	—	—	10,35	
6,8	—	—	10,46	

EXPERIMENT XV. May 27 (*cont.*)

Extreme points.	Divisions.	Time. h. ′ ″	Point of rest.	Time of mid. of vibration. h. ′ ″
13,9	—	—	10,52	
	11	48 30	—	48 42
	10	49 11		
7,5	—	—	10,6	
	10	55 26	—	55 48
	11	56 10		
13,5				

Motion of the arm = 6,34
Time of vibration = 7′ 7″

The two following experiments were made by Mr. Gilpin, who was so good as to assist me on the occasion.

EXPERIMENT XVI. May 28.

Weights in negative position.

22,55				
8,4	—	—	15,09	
21	—	—	14,9	
9,2				

Weights moved to positive position.

26,6				
	22	10 22 53	—	10 23 15
	21	23 20		
15,8	—	—	21	
	20	30 7	—	30 30
	21	36		
25,8	—	—	21,05	
	22	37 23	—	37 45
	21	55		
16,8	—	—	21,11	
	20	44 29	—	45 1
	21	45 4		
25,05	—	—	21,11	
	22	51 54	—	52 20
	21	52 32		
17,57	—	—	21,2	
	21	59 31	—	59 34
	22	11 0 13		
24,6	—	—	21,28	
	22	6 24	—	11 6 49
	21	7 9		
18,3				

Motion of the arm = 6,1
Time of vibration = 7′ 16″

EXPERIMENT XVII. May 30.

Weights in negative position.

Extreme points.	Divisions.	Time. h. ′ ″	Point of rest.	Time of mid. of vibration. h. ′ ″
	17,2	10 19 0		
	17,1	25 0		
	17,07	29 0		
	17,15	40 0		
	17,45	49 0		
	17,42	51 0		
	17,42	11 1 0	17,42	

Weights moved to positive position.

Extreme points.	Divisions.	Time. h. ′ ″	Point of rest.	Time of mid. of vibration. h. ′ ″
28,8				
	24 23	11 11 23 49	—	11 11 37
18,1	—	—	23,2	
	22 23	18 13 43	—	18 42
27,8	—	—	23,12	
	24 23	25 19 49	—	25 40
18,8	—	—	23,2	
	23 24	32 41 33 13	—	32 43
27,38	—	—	23,31	
	24 23	39 28 40 3	—	39 44
19,7	—	—	23,44	
	23 24	46 33 47 11	—	46 46
27	—	—	23,52	
	24 23	53 36 54 17	—	53 48
20,4	—	—	23,57	
	23 24	0 0 34 1 18	—	0 0 55
26,5	—	—	23,55	
	24 23	7 34 8 21	—	7 50
20,8	—	—	23,59	
	23 24	14 30 15 24	—	14 58
26,25				

EXPERIMENT XVII. May 30 (*cont.*)

Weights moved to negative position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibrations.
		h. ′ ″		h. ′ ″
13,3				
	17	0 32 19	—	0 32 44
	18	48		
22,4	—	—	17,95	
	18	39 46	—	39 44
	17	40 19		
13,7	—	—	17,85	
	17	46 26	—	46 48
	18	47 0		
21,6	—	—	17,72	
	18	53 43	—	53 50
	17	54 20		
14	—	—	17,6	
	17	1 0 39	—	1 0 55
	18	1 20		
20,8	—	—	17,47	
	18	7 39	—	7 59
	17	8 21		
14,3	—	—	17,37	
	17	14 54	—	15 4
	18	15 42		
20,1	—	—	17,27	
	18	21 32	—	22 5
	17	22 22		
14,6				

Motion of the arm on moving weights from – to + = 5,78
+ to – = 5,64

Time of vibration at + = 7′ 2″
– = 7 3

On the Method of computing the Density of the Earth from these Experiments.

I shall first compute this, on the supposition that the arm and copper rods have no weight, and that the weights exert no sensible attraction, except on the nearest ball; and shall then examine what corrections are necessary, on account of the arm and rods, and some other small causes.

The first thing is, to find the force required to draw the arm aside, which, as was before said, is to be determined by the time of a vibration.

The distance of the centres of the two balls from each other is 73,3 inches, and therefore the distance of each from the centre of motion is 36,65, and the length of a pendulum vibrating seconds, in this climate, is 39,14; therefore, if the stiffness of the wire by which the arm is suspended is such, that the force which must be applied to each ball, in order to draw the arm aside by the angle A, is to the weight of that ball as the arch of A to the radius, the arm will vibrate in the same time as a pendulum whose length is 36,65 inches, that is, in $\sqrt{\frac{36,65}{39,14}}$ seconds; and therefore, if the stiffness of the wire is such as to make it vibrate in N seconds, the force which must be applied to each ball, in order to draw it aside by the angle A, is to the weight of the ball as the arch of $A \times \frac{1}{N^2} \times \frac{36,65}{39,14}$ to the radius. But the ivory scale at the end of the arm is 38,3 inches from the centre of motion, and each division is $\frac{1}{20}$ of an inch, and therefore subtends an angle at the centre, whose arch is $\frac{1}{766}$; and therefore the force which must be applied to each ball, to draw the arm aside by one division, is to the weight of the ball as $\frac{1}{766N^2} \frac{36,65}{39,14}$ to 1, or as $\frac{1}{818N^2}$ to 1.

The next thing is, to find the proportion which the attraction of the weight on the ball bears to that of the earth thereon, supposing the ball to be placed in the middle of the case, that is, to be not nearer to one side than the other. When the weights are approached to the balls, their centres are 8,85 inches from the middle line of the case; but, through inadvertence, the distance, from each other, of the rods which support these weights, was made equal to the distance of the centres of the balls from each other, whereas it ought to have been somewhat greater. In consequence of this, the centres of the weights are not exactly opposite to those of the balls, when they are approached together; and the effect of the weights, in drawing the arm aside, is less than it would otherwise have been, in the triplicate ratio of $\frac{8,85}{36,65}$ to the chord of the angle whose sine is $\frac{8,85}{36,65}$, or in the triplicate ratio of the cosine of $\frac{1}{2}$ this angle to the radius, or in the ratio of ,9779 to 1.

Each of the weights weighs 2439000 grains, and therefore is equal in weight to 10,64 spherical feet of water; and therefore its attraction on a particle placed at the centre of the ball, is to the attraction of a spherical foot of water on an equal particle placed on its surface, as

$$10,64 \times ,9779 \times \left(\frac{6}{8,85}\right)^2 \text{ to } 1.$$

The mean diameter of the earth is 41800000 feet[1]; and therefore, if the mean

[1] In strictness, we ought, instead of the mean diameter of the earth, to take the diameter of that sphere whose attraction is equal to the force of gravity in this climate; but the difference is not worth regarding.

density of the earth is to that of water as D to one, the attraction of the leaden weight on the ball will be to that of the earth thereon, as

$$10{,}64 \times {,}9779 \times \left(\frac{6}{8{,}85}\right)^2 \text{ to } 41800000D :: 1 \text{ to } 8739000D.$$

It is shewn, therefore, that the force which must be applied to each ball, in order to draw the arm one division out of its natural position, is $\frac{1}{818N^2}$ of the weight of the ball; and, if the mean density of the earth is to that of water as D to 1, the attraction of the weight on the ball is $\frac{1}{8739000D}$ of the weight of that ball; and therefore the attraction will be able to draw the arm out of its natural position by $\frac{818N^2}{8739000D}$ or $\frac{N^2}{10683D}$ divisions; and therefore, if on moving the weights from the midway to a near position the arm is found to move B divisions, or if it moves $2B$ divisions on moving the weights from one near position to the other, it follows that the density of the earth, or D, is $\frac{N^2}{10683B}$.

We must now consider the corrections which must be applied to this result; first, for the effect which the resistance of the arm to motion has on the time of the vibration: 2d, for the attraction of the weights on the arm: 3d, for their attraction on the farther ball: 4th, for the attraction of the copper rods on the balls and arm: 5th, for the attraction of the case on the balls and arm: and 6th, for the alteration of the attraction of the weights on the balls, according to the position of the arm, and the effect which that has on the time of vibration. None of these corrections, indeed, except the last, are of much signification, but they ought not entirely to be neglected.

As to the first, it must be considered, that during the vibrations of the arm and balls, part of the force is spent in accelerating the arm; and therefore, in order to find the force required to draw them out of their natural position, we must find the proportion which the forces spent in accelerating the arm and balls bear to each other.

Let *EDCedc* (fig. 4) be the arm. *B* and *b* the balls. *Cs* the suspending wire. The arm consists of 4 parts; first, a deal rod *Dcd*, 73,3 inches long; 2d, the silver wire *DCd*, weighing 170 grains; 3d, the end pieces *DE* and *ed*, to which the ivory vernier is fastened, each of which weighs 45 grains; and 4th, some brass work *Cc*, at the centre. The deal rod, when dry, weighs 2320 grains, but when very damp, as it commonly was during the experiments, weighs 2400; the transverse section is of the shape represented in fig. 5; the thickness *BA*, and the dimensions of the part *DEed*, being the same in all parts; but the breadth *Bb* diminishes gradually, from the middle to the ends. The area of this section is ,33 of a square inch at the middle, and ,146 at the end; and therefore, if any point x (fig. 4.) is taken in cd, and $\frac{cx}{cd}$ is called x, this rod weighs $\frac{2400 \times ,33}{73,3 \times ,238}$ per inch at the middle; $\frac{2400 \times ,146}{73,3 \times ,238}$ at the end, and $\frac{2400}{73,3} \times \frac{,33 - ,184x}{,238} = \frac{3320 - 1848x}{73,3}$ at x; and therefore, as the weight of the wire is $\frac{170}{73,3}$ per inch, the deal rod and wire together may be considered as a rod whose weight at x

$$= \frac{3490 - 1848x}{73,3} \text{ per inch.}$$

But the force required to accelerate any quantity of matter placed at x, is proportional to x^2; that is, it is to the force required to accelerate the same quantity of matter placed at d as x^2 to 1; and therefore, if cd is called l, and x is supposed to flow, the fluxion of the force required to accelerate the deal rod and wire is proportional to $\frac{x^2 l\dot{x} \times 3490 - 1848x}{73,3}$, the fluent of which, generated while x flows from c to d, $= \frac{l}{73,3} \times \frac{3490}{3} - \frac{1848}{4} = 350$; so that the force required to accelerate each half of the deal rod and wire, is the same as is required to accelerate 350 grains placed at d.

The resistance to motion of each of the pieces de, is equal to that of 48 grains placed at d; as the distance of their centres of gravity from C is 38 inches. The resistance of the brass work at the centre may be disregarded; and therefore the whole force required to accelerate the arm, is the same as that required to accelerate 398 grains placed at each of the points D and d.

Each of the balls weighs 11262 grains, and they are placed at the same distance from the centre as D and d; and therefore, the force required to accelerate the balls and arm together, is the same as if each ball weighed 11660, and the arm had no weight; and therefore, supposing the time of a vibration to be given, the force required to draw the arm aside, is greater than if the arm had no weight, in the proportion of 11660 to 11262, or of 1,0353 to 1.

To find the attraction of the weights on the arm, through d draw the vertical plane dwb perpendicular to Dd, and let w be the centre of the weight, which, though not accurately in this plane, may, without sensible error, be considered as placed therein, and let b be the centre of the ball; then wb is horizontal and $= 8,85$, and db is vertical and $= 5,5$; let $wd = a$, $wb = b$, and let $\frac{dx}{dc}$, or $1 - x = z$; then the attraction of the weight on a particle of matter at x, in the direction dw, is to its attraction on the same particle placed at $b :: b^3 : (a^2 + z^2l^2)^{\frac{3}{2}}$, or is proportional to $\frac{b^3}{(a^2 + z^2l^2)^{\frac{3}{2}}}$, and the force of that attraction to move the arm, is proportional to $\frac{b^3 \times \overline{1-z}}{(a^2+z^2l^2)^{\frac{3}{2}}}$, and the weight of the deal rod and wire at the point x, was before said to be $\frac{3490-1848x}{73{,}3} = \frac{1642+1848z}{73{,}3}$ per inch; and therefore, if dx flows, the fluxion of the power to move the arm

$$= l\dot{z} \times \frac{1642+1848z}{73{,}3} + \frac{b^3 \times \overline{1-z}}{(a^2+z^2l^2)^{\frac{3}{2}}} = \dot{z} \times (821+924z) \times \frac{b^3 \times \overline{1-z}}{(a^2+l^2z^2)^{\frac{3}{2}}}$$

$$= \frac{b^3\dot{z} \times \overline{821+103z-924z^2}}{(a^2+l^2z^2)^{\frac{3}{2}}} = \frac{b^3z \times \overline{821+103z+\frac{924a^2}{l^2}}}{(a^2+l^2z^2)^{\frac{3}{2}}} - \frac{924b^3\dot{z} \times \overline{\frac{a^2}{l^2}+z^2}}{(a^2+l^2z^2)^{\frac{3}{2}}};$$

which, as $\frac{a^2}{l^2} = {,}08 = \frac{b^3\dot{z} \times \overline{895+103z}}{(a^2+l^2z^2)^{\frac{3}{2}}} - \frac{924b^3z}{l^2\sqrt{a^2+l^2z^2}}$.

The fluent of this

$$= \frac{895b^3z}{a^2\sqrt{a^2+l^2z^2}} - \frac{103b^3}{l^2\sqrt{a^2+l^2z^2}} + \frac{103b^3}{l^2a} - \frac{924b^3}{l^3}\log.\frac{lz+\sqrt{a^2+l^2z^2}}{a},$$

and the force with which the attraction of the weight, on the nearest half of the deal rod and wire, tends to move the arm, is proportional to this fluent generated while z flows from 0 to 1, that is, to 128 grains.

The force with which the attraction of the weight on the end-piece *de* tends to move the arm, is proportional to $47 \times \frac{b^3}{a^3}$, or 29 grains; and therefore the whole power of the weight to move the arm, by means of its attraction on the nearest part thereof, is equal to its attraction on 157 grains placed at b, which is $\frac{157}{11262}$, or ,0139 of its attraction on the ball.

It must be observed, that the effect of the attraction of the weight on the whole arm is rather less than this, as its attraction on the farther half draws it the contrary way; but, as the attraction on this is small, in comparison of its attraction on the nearer half, it may be disregarded.

The attraction of the weight on the furthest ball, in the direction *bw*, is to its attraction on the nearest ball :: $wd^3 : wD^3$ :: ,0017 : 1; and therefore the effect of the attraction of the weight on both balls, is to that of its attraction on the nearest ball :: ,9983 : 1.

To find the attraction of the copper rod on the nearest ball, let b and w (fig. 6.) be the centres of the ball and weight, and *ea* the perpendicular part of the copper rod, which consists of two parts, *ad* and *de*. *ad* weighs 22000 grains, and is 16 inches long, and is nearly bisected by w. *de* weighs 41000, and is 46 inches long. *wb* is 8,85 inches, and is perpendicular to *ew*. Now, the attraction of a line *ew*, of uniform thickness, on b, in the direction *bw*, is to that of the same quantity of matter placed at w :: $bw : eb$; and therefore the attraction of the part da equals that of $\frac{22000 \times wb}{db}$, or 16300, placed at w; and the attraction of de equals that of

$$\overline{41000} \times \frac{\overline{ew}}{\overline{ed}} \times \frac{\overline{bw}}{\overline{be}} - \overline{41000} \times \frac{\overline{dw}}{\overline{ed}} \times \frac{\overline{bw}}{\overline{bd}},$$

or 2500, placed at the same point; so that the attraction of the perpendicular part of the copper rod on b, is to that of the weight thereon, as 18800 : 2439000, or as ,00771 to 1. As for the attraction of the inclined part of the rod and wooden bar, marked Pr and rr in fig. 1, it may safely be neglected, and so may the attraction of the whole rod on the arm and farthest ball; and therefore the attraction of the weight and copper rod, on the arm and both balls together, exceeds the attraction of the weight on the nearest ball, in the proportion of ,9983 + ,0139 + ,0077 to one, or of 1,0199 to 1.

The next thing to be considered, is the attraction of the mahogany case. Now it is evident, that when the arm stands at the middle division, the attractions of the opposite sides of the case balance each other, and have no power to draw the arm either way. When the arm is removed from this division, it is attracted a little towards the nearest side, so that the force required to draw the arm aside is rather less than it would otherwise be; but yet, if this force is proportional to the distance of the arm from the middle division, it makes no error in the result; for, though the attraction will draw the arm aside more than it would otherwise do, yet, as the accelerating force by which the arm is made to vibrate is diminished in the same proportion, the square of the time of a vibration will be increased in the same proportion as the space by which the arm is drawn aside, and therefore the result will be the same as if the case exerted no attraction; but, if the attraction of the case is not proportional to the distance of the arm from the middle point, the ratio in which the accelerating force is diminished is different in different parts of the vibration, and the square of the time of a vibration will not be increased in the same proportion as the quantity by which the arm is drawn aside, and therefore the result will be altered thereby.

On computation, I find that the force by which the attraction draws the arm from the centre is far from being proportional to the distance, but the whole force is so small as not to be worth regarding; for, in no position of the arm does the attraction of the case on the balls exceed that of $\frac{1}{8}$th of a spheric inch of water, placed at the distance of 1 inch from the centre of the balls; and the attraction of the leaden weight equals that of 10,6 spheric feet of water placed at 8,85 inches, or of 234 spheric inches placed at 1 inch distance; so that the attraction of the case on the balls can in no position of the arm exceed $\frac{1}{1170}$ of that of the weight. The computation is given in the Appendix.

It has been shewn, therefore, that the force required to draw the arm aside one division, is greater than it would be if the arm had no weight, in the ratio of 1,0353 to 1, and therefore $= \frac{1,0353}{818N^2}$ of the weight of the ball; and moreover, the attraction of the weight and copper rod on the arm and both balls together, exceeds the attraction of the weight on the nearest ball, in the ratio of 1,0199 to 1, and therefore $= \frac{1,0199}{8739000D}$ of the weight of the ball; consequently D is really equal to

$$\frac{818N^2}{1,0353} \times \frac{1,0199}{8739000B}, \text{ or } \frac{N^2}{10844B}, \text{ instead of } \frac{N^2}{10683B},$$

as by the former computation. It remains to be considered how much this is affected by the position of the arm.

Suppose the weights to be approached to the balls; let W (fig. 7.) be the centre of one of the weights; let M be the centre of the nearest ball at its mean position, as when the arm is at 20 divisions; let B be the point which it actually rests at; and let A be the point which it would rest at, if the weight was removed; consequently, AB is the space by which it is drawn aside by means of the attraction; and let $M\beta$ be the space by which it would be drawn aside, if the attraction on it was the same as when it is at M. But the attraction at B is greater than at M, in the proportion of $WM^2 : WB^2$; and therefore,

$$AB = M\beta \times \frac{WM^2}{WB^2} = M\beta \times 1 + \frac{2MB}{MW}, \text{ very nearly.}$$

Let now the weights be moved to the contrary near position, and let w be now the centre of the nearest weight, and b the point of rest of the centre of the ball; then

$$Ab = M\beta \times 1 + \frac{2Mb}{MW}, \text{ and } Bb = M\beta \times 2 + \frac{2Mb}{MW} + \frac{2MB}{MW} = 2M\beta \times 1 + \frac{Bb}{MW};$$

so that the whole motion Bb is greater than it would be if the attraction on the ball was the same in all places as it is at M, in the ratio of $1 + \frac{Bb}{MW}$ to one; and, therefore, does not depend sensibly on the place of the arm, in either position of the weights, but only on the quantity of its motion, by moving them.

This variation in the attraction of the weight, affects also the time of vibration; for, suppose the weights to be approached to the balls, let W be the centre of the nearest weight; let B and A represent the same things as before; and let x be the centre of the ball, at any point of its vibration; let AB represent the force with which the ball, when placed at B, is drawn towards A by the stiffness of the wire; then, as B is the point of rest, the attraction of the weight thereon will also equal AB; and, when the ball is at x, the force with which it is drawn towards A, by the stiffness of the wire, $= Ax$, and that with which it is drawn in the contrary direction, by the attraction, $= AB \times \frac{WB^2}{Wx^2}$; so that the actual force by which it is drawn towards A

$$= Ax - \frac{AB \times WB^2}{Wx^2} = AB + Bx - AB \times \overline{1 + \frac{2Bx}{WB}} = Bx - \frac{2Bx \times AB}{WB},$$

very nearly. So that the actual force with which the ball is drawn towards the middle point of the vibration, is less than it would be if the weights were removed, in the ratio of $1 - \frac{2AB}{WB}$ to one, and the square of the time of a vibration is increased in the ratio of 1 to $1 - \frac{2AB}{WB}$; which differs very little from that of $1 + \frac{Bb}{MW}$ to 1, which is the ratio in which the motion of the arm, by moving the weights from one near position to the other, is increased.

The motion of the ball answering to one division of the arm $= \frac{36{,}35}{20 \times 38{,}3}$; and, if mB is the motion of the ball answering to d divisions on the arm, $\frac{MB}{WM} = \frac{36{,}35d}{20 \times 38{,}3 \times 8{,}85} = \frac{d}{185}$; and therefore, the time of vibration, and motion of the arm, must be corrected as follows:

If the time of vibration is determined by an experiment in which the weights are in the near position, and the motion of the arm, by moving the weights from the near to the midway position, is d divisions, the observed time must be diminished in the subduplicate ratio of $1 - \frac{2d}{185}$ to 1, that is, in the ratio of $1 - \frac{d}{185}$ to 1; but, when it is determined by an experiment in which the weights are in the midway position, no correction must be applied.

To correct the motion of the arm caused by moving the weights from a near to the midway position, or the reverse, observe how much the position of the arm differs from 20 divisions, when the weights are in the near position: let this be n divisions, then, if the arm at that time is on the same side of the division of 20 as the weight, the observed motion must be diminished by the $\frac{2n}{185}$ part of the whole; but, otherwise, it must be as much increased.

If the weights are moved from one near position to the other, and the motion of the arm is $2d$ divisions, the observed motion must be diminished by the $\frac{2d}{185}$ part of the whole.

If the weights are moved from one near position to the other, and the time of vibration is determined while the weights are in one of those positions, there is no need of correcting either the motion of the arm, or the time of vibration.

CONCLUSION

The following Table contains the Result of the Experiments.

Exper.	Mot. weight	Mot. arm.	Do. corr.	Time vib.	Do. corr.	Density.
1	m. to +	14,32	13,42		—	5,5
	+ to m.	14,1	13,17	14′,55″	—	5,61
2	m. to +	15,87	14,69	—	—	4,88
	+ to m.	15,45	14,14	14,42	—	5,07
3	+ to m.	15,22	13,56	14,39	—	5,26
	m. to +	14,5	13,28	14,54	—	5,55
4	m. to +	3,1	2,95		6,54	5,36
	+ to −	6,18	—	7,1	—	5,29
	− to +	5,92	—	7,3	—	5,58
5	+ to −	5,9	—	7,5	—	5,65
	− to +	5,98	—	7,5	—	5,57
6	m. to −	3,03	2,9	—	—	5,53
	− to +	5,9	5,71		—	5,62
7	m. to −	3,15	3,03	7,4		5,29
	− to +	6,1	5,9	by mean	6,57	5,44
8	m. to −	3,13	3,00		—	5,34
	− to +	5,72	5,54	—	—	5,79
9	+ to −	6,32	—	6,58	—	5,1
10	+ to −	6,15	—	6,59	—	5,27
11	+ to −	6,07	—	7,1	—	5,39
12	− to +	6,09	—	7,3	—	5,42
13	− to +	6,12	—	7,6	—	5,47
	+ to −	5,97	—	7,7	—	5,63
14	− to +	6,27	—	7,6	—	5,34
	+ to −	6,13	—	7,6	—	5,46
15	− to +	6,34	—	7,7	—	5,3
16	− to +	6,1	—	7,16	—	5,75
17	− to +	5,78	—	7,2	—	5,68
	+ to −	5,64	—	7,3	—	5,85

From this table it appears, that though the experiments agree pretty well together, yet the difference between them, both in the quantity of motion of the arm and in the time of vibration, is greater than can proceed merely from the error of observation. As to the difference in the motion of the arm, it may very well be accounted for, from the current of air produced by the difference of temperature; but, whether this can account for the difference in the time of vibration, is doubtful. If the current of air was regular, and of the same swiftness in all parts of the vibration of the ball, I think it could not; but, as there will most likely be much irregularity in the current, it may very likely be sufficient to account for the difference.

By a mean of the experiments made with the wire first used, the density of the earth comes out 5,48 times greater than that of water; and by a mean of those made with the latter wire, it comes out the same; and the extreme difference of the results of the 23 observations made with this wire, is only ,75; so that the extreme results do not differ from the mean by more than ,38, or $\frac{1}{14}$ of the whole, and therefore the density should seem to be determined hereby, to great exactness. It, indeed, may be objected, that as the result appears to be influenced by the current of air, or some other cause, the laws of which we are not well acquainted with, this cause may perhaps act always, or commonly, in the same direction, and thereby make a considerable error in the result. But yet, as the experiments were tried in various weathers, and with considerable variety in the difference of temperature of the weights and air, and with the arm resting at different distances from the sides of the case, it seems very unlikely that this cause should act so uniformly in the same way, as to make the error of the mean result nearly equal to the difference between this and the extreme; and, therefore, it seems very unlikely that the density of the earth should differ from 5,48 by so much as $\frac{1}{14}$ of the whole.

Another objection, perhaps, may be made to these experiments, namely, that it is uncertain whether, in these small distances, the force of gravity follows exactly the same law as in greater distances. There is no reason, however, to think that any irregularity of this kind takes place, until the bodies come within the action of what is called the attraction of cohesion, and which seems to extend only to very minute distances. With a view to see whether the result could be affected by this attraction, I made the 9th, 10th, 11th, and 15th experiments, in which the balls were made to rest as close to the sides of the case as they could; but there is no difference to be depended on, between the results under that circumstance, and when the balls are placed in any other part of the case.

According to the experiments made by Dr. Maskelyne, on the attraction of the hill Schehallien, the density of the earth is $4\frac{1}{2}$ times that of water; which differs rather more from the preceding determination than I should have expected. But I forbear entering into any consideration of which determination is most to be depended on, till I have examined more carefully how much the preceding determination is affected by irregularities whose quantity I cannot measure.

APPENDIX

On the Attraction of the Mahogany Case on the Balls.

The first thing is, to find the attraction of the rectangular plane $ck\beta b$ (fig. 8.) on the point a, placed in the line ac perpendicular to this plane.

Let $ac = a$, $ck = b$, $cb = x$, and let $\frac{a^2}{a^2 + x^2} = w^2$, and $\frac{b^2}{a^2 + x^2} = v^2$, then the attraction of the line $b\beta$ on a, in the direction ab, $= \frac{b\beta}{ab \times a\beta}$; and therefore, if cb flows, the fluxion of the attraction of the plane on the point a, in the direction cb,

$$= \frac{b\dot{x}}{\sqrt{a^2 + x^2} \times \sqrt{a^2 + b^2 + x^2}} \times \frac{x}{\sqrt{a^2 + x^2}} = \frac{-b\dot{w}}{w\sqrt{b^2 + \frac{a^2}{w^2}}} = \frac{-b\dot{w}}{\sqrt{b^2w^2 + a^2}} = \frac{-\dot{v}}{\sqrt{1 + v^2}},$$

the variable part of the fluent of which $= -\log. v + \sqrt{1 + v^2}$, and therefore the whole attraction $= \log. \left(\frac{ck + ak}{ac} \times \frac{ab}{b\beta + a\beta}\right)$; so that the attraction of the plane, in the direction cb, is found readily by logarithms, but I know no way of finding its attraction in the direction ac, except by an infinite series.

The two most convenient series I know, are the following:

First series. Let $\frac{b}{a} = \pi$, and let A = arc whose tang. is π,

$$B = A - \pi,\ C = B + \frac{\pi^3}{3},\ D = C - \frac{\pi^5}{5},\ \&c.$$

then the attraction in the direction ac

$$= \sqrt{1 - w^2} \times A + \frac{Bw^2}{2} + \frac{3Cw^4}{2.4} + \frac{3.5Dw^6}{2.4.6},\ \&c.$$

For the second series, let A = arc whose tang.

$$= \frac{1}{\pi},\ B = A - \frac{1}{\pi},\ C = B + \frac{1}{3\pi^3},\ D = C - \frac{1}{5\pi^5},\ \&c.$$

then the attraction

$$= \text{arc}.\ 90^\circ - \sqrt{(1 + v^2) \times A - \frac{Bv^2}{2} + \frac{3Cv^4}{2.4} - \frac{3.5Dv^6}{2.4.6}},\ \&c.$$

It must be observed, that the first series fails when π is greater than unity, and the second, when it is less; but, if b is taken equal to the least of the two lines ck and cb, there is no case in which one or the other of them may not be used conveniently.

By the help of these series, I computed the following table [p. 286].

Find in this table, with the argument $\frac{ck}{ak}$ at top, and the argument $\frac{cb}{ab}$ in the left-hand column, the corresponding logarithm; then add together this logarithm, the logarithm of $\frac{ck}{ak}$, and the logarithm of $\frac{cb}{ab}$; the sum is logarithm of the attraction.

	,1962	,3714	,5145	,6248	,7071	,7808	,8575	,9285	,9815	1,
,1962	,00001									
,3714	,00039	00148								
,5145	,00074	00277	00521							
,6248	00110	00406	00778	01183						
,7071	00140	00522	01008	01525	02002					
,7808	00171	00637	01245	01896	02405	03247				
,8575	00207	00772	01522	02339	03116	03964	05057			
,9285	00244	00910	01810	02807	03778	04867	06319	08119		
,9815	00271	01019	02084	03193	04368	05639	07478	09931	12849	
1,	00284	01054	02135	03347	04560	05975	07978	10789	14632	19612

To compute from hence the attraction of the case on the ball, let the box *DCBA*, (fig. 1.) in which the ball plays, be divided into two parts, by a vertical section, perpendicular to the length of the case, and passing through the centre of the ball; and, in fig. 9, let the parallelopiped *ABDEabde* be one of these parts, *ABDE* being the abovementioned vertical section; let x be the centre of the ball, and draw the parallelogram $\beta npm\delta x$ parallel to *BbdD*, and *xgrp* parallel to βBbn, and bisect $\beta\delta$ in c. Now, the dimensions of the box, on the inside, are Bb = 1,75; BD = 3,6; $B\beta$ = 1,75; and βA = 5; whence I find, that if xc and βx are taken as in the two upper lines of the following table, the attractions of the different parts are as set down below.

xc … …	,75	,5	,25
βx … …	1,05	1,3	1,55
Excess of attract. of *Ddrg* above *Bbrg* …	,2374	,1614	,0813
,, ,, *mdrp* above *nbrp* …	,2374	,1614	,0813
,, ,, *mesp* above *nasp* …	,3705	,2516	,1271
Sum of these …	,8453	,5744	,2897
Excess of attract. of $Bbn\beta$ above $Ddm\delta$ …	,5007	,3271	,1606
,, ,, $Aan\beta$ above $Eem\delta$ …	,4677	,3079	,1525
Whole attraction of the inside surface of the half box … … … … …	,1231	,0606	,0234

It appears, therefore, that the attraction of the box on x increases faster than in proportion to the distance xc.

The specific gravity of the wood used in this case is ,61, and its thickness is $\frac{3}{4}$ of an inch; and therefore, if the attraction of the outside surface of the box was the same as that of the inside, the whole attraction of the box on the ball, when cx = ,75, would be equal to $2 \times ,1231 \times ,61 \times \frac{3}{4}$ cubic inches, or ,201 spheric inches of water, placed at the distance of one inch from the centre of the ball. In reality, it can never be so great as this, as the attraction of the outside surface is rather less than that of the inside; and, moreover, the distance of x from c can never be quite so great as ,75 of an inch, as the greatest motion of the arm is only $1\frac{1}{2}$ inch.

PHILOSOPHICAL TRANSACTIONS. VOL. FOR 1809, page 221.

XIII. *On an Improvement in the Manner of dividing astronomical Instruments. By* Henry Cavendish, *Esq., F.R.S.*

THE great inconvenience and difficulty in the common method of dividing, arises from the danger of bruising the divisions by putting the point of the compass into them, and from the difficulty of placing that point mid-way, between two scratches very near together, without its slipping towards one of them; and it is this imperfection in the common process, which appears to have deterred Mr. Troughton from using it, and thereby gave rise to the ingenious method of dividing described in the preceding part of this volume [Troughton, *Phil. Trans.* 1809, 105]. This induced me to consider, whether the abovementioned inconvenience might not be removed, by using a beam compass with only one point, and a microscope instead of the other; and I find, that in the following manner of proceeding, we have no need of ever setting the point of the compass into a division, and consequently that the great objection to the old method of dividing is entirely removed.

In this method, it is necessary to have a convenient support for the beam compass: and the following seems to me to be as convenient as any. Let *CC* (Fig. 1.) be the circle to be divided, *BBB* a frame resting steadily on its face, and made to slide round on it with an adjusting motion to bring it to any required point: $d\delta$ is the beam compass, having a point near δ, and a microscope *m* made to slide from one end to the other. This beam compass is supported at *d*, in such manner as to turn round on this point as a center, without shake or tottering; and at the end δ it rests on another support, which can readily be lowered, so as either to let the point rest on the circle, or to prevent its touching it. It must be observed, however, that as the distance of *d* from the center of the circle must be varied, according to the magnitude of the arch to be divided, the piece on which *d* is supported had best be made to slide nearer to, or further from, the center; but the frame must be made to bear constantly against the edge of the circle to be divided, so that the distance of *d* from the center of this circle, shall not alter by sliding the frame.

This being premised, we will first consider the manner of dividing by continued bisection. Let F and f be two points on this limb which are to be bisected in ϕ. Take the distance of the microscope from the point nearly equal to the chord of $f\phi$, and place d so that the point and the axis of the microscope shall both be in the circle in which the divisions are to be cut. Then slide the frame BBB till the wire of the microscope bisects the point F; and having lowered the support at δ, make a faint scratch with the point.

Having done this, turn the beam compass round on the center d till the point comes to D, where it must rest on a support similar to that at δ; and having slid the frame till the wire of the microscope bisects the point f, make another faint scratch with the point, which, if the distance of the microscope from the point has been well taken, will be very near the former scratch; and the point mid-way between them will be the accurate bisection of the arch Ff; but it is unnecessary, and better not to attempt to place a point between these two scratches.

Having by these means determined the bisection at ϕ, we must bisect the arches $F\phi$ and $f\phi$ in just the same manner as before, except that the wire of the microscope must be made to bisect the interval between the two faint scratches, instead of bisecting a point.

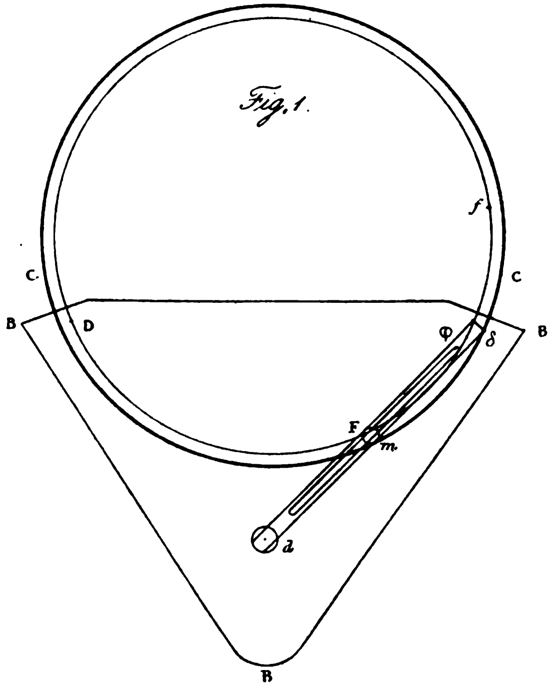
It must be observed that when the arch to be bisected is small, it will be necessary to use a bent point, as otherwise it could not be brought near enough to the axis of the microscope; and then part of the rays, which form the image of the object seen by the microscope, will be intercepted by the point; but I believe, that by proper management this may be done without either making the point too weak, or making the image indistinct; but if this cannot be done, we may have recourse to Mr. Troughton's expedient of bisecting an odd number of contiguous divisions.

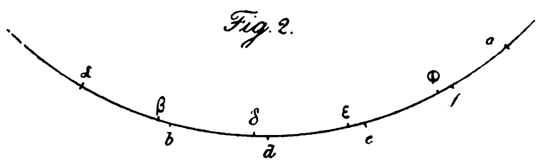
It must be observed too, that in the bisections of all the arches of the same magnitude, the position of the point d on the frame remains unaltered; but its position must be altered every time the magnitude of the arch is altered.

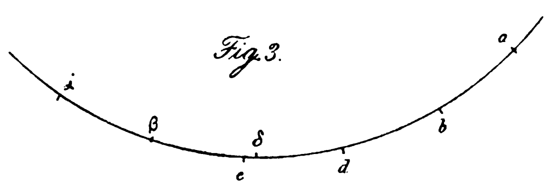
It is scarcely necessary to say, that the bisections thus made are not intended as the real divisions, but only as marks from which they are to be cut. In order to make the real divisions, the microscope must be placed near the point, and the support d must be placed so that $d\delta$ shall be a tangent to the circle at δ. The wire of the microscope must then be made to bisect one of these marks, and a point or division cut with the point, and the process continued till the divisions are all made.

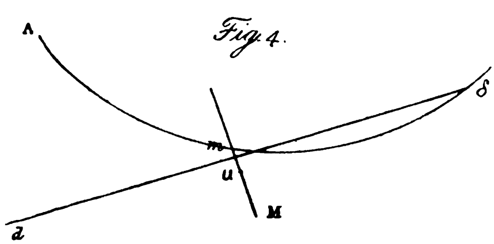
It is plain that in this way, without some further precaution, we must depend on the microscope not altering its position in respect of the point during the operation; for which reason I should prefer placing the axis of the microscope at exactly the same distance from the center of motion d, as the point; but removed from it sideways, by nearly the semi-diameter of the object glass; so that having made the division, we may move the beam compass till the division comes within the field of the microscope, and then see whether it is bisected by the wire, and consequently see whether the microscope has altered its place.

In the operation of bisection, as above described, it may be observed, that if the two scratches are placed so near together, that in making the second the point of the compass runs into the burr raised by the first, there seems to be some danger that the point may be a little deflected from its true course; though in Bird's account of his method, I do not find that he apprehends any inconvenience from it. One way of obviating this inconvenience, if it does exist, would be to set the beam compass not so exactly to the true length, as that one scratch should run into the burr of the other; but as this would make it more difficult to judge of the true point of bisection, perhaps it might be better to make one scratch extend from the circle towards the center, and the other from it.









It is clear, that the entire arc of a circle cannot be divided to degrees, without trisection and quinquesection; and I do not know whether our artists have recourse to this operation, or whether they avoid it by some contrivance similar to Bird's, namely, that of laying down an arch capable of continued bisection; but if the method of quinquesection is preferred, it may be performed by either of the three following methods:

First Method.

Let *aa* (Fig. 2) be the arch to be quinquesected. Open the beam compass to the chord of one fifth of this arch; bring the microscope to *a*, and with the point make the scratch *f*; then bring the microscope to *f*, and draw the scratch *e*; and in the same manner make the scratches *d* and *b*. Then turn the beam compass half round, and having brought the microscope to α, make the scratch β; and proceeding as before, make the scratches δ, ϵ and ϕ. Then the true position of the first quinquesection will be between *b* and β, distant from β by one fifth of $b\beta$; and the second will be distant from δ by two fifths of $d\delta$, and so on.

Then, in subdividing these arches, and striking the true divisions, the wire of the microscope, instead of bisecting the interval between the two scratches, must be brought four times nearer to β than to *b*. But in order to avoid the confusion which would otherwise proceed from this, it will be necessary to place marks on the limb opposite to all those divisions, in which the interval of the scratches is not to be bisected, shewing in what proportion they are to be divided; and these marks should be placed so as to be visible through the microscope, at the same time as the scratches. Perhaps, the best way of forming these marks, would be to make dots with the point of the beam compass contiguous to that scratch which the wire is to be nearest to, which may be done at the time the scratch is drawn.

Perhaps an experienced eye might be able to place the wire in the proper manner, between the two scratches, without further assistance; but the most accurate way would be to have a moveable wire with a micrometer, in the focus of the microscope, as well as a fixed one; and then having brought the fixed wire to *b*, bring the moveable one to β, and observe the distance of the two wires by the micrometer; then reduce the distance of the two wires to one fifth part of this, and move the frame till the moveable wire comes to β, and then the fixed wire will be in the proper position, that is four times nearer to β than to *b*.

It will be a great convenience, that the moveable wire should be made in such manner, as to be readily distinguished from the fixed, without the trouble of moving it.

In this manner of proceeding, I think a careful operator can hardly make any mistake: for if he makes any considerable error in the distance of the moveable wire from the fixed, it will be detected by the fixed wire not appearing in the right position, in respect of the two scratches; and as the mark is seen through the microscope, at the same time as the scratches, there is no danger of his mistaking which scratch it is to be nearest to, or at what distance it is to be placed from it.

To judge of the comparative accuracy of this method with that of bisection, it must be considered that the arches $\alpha\beta$, $\beta\delta$, &c. though made with the same opening of the compass, will not be exactly alike, owing partly to irregularities in the brass, and partly to other causes. Let us suppose, therefore, that in dividing the arch *aα* into five parts, the beam compass is opened to the exact length, but that from the above-mentioned irregularities the arches $\alpha\beta$, $\beta\delta$, $\delta\epsilon$, and $\epsilon\phi$ are all too long by the small quantity ϵ, and that the arches *af*, *fe*, *ed*, and *db* are all too short by the same quantity, which is the supposition the most unfavourable of any to the exactness of the operation; then the error in the position of $\beta = \epsilon$, and the point *b* errs 4ϵ in the same direction, and therefore the point assumed as the true point of quinquesection, will be at the distance of $\frac{3\epsilon}{5}$ from β, and the error in the position of this point $= \epsilon \times 1\frac{3}{5}$.

By the same way of reasoning, the error in the position of the point taken between *d* and $\delta = \epsilon \times 2\frac{2}{5}$.

In trisecting the error of each point $= \epsilon \times 1\frac{1}{3}$; and in bisecting, the error $= \epsilon$; and in quadrisecting, the error of the middle point $= 2\epsilon$.

It appears therefore that in trisecting, the greatest error we are liable to does not exceed that of bisection in a greater proportion than that of 4 to 3; but in quinquesecting the error of the two middle points is $2\frac{2}{5}$ times greater than in bisecting. It must be considered, however, that in the method of continued bisection, the two opposite points must be found by quadrisection; and the error of quinquesection exceeds that of quadrisection in no greater proportion than that of six to five; so that we may fairly say, that if we begin with quinquesection, this method of dividing is not greatly inferior, in point of accuracy, to that by continued bisection.

Second Method.

This differs from the foregoing, in placing dots or scratches in the true points of quinquesection and trisection, before we begin to subdivide. For this purpose, we must have a microscope placed as in page [287] first par. at the same distance from the center of motion as the point is; and this microscope must be furnished with a moveable wire and micrometer, as in page [290]; and then having first made the fixed wire of this microscope correspond exactly with the point, we must draw the scratches *b* and β, *d* and δ, &c. as before, and bring the fixed wire to the true point of quinquesection between *b* and β, in the manner directed in page [290], and with the point strike the scratch or dot; and if we please, we may, for further security, as soon as this is done, examine, by means of the moveable wire, whether this intermediate scratch or dot is well placed.

The advantage of this method is, that when this is done, we may subdivide and cut the true divisions, by making the wire of the microscope bisect the intermediate scratches, instead of being obliged to use the more troublesome operation of placing it in the proper proportion of distance between the two extremes.

This method certainly requires less attention than the former, and on the whole seems to be attended with considerably less trouble; but it is not quite so exact, as we are liable to the double error of placing the intermediate point and of subdividing from it.

As in this method the intermediate points are placed by means of the micrometer, there is no inconvenience in placing the extreme scratches b and β, &c. at such a distance from each other, that the intermediate one shall be in no danger of running into the bur raised by the extremes.

Third Method.

Let $a\alpha$ (Fig. 3) be the arch to be quinquesected; lay down the arches ab, bd, and de, as in the first method; then turn the beam compass half round, and lay down the arches $\alpha\beta$ and $\beta\delta$; then, without altering the frame, move the moveable wire of the microscope till it is four times nearer to δ than to e, and, having first rubbed out the former scratches, lay them down again with the compass thus altered; but as this method possesses not much, if any, advantage over the second, in point of ease, and is certainly inferior to it in exactness, it is not worth while saying anything further about it.

It was before said[1], that the center of motion of the beam compass is to be placed, so that the point and axis of the microscope shall both be in the circle in which the divisions are made; but it is necessary to consider this more accurately. Let $A\delta$ (Fig. 4) be the circle in which the scratches are to be made, δ the point of the beam compass, which we will suppose to be exactly in this circle, d the center on which it turns, and Mm the wire in the focus of the microscope, and let m be that point in which it is cut by the circle; and let us suppose that this point is not exactly in the line $d\delta$, then, when the beam compass is turned round, the circle will cut the wire in a different point μ, placed as much on one side of $d\delta$, as m is on the other, so that if the wire is not perpendicular to $d\delta$, the arch set off by the beam compass, after being turned round, will not be the same as before; but if it is perpendicular, there will be no difference; for which reason, care should be taken to make the wire exactly perpendicular to $d\delta$, which is easily examined by observing whether a point appears to run along it, while the beam compass is turned a little on its center. It is also necessary to take care that the point δ is in the arc of the circle, while the bisection

[1] Page 288.

is observed by the microscope, which may most conveniently be obtained, by placing a stop on the support on which that end of the beam compass rests. If proper care, however, is taken in placing the wire perpendicular, no great nicety is required either in this or in the position of *d*.

Another thing to be attended to, in making the wire bisect two scratches, is to take care that it bisects them in the part where they cut the circle; for as the wire is not perpendicular to the circle, except in very small arches, it is plain, that if it bisects the scratches at the circle, it will not bisect them at a distance from it.

There are many particulars in which my description of the apparatus to be employed will appear incomplete; but as there is nothing in it which seems attended with difficulty, I thought it best not to enter further into particulars, than was necessary to explain the principle, and to leave the rest to any artist who may choose to try it.

It is difficult to form a proper judgment of the conveniences or inconveniences of this method, without experience; but, as far as I can judge, it must have much advantage, both in point of accuracy and ease, over that of dividing by the common beam compasses; but it very likely may be thought that Mr. Troughton's method is better than either. Whether it is or is not, must be left for determination to experience and the judgment of artists. Thus much, however, may be observed, that this, as well as his, is free from the difficulty and inaccuracy of setting the point of a compass exactly in the center of a division. It also requires much less apparatus than his, and is free from any danger of error, from the slipping or irregularity in the motion of a roller; in which respect his method, notwithstanding the precautions used by him, is perhaps not entirely free from objection; and what with some artists may be thought a considerable advantage, it is free from the danger of mistakes in computing a table of errors, and in adjusting a sector according to the numbers of that table.

UNPUBLISHED PAPERS

FROM THE

ORIGINAL MANUSCRIPTS

IN THE POSSESSION OF

THE DUKE OF DEVONSHIRE,

K.G., LL.D., F.R.S.

WITH EXPLANATORY NOTES BY THE EDITOR

UNPUBLISHED PAPERS

AMONG the manuscripts preserved at Chatsworth not only are there the "minutes," as they are termed by Cavendish, of much of the experimental matter of his published work, arranged more or less systematically, and paged and indexed by himself, together with some of the rough drafts of certain of his memoirs and reports, with odds and ends of calculations, notes and memoranda, abstracts of foreign notices, and a few letters; but there are also a variety of papers relating to experimental inquiries, some of which must have required a considerable expenditure of time and labour, and although certain of these papers are put together as if for the press, or for perusal by a friend, they were, for reasons which are not apparent, withheld from publication. In a few cases these unpublished papers were obviously intended as sections of memoirs which were eventually communicated to the Royal Society and are printed in the *Philosophical Transactions*. In others, they are in the form of short notes on side-issues, details of experiments made to settle queries which occurred in the course of the main inquiry, but which even when settled, and in spite of their occasional interest and novelty, he refrained from publishing. The manuscripts deal with a wide range of subjects, and with many departments of physical science—mathematics, astronomy, geodesy, geology, mineralogy, chemistry, heat, electricity, terrestrial magnetism and meteorology. Some small portion of this material has already seen the light. The Rev. William Vernon Harcourt, who examined the papers in connection with the Water Controversy, printed one or two of them as a postscript to his address in 1839 as President of the British Association, and, as already stated, Professor Clerk Maxwell has sifted and published the notes of experimental work relating to electricity. Of the remainder, there is, of course, much that it is unnecessary to reproduce, in spite of its historical interest. Some of it is merely the detail of observations already incorporated in the published memoirs. Other portions are too fragmentary and detached, and their meaning is not always apparent. But there are certain of the papers, more especially those dealing with chemical and physical subjects which unquestionably are of value and interest, and should find a place in any account of Cavendish's work which aims at being reasonably complete. Accordingly when drawn up by Cavendish, as if for publication, they will be printed *in extenso* in what follows. When still in the form of "minutes" an abstract of their contents, as far as possible in the original words, will be given.

It is not possible in all cases to associate definite dates with the papers; this can only be surmised from such internal evidence as they afford. They will be dealt with in sections, and, as far as practicable, in what is presumed to be their chronological sequence.

EXPERIMENTS ON ARSENIC

The parcel of unpublished papers under this title contains: (1) notes of the details of the experiments in question; (2) a rough draft of a systematic account of them; (3) a fair copy of this draft. It is from the last-named that the account has now been printed. Apparently it was written out for the information of a friend, whose name is not mentioned, but who seems to have witnessed some of the experiments. From a date among the notes it would appear that the work was done in or about 1764. It consists of a study of the action of alkalis and acids upon arsenious oxide (As_4O_6); the preparation of properties of arsenic acid and of potassium and other arsenates, with "conjectures" concerning the nature of arsenic acid and its relations to arsenious oxide. In the outset, it was to a large extent a review of Macquer's work, published originally in the memoirs of the French Academy, but contains many original observations. It describes accurately the preparation and behaviour of solutions of potassium arsenite, obtained by dissolving arsenious oxide in a boiling solution of potassium carbonate. Cavendish found that "the greatest quantity of arsenic [arsenious oxide] which a solution of f. Alkali can retain properly dissolved, is about 2½ times of the dry alcaline salt contained in the solution." The theoretical ratio of potassium carbonate to arsenious oxide is 2·8. He accurately notes the action of the mineral acids on solutions of this salt.

He prepares what he terms "neutral arsenical salt" (potassium arsenate) by Macquer's method of heating a mixture of equal weights of nitre and arsenious oxide. If the deflagrated mass is dissolved in a proper quantity of hot water

> it readily shoots on cooling into crystals, which do not at all grow moist in the air, and require about 3½ times their weight of water to dissolve them. A solution of these crystals scarcely alters the colour of syrop of violets; if anything they give it a reddish cast; they turn tournsol paper of a brownish red.

This is the first accurate description of acid potassium arsenate (KH_2AsO_4). Cavendish points out that, strictly speaking, it is not a neutral salt, as Macquer supposed, since it dissolves, with effervescence, the carbonates of potassium and lime. He also correctly describes its behaviour with solutions of copper and iron salts. In preparing potassium arsenate by Macquer's process, he collected the red fumes evolved in a solution of pearl-ash, and incidentally obtained potassium nitrite, and notes its behaviour with the mineral acids, and with acetic acid. He assumes that the nitrous acid so formed is a modification of nitric acid "so much altered by this process as to have a less affinity to f. Alk. than the marine acid [hydrochloric acid] though not so small, I suppose, as distilled vinegar."

By heating strong oil of vitriol with arsenious oxide he appears to have obtained the compound described by Richter and analysed by Reich (*J. pr. Chem.* 90. 176) and subsequently examined by Mr R. H. Adie (*Chem. Soc. Journ.* 55. 1899, 157). Cavendish found that 1 part of arsenious oxide yielded 1·5 parts of "an irregular crystallised mass" after solution in strong oil of vitriol. Theoretically 1 part of arsenious oxide forms 1·4 parts of $As_4O_6 . (SO_3)_2$.

The action of strong nitric acid upon arsenious oxide is described at length, as the result was very different from that observed in the case of the other mineral acids. "The arsen. by being dissolved in this acid was found to have undergone the change necessary to enable it to form the neut. arsen. salt when united to f. Alk." In other words, it was transformed into arsenic acid which, when combined with potash, formed potassium arsenate.

The saturated solution yielded on evaporation crystals of nitre mixed with other crystals of a different shape which proved to be neut. arsen. salt. The success of this experiment induced me to try whether by dissolving arsen. [arsenious oxide] in aq. fort. and driving off the acid by heat, I could not procure the arsen. which had sufferd the above-mentioned change (or the arsenical acid, if you will allow me to call it by that name) by itself.

The experiment is described at length. 4 oz. of arsenious oxide were used: the "Caput mortuum" in the retort, after the excess of acid had been driven off, and the residue heated to "almost as great a heat...as the furnace would admit of" was found to "weigh 4 oz. 13 dwt. 6 gra. *id est* about $\frac{1}{8}$ part more than arsen. from which it was made." This increase corresponds almost exactly with the theoretical amount. "It attracted the moisture of the air though but slowly. It requires very little water to dissolve it; I believe scarcely more than $\frac{1}{2}$ its weight; but it does not dissolve fast without the assistance of heat." The substance was arsenic pentoxide, and its properties are accurately described. He proved that it contained no nitric acid, and that it yielded potassium hydrogen arsenate, identical with that formed by Macquer's process.

It also seems to possess all the properties of an acid (unless perhaps it should fail in respect of taste which I have not thought proper to try) since it effervesces with, and neutralizes the fixed and volatile alcalies and calcarious Earths and magnesia, and turns syrop of violets red and also unites to the Earth of alum.

The excess of the weight of the cap. mort. above that of the arsen. it was made from, must be owing, I suppose, to its retaining some of the matter of the aq. fort. used in making it.

How near he was to discovering what that particular matter was, is indicated by his next experiment in which he roasts an intimate mixture of arsenious oxide and potassium carbonate

in a broad shallow earthen pan, care being taken to keep it frequently stirrd. The heat was as great as the matter could bear without caking together. Some of it was taken out now and then, and dissolved in water, and tried with solut. silver; the colour of the precipitate formed thereby changed gradually the more the matter was Calcined, from a pale yellow [silver arsenite], which it was at 1st to a purplish red [silver arsenate] the same as that made by neut. arsen. salt [potassium arsenate].

The mass was then dissolved in water and carefully neutralised with hydrochloric acid.

It was then evap. There 1st shot some crystals resembling neut. arsen. salt, and afterwards some crystals of Sal Sylvii [potassium chloride]. Some of the crystals resembling neut. arsen. salt were dissolved in water: the solution efferv'd with whiting and f. alkali; reddened the colour of blue papers; made the same colourd precip. with solut. silver and blue vitr. as the neut. arsen. salt; in a word I could perceive no difference between that and the neut. arsen. salt made in the common manner.

He then theorises as to the rationale of the change, and as to the nature of the difference between arsenious oxide and arsenic oxide, and, like a true phlogistian, he is oblivious to the significance of the increase of weight he had found to occur when common or white arsenic passes into the "arsenical acid." He says:

I think these experiments shew pretty plainly that the only difference between plain arsenic and the arsenical acid is that the latter is more thoroughly deprived of its Phlogiston than the former. For all the ways I know of making arsenical acid or neut. arsen. salt are such as may reasonably be supposed to deprive the arsen. of its Phlogiston as, for example, in making arsen. acid by solution in aq. fort. the nitrous acid [nitric acid] is known to have a great disposition to lay hold of Phlogiston, and there are strong reasons for thinking that the dissolving of metallic substances in that acid is a very powerful method of depriving them of it, as I shall take notice of by and by.

It is not necessary to follow Cavendish into the maze in which he now enters. His reasoning is consistent, and from his point of view, ingenious and thoroughly sound. The paper is full of acute and accurate observations, many of which contain the germs of future discoveries as, for example, the evolution of chlorine by the action of strong hydrochloric acid on arsenic acid, and the formation of the brown coloured compound formed by the action of green vitriol upon solutions of nitrites etc.

Why Cavendish refrained from publishing these results can only be surmised. He must have known that they were largely original. Had he done so at or about the time they were obtained he would have anticipated Scheele, who is usually credited with the discovery of arsenic acid, by some nine or ten years. Scheele's well-known memoir, which appeared in 1775, contains a great number of observations on arsenic acid, but his method of procuring it was not so simple or direct as that discovered by Cavendish, which is the one now in use, and there is not that sense of quantitative accuracy in Scheele's work which seems to pervade all Cavendish's attempts.

EXPERIMENTS ON TARTAR

The account of these experiments is written, apparently for publication, on small sheets of paper ($6\frac{5}{8} \times 4\frac{1}{8}$ in.). There is no indication of the time at which they were performed, but they seem to have been made at two different periods as the description is divided into two sections entitled, respectively, "old experiments on tartar" and "new experiments on tartar." Although practically unpunctuated, and plentifully interspersed with contractions, the account is easily legible and its meaning is clear. Nothing is stated as to the object of the inquiry, but it eventually resolved itself into an attempt to determine the amount of alkali, respectively, in cream of tartar (potassium hydrogen tartrate, $C_4H_5O_6K$) and the more readily soluble normal potassium tartrate ($C_4H_4O_6K_2 . \frac{1}{2}H_2O$).

No reference is made to any prior workers on the chemical nature of tartar, but Cavendish was probably familiar with all that had been published on the subject up to his time, although the *Phil. Trans.*, even down to the end of the eighteenth century, contains no reference to tartar nor are any memoranda or notes from foreign literature to be found among his papers. Wine-lees or argol, the *tartarum*, or *tartarus* (Arabic *tartir*), of the iatro-chemists was originally considered to be an acid, and its solution was termed *aqua dissolvens*. It was known to the Romans that it yielded an alkali on incineration, but until late in the eighteenth century it was considered that this substance was formed during the burning, and was not a real constituent of tartar, in spite of the observations of Kunkel in 1677 and of Duhamel and Grosse in 1732. The existence of the alkali may be said to have been first definitely established by Marggraf in 1764, but the precise nature of the action of acids and alkalis upon tartar was not clearly understood, and it was apparently this matter that Cavendish set himself to elucidate. It was probably one of his earliest attempts at chemical inquiry, and may have been begun shortly after the publication of Marggraf's work.

The "old experiments on tartar" consisted of a study of the action of nitric and sulphuric acids on cream of tartar. In the case of nitric acid Cavendish recognised the formation of nitre, proving that the alkali present in tartar is identical with that in nitre. He further noticed that as the solution in nitric acid was gradually neutralised by lime, cream of tartar was again precipitated, but he failed to recognise the formation of calcium tartrate.

The action of sulphuric acid liberated tartaric acid which was isolated by evaporating the solution on the water-bath. The solution neutralised with lime caused a precipitate which was held to be selenite (calcium sulphate). This was repeatedly boiled with water and filtered; "the solution seemed by evaporation to consist of selenite, cr. tart and vitr. tart [potassium sulphate] but it was not examined accurately." The experiments with the two acids were repeated, but no additional observations of importance were noted. Although, as in all Cavendish's experiments, the work was quantitative, it is impossible to deduce any numerical results from the figures given. At this stage the work was temporarily abandoned. When it was resumed we have no means of knowing.

In 1769 Scheele communicated his memorable paper on tartaric acid to the Swedish Academy of Science, and an account of it subsequently appeared in Crell's *Chemical Journal*. This may have induced Cavendish to take up the inquiry again. On the other hand, he may have resumed it before the appearance of Scheele's memoir, and it may have been this latter circumstance that caused him to withhold his own paper from publication. But in any case Cavendish's "new experiments on tartar" furnished far more definite information than his previous ones. He began by studying the action of chalk upon a hot solution of cream of tartar. He notes the evolution of gas and is aware of the nature of the change, viz. that "soluble tartar" ($C_4H_4O_6K_2$) goes into the solution, and that "tartareous selenite" (calcium tartrate, $C_4H_4O_6Ca$) is precipitated, and can be separated by filtration.

To a portion of the solution of the "soluble tartar" he added some f. alkali which caused no precipitate: hence no lime was in the solution. To a second and known portion of the solution he added nitric acid so long as a precipitate was formed. This consisted of cream of tartar, "which being washed and dried weighed 231 grains." It can hardly be expected from the nature of the operations that the quantitative results would be of a high degree of accuracy, but it may be interesting to see how far the numbers given by Cavendish accord with theory. To begin with, he operated upon 2370 grains of cream of tartar. This would be decomposed by the chalk as follows:

$$\underset{\text{cream of tartar}}{2C_4H_5O_6K} + \underset{\text{chalk}}{CaCO_3} = \underset{\text{sol. tartar}}{C_4H_4O_6K_2} + \underset{\text{tart. selenite}}{C_4H_4O_6Ca} + H_2O + \underset{\text{fixed air}}{CO_2}$$

The action of the nitric acid on the "soluble tartar" is:

$$\underset{\text{sol. tartar}}{C_4H_4O_6K_2} + \underset{\text{aq. fort.}}{HNO_3} = \underset{\text{cream of tartar}}{C_4H_5O_6K} + \underset{\text{nitre}}{KNO_3}$$

In other words, 2 mols of the original cream of tartar would give 1 mol. of reprecipitated cream of tartar and 1 mol. of nitre. Now from $\frac{22}{100}$ of the solution of the "soluble tartar" Cavendish obtained 231 grains of cream of tartar by the addition of nitric acid and 126 grains of true nitre; that is, the amount of cream of tartar was 1·83 times that of the nitre; theoretically he should have obtained 260·7 grains of cream of tartar and 140 grains of nitre, or 1 86 times the weight of the nitre. Having regard to the conditions, imperfect decomposition, washing and separation, and the fact that the original cream of tartar might not be pure, the result is in fair accordance with the theory of the reactions as we now know them.

One half of the calcium tartrate ("tartareous selenite") was then triturated with dilute oil of vitriol. The precipitate evidently contained free chalk as the mixture effervesced on the addition of the acid. The liquid, which was very acid, was separated from the gypsum by filtration, evaporated to a small bulk, again filtered from a further sediment (of calcium sulphate) and further evaporated to a thin syrup when tartaric acid "shot on cooling." The crystals weighed approximately 245 grains. The mother liquor was again concentrated when a further crop of crystals was formed. These last were purified by re-solution, added to what remained of the mother liquor, and the tartaric acid in solution precipitated by salt of tartar (potassium carbonate). In all 73 grains of cream of tartar were thus obtained. The total weight of tartaric acid obtained from this portion of the tartareous selenite was thus about 303 grains as against a theoretical yield of 473 grains.

The 245 grains of tartaric acid were then converted by a solution of barilla (sodium carbonate) into the acid sodium tartrate, and into the neutral salt "which had a good deal of the mawkish bitter of Glauber's salt."

By triturating the tartareous selenite with a solution of volatile sal ammoniac (ammonium carbonate), "fixed air" (carbon dioxide) was evolved, and ammoniacal tartar (the neutral ammonium tartrate) passed into solution. The clear liquor on concentration crystallised "pretty readily" and the crystals of ammonium tartrate were found to be soluble in about twice their weight of water.

The ammonium tartrate was then subjected to destructive distillation, with a view, apparently, of preparing the *spiritus tartari* of Lully (pyruvic acid) but the distillate was not examined. Cavendish seems, however, to have noticed the formation of pyrotartaric acid, and tartaric anhydride, but although the behaviour of the salt on heating is accurately described, the account of the results is too vague to enable any certain conclusions to be drawn.

Cavendish then enters upon some calculations as to the distribution of the tartaric acid in the "soluble tartar" (normal potassium tartrate) and the "tartareous selenite" and the equivalent amounts of cream of tartar. He found that the "soluble tartar" ($C_4H_4O_6K_2$) obtained from 2370 grains of cream of tartar was capable of yielding rather more than 1050 grains of cream of tartar which, as it existed in solution, contains in addition rather more alkali than is contained in 573 grains of nitre. Cavendish thus clearly recognised that the "soluble tartar"—the normal potassium tartrate—contained considerably more alkali than cream of tartar, the acid potassium tartrate. 2370 grains of cream of tartar should furnish 1185 grains of cream of tartar by the action of nitric acid upon the normal salt: Cavendish found 1050. This amount (1050) to be converted into the normal salt he found would require as much alkali as is contained in 573 of nitre, that is 221 grains (K)—making in all 1271 grains: the quantity of the normal tartrate equivalent to 1050 grains of the acid tartrate is 1262 grains.

The calculations based upon the amount of cream of tartar derivable from the "tartareous selenite" are vitiated by the fact that the calcium tartrate was not homogeneous. But the general conclusion drawn is that the amount of alkali required to completely saturate tartaric acid, that is to convert it into the normal tartrate, is at least double that contained in cream of tartar.

Cavendish proved that tartaric acid contains no alkali as a normal constituent—a fact of importance in relation to the vague ideas then current as to the mutual relations of cream of tartar and tartaric acid. In fact, certain pharmacopœias confused the two substances as late as nearly the end of the eighteenth century. He further found that the whole of the tartaric acid in calcium tartrate could be completely "dislodged" by oil of vitriol.

He also noted the "stiff gluey" character of calcium tartrate when precipitated from alkaline solutions and speaks of the difficulty in filtering it, and of the fact that its solutions become turbid on warming, and that the salt is soluble in "sope leys" (potash solution).

The paper concludes with the details of calculations of the amount of alkali contained in cream of tartar or required to completely saturate it, and of the equivalent quantity of marble and pearl ash. The statement that the acid of 1 part of tartar is saturated by ·525 of whiting is substantially correct: the theoretical amount is ·531.

It will thus be seen that Cavendish's work on tartar was remarkably accurate, and was a notable contribution to the chemical history of the subject. Whether it was done independently of Scheele's work, or withheld from publication on the ground that he had been anticipated, it is impossible to say.

But even in the latter case its publication would have been of service as tending to clear up much that was vague concerning the nature of tartar, the production from it of tartaric acid, and the properties of the tartrates. But Cavendish was never in a hurry to publish his work: his interest in it was largely satisfied when he had satisfied the questioning of his own intelligence.

ON THE SOLUTION OF METALS IN ACIDS

Digression to paper on Inflammable Air.

[This "digression" was intended to be added to the section on Inflammable Air in Cavendish's paper on Factitious Air, published in 1766. For some reason it was omitted, possibly because, on reflection, he considered as, he says, that he had not "made sufficient experiments to speak quite positively as to this point," i.e. of the solution of metals in acids. The "digression," however, is interesting for several reasons. It serves to show what were Cavendish's views concerning the nature of the action of acids upon metals in general, and the reasons for the difference in their mutual behaviour. These views, as far as can be gathered, were consistently held by him to the last, at least to the extent that phlogiston was concerned in the phenomena. It will be noticed that the arguments, and to a great extent the language, are identical with those in the paper on "Arsenic," which seems additional evidence that Cavendish's experiments on that substance were made prior to 1766 and that therefore he anticipated Scheele in the discovery of free arsenic acid by 9 or 10 years.]

If it is not digressing too much I should be glad to make some observations concerning the solution of metals in acids. There seems to be only the 3 above-mentioned metallic substances which dissolves easily in spt. of salt or the diluted vitriolic acid. I have not indeed made sufficient experiments to speak quite positively as to this point, but I will relate what I have tried relating to copper, which is always looked upon as one of the most soluble of the metallic substances. Some clean copper wire seemed not at all acted on by oil of vitriol diluted with an equal weight of water while cold, though it was kept in the acid several days; it gave no signs of solution neither though assisted by a heat almost sufficient to make the acid boil. Copper does not discharge any air-bubbles neither, when put into strong spt. of salt while cold, though if kept a great while in the acid, especially if exposed to the open air, it does dissolve slowly; but with the assistance of heat it makes a considerable effervescence, and discharges vapours which, as will be shown hereafter are not inflammable; but though it makes so much effervescence yet it dissolves extremely slowly. [Cf. p. 11.]

All metallic substances except gold and platina unite, readily with the assistance of heat, to the concentrated acid of vitriol. The union is performed with a great effervescence and discharge of vapours smelling strongly of the volatile sulphureous acid. All metallic substances also except gold and platina dissolve readily in the nitrous acid [nitric acid]. They dissolve with great effervescence and discharge plenty of vapours, which appear plainly by the smell to contain a great deal of the acid; but which are of a more penetrating smell, more volatile, and in general are of a deeper colour than the fumes of the plain nitrous [nitric] acid; and which seem therefore to be composed of the nitrous acid united to the phlogiston of the metal. It is remarkable, too, that, though in general the nitrous acid has the least affinity to metallic substances of any of the mineral acids, yet it dissolves them all the readiest of any acid.

The reason of these phenomena seems to be as follows. It is well known that no metallic substance, the perfect ones excepted, can dissolve in acids without being deprived of its phlogiston. This seems to form the principal impediment towards their solution in acids. Zinc, iron and tin seem to have a greater affinity to the vitriolic and marine acids than they have to their phlogiston; whence they dissolve without much difficulty in either of these acids. I do not at all know indeed why they show so much less disposition to unite to the concentrated vitriolic acid than to the diluted. But all the other metallic substances seem to have a greater affinity to their phlogiston than they have to either of the mineral acids. In all probability the reason why notwithstanding this they unite so readily, with the assistance of heat, to the concentrated vitriolic acid is, that this acid when heated to a certain degree has a great disposition to unite to phlogiston, the affinity of the phlogiston to the acid counteracting its own affinity to the metal, whereby part of the acid unites to the metal, while the remainder unites to the phlogiston. The volatile sulphureous smell produced during the solution is a certain proof that the acid does actually unite to the phlogiston of the metal, and is a strong reason to suppose that it is in good measure owing to the affinity of the phlogiston to the acid that the metal is enabled to unite to the acid. In all probability the reason why metals dissolve so easily in the nitrous acid is owing to a like cause, namely, the affinity of the phlogiston of the metals to the nitrous acid; the fumes produced in dissolving metals in this acid seeming to be no more than the acid united to the phlogiston of the metals. The diluted vitriolic acid and marine acid seem to have very little disposition to unite to phlogiston; which is most likely the reason why metals dissolve so difficultly in those acids. The experiment which will be mentiond hereafter, concerning the solution of copper in spt. of salt, looks, however, as if spt. of salt had some small disposition to unite to phlogiston. It seems not unlikely that the reason why the other metallic substances will not furnish inflammable air, as well as zinc, iron and tin, is that their phlogiston will not fly off in close vessels without uniting to the acid whereby it is separated, and thereby losing its inflammable quality.

As the precipitates from the solutions of mercury and the perfect metals in acids are reducible without the help of inflammable fluxes, it has usually been thought that they are not deprived of their phlogiston by solution in acids. But yet the volatile sulphureous acid produced by dissolving silver and mercury in oil of vitriol is a strong proof that these

2 metallic substances are deprived of their phlogiston by solution in that acid at least. I should imagine therefore that mercury and the perfect metals were deprived of their phlogiston by solution in acids as well as the imperfect ones; but that by reason of their great affinity to phlogiston they acquired it again from the matter which must be added to separate the acid from them, when assisted by the heat necessary to reduce them; since there seems no reason to think that the purest fixed alcali, or even lime, is quite free from phlogiston. The effervescence and elastic vapours produced during their solution in aqua fortis or aqua regia (which are seemingly just of the same nature as those which attend the solution of the imperfect metals in these acids) agree very well with this hypothesis; whereas it is likely that if they were not deprived of their phlogiston thereby they would dissolve quietly and without effervescence; for the effervescence can proceed only from the separation of some elastic fluid either from the metal or the acid. If this hypothesis is true it will account very well for gold not being soluble in any simple acid, but only in a mixture of the nitrous and marine acids. Gold, I imagine, has little or no affinity to the nitrous acid, but only to spt. of salt; but its affinity to that acid alone is not sufficient to deprive it of its phlogiston. It therefore requires the united efforts of the nitrous and marine acids, the nitrous to absorb the phlogiston and the marine to dissolve the metal.

That gold has little or no affinity to the nitrous acid seems likely from what Dr. Lewis says, that gold when by particular management made to dissolve in the nitrous acid is precipitated again only by exposure to the air, and that upon committing a solution of gold in aqua regia to distillation the nitrous acid flies off, leaving the gold united to the spt. of salt.

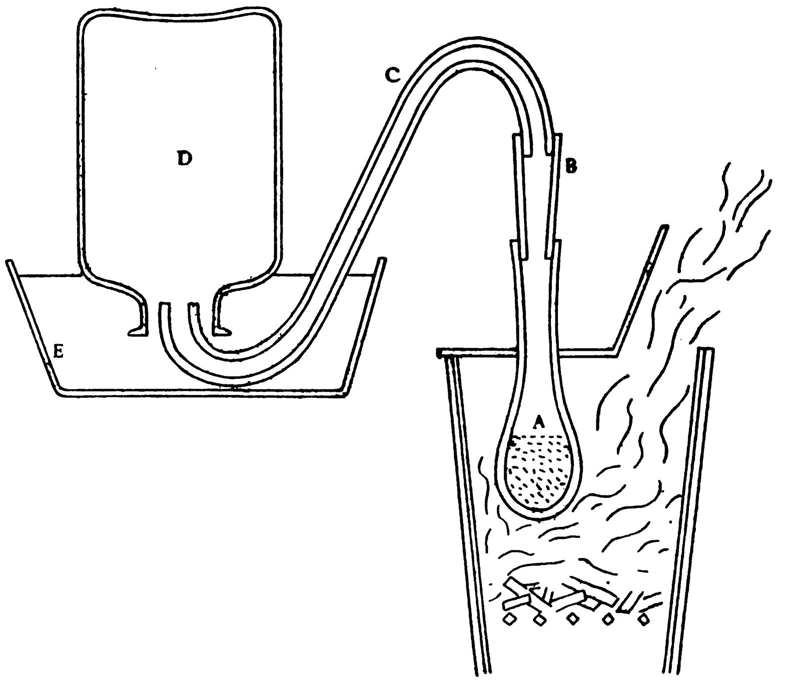
EXPERIMENTS ON FACTITIOUS AIR

PART IV

Containing experiments on the air produced from vegetable and animal substances by distillation.

[This paper was evidently intended to form a continuation of Cavendish's first communication to the *Philosophical Transactions*, entitled "Three papers containing Experiments on Factitious Air," and published in 1766. There is nothing to show why it was withheld from publication. The experiments were probably made not later than 1767. They are interesting as early attempts to gain an insight into the nature of the inflammable air obtained from wood and charcoal, and hence are of importance in connection with the Water Controversy.]

I received the air produced from these substances in inverted bottles of water nearly in the same manner as in the former experiments read to the Society, by means of the apparatus represented in the annexed drawing: where *A* represents a brass pot, in which are placed the materials



for distillation: *B* is a brass pipe fitted on to it by a cement composed of ochre and drying oil: *C* is a bent glass tube fitted to the brass pipe; and *D* is the bottle in which the air is to be received, filled with water, and inverted into the vessel of water *E*, over the end of the glass tube.

I used the cement above-mentioned in preference to that used in the former experiments, as it will bear a greater heat; and the reason why I placed the brass pipe *B* between the glass tube and distilling vessel, is that I expected the cement would bake so hard by the heat, that I could not get a glass tube out without breaking it. The joints were so well secured by this means that extreamely little air seemed to escape, though the brass pot was heated pretty strongly red hot. The pot was made of such a shape that I could easily clean the inside by scraping it with a bent piece of iron.

Exp. I. 400 grains of raspings of Norway oak, called wainscot by the Carpenters, were distilled in the above-mentioned manner, till no more air would rise with a heat just sufficient to make the distilling vessel obscurely red hot. The bottle in which the air was received was then removed, and another put in its place, and the distillation compleated with a pretty strong red heat. By this means that part of the air which requires a red heat to disengage it was procured separate from that which rises with a less heat. Each of these parcels of air were then brought in contact with sope leys in the manner described in my experiments on Rathbone-place water, in order to see whether they contained any fixed air, and to free them from it if there was any. The first parcel of air, namely that which rose first in distillation, measured 22100 grains when first made, and was reduced by the sope leys to 12700. The 2nd parcel measured 34600 grains, and was reduced by the same means to 30700 grains.

The quantity of common air containd in the distilling apparatus, allowing for the room occupied by the wood, was about 1700 grains; all of which must have been forced into the inverted bottle along with the first distilled parcel of air, and would not be absorbed by the sope leys. 1700 is about $\frac{2}{15}$ of 12700; so that the first distilled air, when reduced by the sope leys, contains about $\frac{2}{15}$ of its bulk of common air, or is a mixture of above 13 parts of pure factitious air to 2 of common air. The last distilled parcel must have been intirely free from common air.

All that air which was absorbed by the sope leys may, I think, be fairly supposed to be fixed air. The remaining air of each parcel was inflammable, but required a much greater quantity of common air to make it explode than the inflammable air from metals does: for a vial holding near 1200 grain measures being filled with 1 part of the first distilled air with $2\frac{1}{2}$ of common air, the mixture caught fire on applying a lighted candle to the mouth of the vial and went off with a small puff; but when the vial was filled with 1 part of the same air to 2 of common air it would not catch fire. In like manner a mixture of 1 part of the 2nd distilled air with 3 of common air went off with a puff; but 1 part of the same air with $2\frac{1}{2}$ of common air would not. So that the first distilled air required to be mixed with not less than between 2 and $2\frac{1}{2}$ times its bulk of common air, and the 2nd distilled air with between $2\frac{1}{2}$ and 3 times its bulk of common air, before it would explode; whereas the air from metals, when tried the same way, would explode though mixed with only $\frac{1}{2}$ its bulk of common air.

I next tried which of these parcels of air would explode with most force when mixed with considerably more common air than what was sufficient to enable them to catch fire. For this purpose I mixed some of each of these parcels of air, and also some inflammable air from zinc, with 4 times their bulk of common air, and tried them in the same bottle. The first distilled air went off with the least noise. As for the 2 others I was uncertain which made most: but the air from zinc went off with a sharper sound than the other, and no light could be seen in the bottle; whereas in the trials of each of the distilled airs a small light was seen.

The experiment was then repeated with mixtures of each of these airs with 5 times their bulk of common air. The first distilled air took fire, but with scarce any noise. The 2 others went off as near as I could judge with the same degree of noise, the distilled air with a small light visible in the bottle and a duller sound; the air from zinc without any light and a sharper sound.

It should seem therefore as if the second distilled air contained about as much phlogiston as the air from zinc, but that the first did not contain so much; for when the quantity of common air is considerably more than sufficient to consume the whole of the inflammable air, it seems likely that the loudness of the explosion should be in proportion to the quantity of phlogiston containd in the mixture.

In all these experiments the air was measured in a cylindrical glass with divisions on its sides, in such manner that I think I could not well err more than 5 grains or a 240th part of the whole mixture. The vial in which the explosions were made had a glass tube about an inch and a $\frac{1}{2}$ long and $\frac{4}{10}$ of an inch in bore fitted to its mouth, by way of contracting the orifice.

I also tried the specific gravity of each of these parcels of distilled air in my usual manner. 1000 grains of the first distilled air being forced into a bladder, which held 48000 grains and had a brass cock fitted to it, the bladder increased $\frac{3}{4}$ of a grain in weight on pressing out the air. So that, supposing common air to be 800 times lighter than water, this air, which was before said to contain $\frac{2}{15}$ of its bulk of common air, should be about $\frac{1}{17}$th part lighter than common air; and the pure factitious air without any mixture of common air should be $\frac{1}{14}$th or $\frac{1}{15}$th part lighter than common air, or near $6\frac{1}{2}$ times heavier than the inflammable air from metals.

21100 grain measures of the last distilled air being forced into the same bladder, there was an increase of 12 grains on pressing it out; whence this air appears to be lighter than common air in the proportion of 11 to 6, or near 4 times heavier than the air from metals.

The *caput mortuum* or matter remaining in the brass pot after the distillation was compleated, consisting of the wood reduced to charcoal, weighd 134 grains.

On the whole, the 400 grains of wainscot yielded with a heat less than sufficient to make it red hot, 9400 gra. measures of fixed air, whose specific gravity was before found to be about $1\frac{1}{2}$ times greater than that of common air, and 12700 of an inflammable air, and which was about $\frac{1}{16}$th part lighter than common air, and which required to be mixed with more than 2^{ce} [twice] its bulk of common air to make it explode. .With a greater heat than that, it yielded 5800 grains of fixed air, and 30700 of an inflammable air, which required to be mixed with above $2\frac{1}{2}$ times its bulk of common air to make it explode, and whose density was $\frac{6}{11}$ of that of common air. The weight of all this air together is 64 grains *id est* $\frac{16}{100}$

of the wood it was produced from, or near $\frac{1}{4}$ of the loss of weight which it suffered in distillation. It must however be observed that there was most likely more fixed air discharged than is here set down; as in all probability some of it must have been absorbed by the water.

As this inflammable distilled air is much heavier than that from metals, and requires to be mixed with a much greater proportion of common air to make it explode, I at first imagined it might consist of an inflammable air exactly of the same kind as that from metals, mixed with a good deal of air, heavier than it, and which had a power of extinguishing flame like fixed air; as I hinted before with regard to the air produced from meat by putrefaction; but on consideration, I fancy this air must really be of a different kind from that of metals; for if it had been only a compound of that air with some of a different kind, then a mixture of that compound with common air must necessarily I think have exploded with less noise than a mixture of pure inflammable air with the same proportion of common air, as it contains less inflammable matter than the latter mixture, and that compounded with a substance which should rather diminish than increase the explosion. Whereas the last distilled air was found to make as great an explosion as the air from metals, when both were mixed with 4 times their bulk of common air.

Exp. 2nd. In another trial made in the same manner, except that the whole of the distilled air was received together, without changing the bottle, the like quantity of wainscot yielded 19200 grain measures of fixed and 42700 of inflammable air. The inflammable air required to be mixed with more than 2^{ce} its bulk of common air to make it explode, and its density was less than that of common air in the proportion of 1·52 to 1. The weight of the whole of this air is 71 grains, *id est* near $\frac{18}{100}$ of the wainscot it was produced from. This experiment is exactly consistent with the former, except that the quantity of fixed air was greater, as might be expected, since the distillation was performed in much less time, and consequently much less fixed air could be absorbed by the water.

Exp. 3. I made another experiment with the same quantity of wainscot, the distilling pot being this time placed in oil, that I might see what would be the nature of the air which would rise with no greater heat than that of boiling oil. The oil caught fire which prevented me from compleating the experiment; I however, got 11500 grain measures of air, 5400 of which were fixed air, the remaining 6100 were inflammable, requiring somewhat more than 2^{ce} their bulk of common air to make them explode. Their density allowing for the common air in the distilling vessel was about $\frac{1}{18}$ part greater than that of common air.

Exp. 4. I also examined the air produced from tartar by distillation, though not in so careful a manner as the wainscot. It yielded more fixed, and less inflammable air than wainscot; 400 grains of it yielding 46600 grains of fixed air and 23500 of inflammable air. The inflammable air required to be mixed with more than 4 times its bulk of common air to make it explode, and was about $\frac{1}{11}$ part heavier than common air.

Exp. 5. 900 grains of Hartshorn shavings were distilled exactly in the same manner as the wainscot in the first experiment, except that the heat was raised to a rather greater degree before the bottle was changed. The first distilled parcel of air measured 33600 grains, and was reduced by sope leys to 20400. The common air left in the distilling vessel was 1630 grains; so that this air when reduced by the sope leys contained $\frac{4}{50}$ of its bulk of common air. The last distilled parcel measured 9400 grains, and was reduced by sope leys to 8900.

Each of these parcels of air, when thus reduced, was found to be inflammable. The first distilled air, tried in the same bottle as was used for similar experiments on the air from wainscot, caught fire on applying a lighted candle when mixed with 5 times its bulk of common air, but would not when mixed with only 4 times its bulk. The 2nd parcel caught fire when mixed with $2\frac{1}{2}$ times its bulk of common air, but would not with 2ce its bulk. I then compared the loudness of the explosion made by each of these parcels of air and of some air from zinc, when mixed with 6 times their bulk of common air. I could perceive very little difference between the 2 parcels of distilled air: but both of them seemed to make rather more noise than the air from zinc. The same difference in the manner of explosion between the distilled air and air from zinc might be observed with these, as with that from wainscot; namely that the distilled airs went off with the duller sound, and exhibited a light in the bottle, which was not visible with the air from zinc.

18240 grain measures of the first distilled air being forced into a bladder holding about 21600, there was an increase of weight of $5\frac{3}{4}$ grains on pressing out the air, so that allowing for the common air mixed with it the pure factitious air is lighter than water [air] in the proportion of 137 to 100.

8160 grain measures of the 2nd distilled air being forced into a bladder holding near 14000, it increased $4\frac{1}{4}$ grains on pressing out the air, whence it appears to be lighter than common air in the proportion of 171 to 100.

The *caput mortuum* consisting of the hartshorn burnt to a coal weighed 623 grains. The weight of all the air discharged appears from what has been said to be 51 grains, *id est* $\frac{1}{18}$ part of the weight of the hartshorn, or about $\frac{2}{11}$ of the loss of weight which it sufferd in distillation.

We have examined therefore 3 substances of very different natures, namely the first a simple wood, the 2nd a vegetable substance of a saline nature, and the 3rd an animal substance of the nature of bones. Each of them agreed in furnishing some fixed and some inflammable air; but the proportions of these airs were considerably different, the nature of the inflammable air was not quite the same in each, but yet hardly differing more than that produced from the same substance at different periods of the distillation; so that there should seem to be a considerable resemblance between the air produced by distillation from all animal and vegetable substances.

Dr Hales in his *Vegetable Statics* has given the quantity of air produced by distillation from a great variety of different substances. He observed that the air produced from some of them was inflammable, but does not mention whether he found any which was not. One of the methods which he used for measuring the air did not differ essentially from that used in these experiments.

In the first and 2nd experiments we have an examination of all the air which can be procured from wainscot by distillation in close vessels; but this is by no means all the air which it contains; for the *caput mortuum*, which, as was before said, consists of the wood burnt to charcoal, seems to contain a very remarkable quantity of fixed air.

The alcali produced by deflagrating nitre with charcoal is well known to effervesce with acids, and consequently to contain fixed air; which air I think can proceed only from the charcoal; for when nitre is alcalized by metals in their metallic form, which contain no fixed air[1] the alcali makes no effervescence with acids, as I know by experience; and I think it seems very unlikely that the nitre should furnish fixed air when deflagrated by charcoal, and not produce any when deflagrated by metals. This induced me to make the following experiment.

Exp. 6. 150 grains of the *caput mortuum* remaining after the distillation of wainscot in the first and 2nd experiments, well dried, were ground with 5 times their weight of nitre and about 130 grains of water, and when the whole was thought to be perfectly mixed, was deflagrated by little and little in an iron ladle. The intention of the water, was to make the matter deflagrate with less violence; whereby there was less danger of any fixed air being dissipated by the heat. The deflagrated matter was put into water to dissolve the alcali. The insoluble matter, consisting partly of the ashes of the *caput mortuum* and partly of some of the *caput mortuum* which had escaped the fire, weighed, when well dried, 38 grains; so that the loss of weight which the *caput mortuum* sufferd in

[1] The late Dr Hadley found that the volatile alcali produced by distilling sal ammoniac with red lead made a very considerable effervescence with acids; whereas that procured by distilling sal ammoniac with some metal in its metallic form (I believe it was copper) made none at all; which shews that though metals themselves contain no fixed air, yet some metallic calces contain a great deal; and probably all those do which are exposed during their calcination to the fumes of the burning fewel, and thereby have an opportunity of absorbing fixed air; for those fumes contain a great deal. It is doubtless owing to the fixed air it absorbs that lead increases in weight by being converted into minium.

deflagration was 112 grains. In order to find the quantity of fixed air in the alcaline solution, $\frac{1}{2}$ of it was saturated with the vitriolic acid, and the loss of weight which it suffered in effervescence observed with the same precautions as were used for finding the quantity of fixed air in pearl ashes in the 2nd part of these experiments [see *Phil. Trans.* 1766, also p. 91 *et seq.*]: it appeared to contain 62 grains. As the experiment makes the quantity of fixed air produced from the *caput mortuum* appear to be greater than the loss of weight which it sufferd in deflagration, which is impossible, I took another method to find the quantity in the remaining $\frac{1}{2}$ of the alcaline solution; namely, I mixed it with a sufficient quantity of lime water, whereby all the fixed air therein was transferred into the lime, which was thereby precipitated. I then found the quantity of fixed air in this precipitate; it appeard to be 59 grains which is only 3 grains less than it appeard to be the other way. By a mean of these experiments the quantity of fixed air separated from the 150 grains of *caput mortuum* should be 121 grains which is 9 grains more than the loss of weight which it sufferd in deflagration.

By a like experiment made with some more *caput mortuum* of the same kind the quantity of fixed air seemed still greater.

As it is impossible that the quantity of fixed air separated from the *caput mortuum* should exceed the loss which it suffers in deflagration, I must either be mistaken in supposing that all the fixed air in the alcali proceeded from the *caput mortuum*, and not from the nitre, or else some moisture must have flown off along with the fixed air in saturating the alcali with the acid, which would make the quantity of fixed air therein appear greater than it really is. This last supposition seems much the most probable.

In the 10th experiment of my 2nd paper on Factitious Air, in which the fixed air produced by dissolving marble in spirit of salt was made to pass through a glass cylinder filled with filtering paper, very little moisture was found to be condensed in the paper; from whence I concluded that very little moisture could fly off along with the fixed air in effervescence, as thinking that the greatest part of what did fly off must have been condensed in the filtering paper. But this conclusion was too hasty; as it seems not improbable, that a considerable quantity of moisture might fly off along with the fixed air, but which might adhere to it too strongly to be absorbed from it by the filtering paper. Perhaps some moisture may be necessary to enable the fixed air to assume an elastic form.

If we suppose, as I think seems much most probable, that all the fixed air in the alcali proceeded from the *caput mortuum*, it follows that this substance contains a remarkably greater proportion of fixed air than any other substance we have examined; though we cannot determine the exact quantity. I should think it very likely though, that, excepting the small quantity of ashes which it leaves on burning, it consists almost intirely of fixed air. It is almost needless to say that, according to this supposition, the determinations of the quantity of fixed air in alcaline substances, in the latter part of my 2nd paper on Factitious Air, cannot be depended on as to the exact quantity of it in any one substance; but I see no reason why this should incline one to think there is any fallacy in the determination of the proportion which the quantity of it in one substance bears to that in another.

Exp. 7. I made also an experiment of the same kind with common charcoal. It appeared to contain a great deal of fixed air, though not so much as the *caput mortuum*, namely $\frac{93}{100}$ of the loss of weight which it sufferd in deflagration. This difference may very likely be owing, partly to the charcoal not being dried before I weighed it, for charcoal contains a good deal of moisture though kept in a dry room, a circumstance that I did not attend to when I made the experiment, and partly perhaps to its not being well burnt; for I hardly imagine there can be any difference between the *caput mortuum* and charcoal perfectly burnt.

Exp. 8. The *caput mortuum* remaining after the distillation of Hartshorn shavings, when deflagrated with nitre is changed into a white calx, which in the experiments I made seemd not at all inferior in weight to the matter unburnt. So that as it seems to suffer very little loss of weight in deflagration it cannot be expected to furnish much fixed air. However, 382 grains of this matter well dried, being ground with $\frac{3}{8}$ of their weight of nitre and deflagrated, the fixed alcali produced thereby appeard to contain 22 grains of fixed air. The ashes washed and dried weighd not at all less than the *caput mortuum* they were produced from; so that it did not appear from this experiment that the *caput mortuum* sufferd any loss by deflagration. This in all probability must be owing either to some of the fixed alcali adhering to the ashes in such manner that it was not separated from thence by washing, or else to the ashes containing more moisture than the *caput* they were made from, though they were dried with a pretty considerable heat: for it is certain that the *caput mortuum* must have lost some weight by deflagration, though most likely it was not much. The ashes dissolved readily in spt. of salt and appeard to contain 30 grains of fixed air.

The event was very nearly the same in another experiment made with some more *caput mortuum* of the same kind.

[From the circumstance that Cavendish refrained from communicating this paper we may infer that he was not altogether satisfied with the results of his experiments, or that he was unable to explain them as fully as he might wish, and this would seem to be confirmed by the fact that he returned to the subject at several subsequent periods, as his laboratory notes show. The inflammable gas, freed from carbon dioxide, was a variable mixture of carbonic oxide, marsh-gas and hydrogen, in relative amounts depending upon the stage of the distillation, temperature, presence of moisture, etc. Doubtless it was this variable nature, affecting the physical characters of the "inflammable air," its specific gravity, ignition point, explosibility etc. which, in the absence of analytical means to prove that it was non-homogeneous, baffled Cavendish. Had he been able to procure either carbon monoxide or marsh-gas in a separate state, the inquiry would, in all probability, have been greatly facilitated. How near he was to the discovery and isolation of carbonic oxide on several occasions is obvious from his notes. He shows that after freeing the gas prepared from charcoal from carbonic acid, it at once killed a bird. "I plunged a burning candle in this air freed of its fixed air: it inflamed with a slight explosion: the flame was blue, in colour like that of burning sulphur." Unlike carbonic acid it was not affected by tincture of litmus. "The investigation of this shall be my employment for another memoir." Although he returned to the subject again and again, he got little beyond the stage of clearly recognising that the inflammable air from metals and from charcoal were not the same things, in which respect he was in advance of Priestley. Had Cavendish published Part IV of his paper on "Factitious Air" in or about 1767, it might have expedited the more general recognition of this fact, as, in spite of uncertainties, his determination of the physical characteristics of the inflammable air from wood, charcoal, etc., left little room for doubt that it was essentially different from that from zinc and iron. The delay may be said to have occasioned Priestley's error, and thus indirectly led to the controversy concerning Cavendish's claim to be the first to establish and announce the fact of the compound nature of water.]

THE LABORATORY NOTES OF "EXPERIMENTS ON AIR"

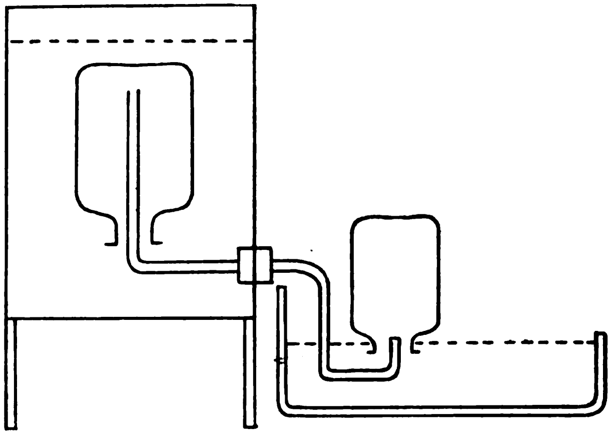
[The laboratory notes in connection with the "Experiments on Air" have been preserved, and are among the Chatsworth papers, as a separate parcel, paged and indexed by Cavendish. They are written on small detached sheets, $6\frac{3}{8} \times 4\frac{1}{8}$ inches, about 400 in number, and are mostly in the form of short memoranda and jottings of experimental data, interspersed with calculations of results. They rarely contain any deductions: any account of these was presumably reserved for the draft of the memoir in case it should be published. It is, however, not difficult to follow and interpret the greater portion of the notes in the light of the papers in the *Phil. Trans.* But many observations are recorded which are not included in the published memoirs, possibly for the reason that they were not directly relevant to the subject-matter, or that Cavendish was not satisfied with the numerical results. The whole of the work, which extends over several years and includes many hundreds of measurements of one kind or another, is almost entirely quantitative in character. It seems to have been impossible for Cavendish to work otherwise than by weight and measure.

If he has to prepare oxygen he invariably states the amount of red precipitate or turbith mineral he uses and the volume of the gas obtained. If he has occasion to use "sope-lees" he notes its strength by the amount of nitre the solution is capable of yielding on neutralisation with nitric acid. This custom is frequently of service in unravelling the real nature of the phenomena he observes, and which he sets down with remarkable fidelity. In the light of present knowledge we are now able to perceive many things which were obscure to him, and to see how frequently he was on the verge of discoveries which are credited to later workers.

A few extracts from these notes of hitherto unpublished observations are given—as showing Cavendish's manner of work and the patient care and thoroughness with which he investigated all sides of a subject on which he was engaged. For example, he has occasion to mix two gases which have no chemical action on each other. He inquires if the resultant volume is the sum of the volumes, or if there is "any penetration of parts." Do gases of different densities mix perfectly or does the heavier one subside? In connection with the working of his "new eudiometer" he is concerned to know whether there is any difference in the rate of movement of nitrogen and of common air. He is anxious to obtain some numerical estimate of the relative violence of the detonation of explosive gaseous mixtures, and contrives a kind of dynamometer for the purpose. The notes reveal with what care he investigated the different modes of preparing nitric oxide in order to ensure uniformity in its character in view of its use in the eudiometer, and they show the patience with which he established the conditions upon which depended the formation of the nitric acid in his synthesis of water. He was the first to show that a fairly accurate determination of the amount of oxygen in air could be made by the combustion of phosphorus. He performed an approximately accurate analysis of carbon dioxide and made repeated but unavailing attempts to establish the composition of carbon monoxide.]

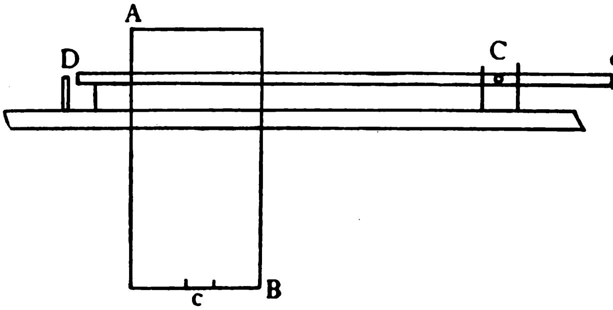
Is there any penetration of parts on mixing gases? "It was tried whether there was any penetration of parts on mixing common and inflammable air by means of the eudiometer. For this purpose 1 measure common air was let up by the cock into small bottle containing least measure of inflamm. air [that is, the least measure required to combine with the oxygen in it]. The diminution of bulk appeard to be ·002. One measure of infl. air being let up that way into the small bott. with least meas. comm. air, the diminution of bulk appeard to be ·004. Conseq. the diminution of bulk cannot exceed $\frac{3}{2250}$ of the whole."

Do the gases of a mixture separate out in the order of their densities? "It was also tried whether these airs mix perfectly or whether the com. air subsided to bottom. For this reason some of these were mixed in oblong spheroidical bottle and placed over the syphon in apparatus so that the syphon reachd to the top of bott. After standing some hours the air was drawn off by pouring water gently into the vessel so as not to



shake the glass and some of the 1st runnings and also some of the last caught in separate bottles, and the test of these bottles tried by new eudiom. For the 1st exper. test of $\left.\begin{matrix}\text{1st}\\\text{last}\end{matrix}\right\}$ runnings was $\left\{\begin{matrix}\cdot 472\\\cdot 482\end{matrix}\right.$. But the quant. of last runnings tried was only ·783 which might make the dimin. greater than it ought to be. In the 2nd exper. the test of $\left.\begin{matrix}\text{1st}\\\text{last}\end{matrix}\right\}$ runnings was $\left\{\begin{matrix}\cdot 537\\\cdot 540\end{matrix}\right.$ so that the last runnings appear to contain very little if at all more com. air than the first."

A "Measurer of explosions of inflam. air." "The strength of the explosion was tried by the machine repr in fig. *AB* is the brass cyl. for



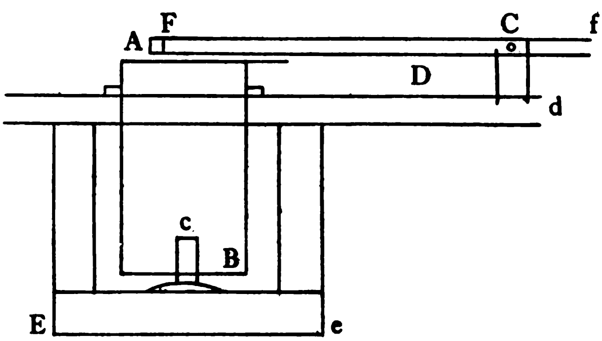
the air with a hole *c* at bottom. This is fixed tight to a board *Dd* turning on center at *C* and more or less weight is laid on *Dd*. In using it the cyl.

was first filled with water, the water entering by hole at bott. and the air escaping by cock at top, the app. for firing the air by el[ectricity] being placed in recess so as not to be wetted thereby. A proper quantity of mixt air was then pourd in by hole at bott. (but not near suffic. to fill the whole cyl.) and fired and the height which the cyl. sprung up thereby measured by an index. The force with which the cyl. was pressed down was found by hanging it to the end of pair of scales while in its place by a wire fastend to the middle of the top of the cyl.

The hole was exactly ½ inc. in diam. the cyl. was about 6 inc. long and 1·8 in diam. and held 4300 gra. [measures]. The recess in which the app. for firing held about 12 gra.

1713 [grain measures] of a mixt of 1 part of inf. [hydrogen] and 2 of common air being put into this machine and fired, it sprung up 4·23 [inches?] when weight lying on it was 32400 [grains] and 3·43 [inches] when the weight was 37300.

As it was suspected that the water driven out from the hole by the explosion was resisted by the water below and thereby was pressed against the bott. of the cyl. and made the force appear too great the machine was alterd in this manner.



AB is the cylinder passed through a round hole in the fixed board *Dd* so as to rise and fall in it freely and resting thereon by a projecting piece of brass *c* is a cylindrical piece of brass filling up the whole [hole] almost intirely and fixed upon the piece of wood *Ee* fastened to the bottom of the board *Dd*. *Ff* is a board turning on a center at *C* and resting on the brass cyl. intended to carry weight. Mixt air is put into the cyl. and fired as before and the height which it rises measured by index as before.

As the bottom of the cyl. almost touchd the wood *Ee* it was suspected that the small quant. water issuing between the plug and the sides of the hole might have partly the same effect as was apprehended in the former machine. A small piece of wood was in some trials placed between

Ee and the bott. cyl. so as to keep it raised about ¼ inch above it; but the effect was the contrary to what was expected as it always sprung up more when this wood was placed under than without it.

Some of the obs. made with it are given below:

Index before firing	D° after	Weights lying on board	Dist. nearest end from end board	Weight with which cyl. is pressed
·0	·0	1 + 2 + 3	14·6	97000
·72	·77	......	...	D°
·0	·0	1 + 3 + 4	14·6	84000
·73	·82	......		D°
·0	·16	1	14·6	68000
·74	1·27	......		D°
·0	·38	1	13·6	62000
·75	1·63	......		D°
·0	1·38	1	11·5	50000
·77	2·67	......		D°

In the next the bit of wood was placed between *Dd* and the projecting brass

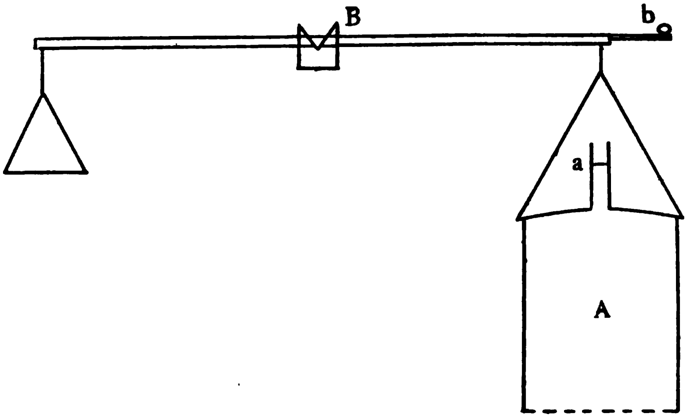
·93	1·15	1	14·6	68000

In all the foregoing 1713 of air consisting of 1 part inf. and 2 of common was used but in the 2 next 2^{ce} [twice] that quant. was used:

·0	·03	1	14·6	68000
·0	·13	1	13·6	62000
·0	·10	D° with 1713		"

[The experiments were continued at intervals during the greater part of August to Sept. 9, but apparently led to no definite numerical results, although a considerable number of "trials" under variable conditions were made.] "There seemed to be some minute interval of time between the electric spark and the explosion."..."It does not seem as if there was any connection between the strength of the spark and of the explosion."

Rate of efflux of gases. "It was tried whether the *vis inertia* of phlogisticated air was the same in proport. to its weight as that of common air by noting the time in which a given quantity passed through a given hole when urged by a given pressure by means of the following apparatus. *A* is a tin vessel 8½ inches in diam. and 10- deep with a small hole in the diaphragm *a*. This vessel is suspended over a vessel of water by the rod *B* turning on a center near the middle point and partly ballanced by a weight at the other end and sufferd to descend as the air runs through the hole. The time in which it descended a given space (about 7½ inches) was found by observing the time in which the knob *b* moved from one mark to another. The force with which the vessel was pressed down was about 10½ oz. the rest of the weight of the tin vessel being taken off by the counterpoise. The way by which it was filled with air was by holding



it under water till all the air was run out, then stopping pipe with thumb, raising up the vessel till the bottom was near the surface and pouring in the air [i.e. the gas to be experimented upon].

The event was as follows:

Oct. 28. 1780.

With com. air was	2′ 15″ running out
A 2nd time	2 12½
With air phlogisticated by liver of sulph.	2 7
With com. air	2 9 ″

[Common air phlogisticated with liver of sulphur would be practically nitrogen: its specific gravity was found to be $\frac{1}{47}$ less than that of common air. Apparently Cavendish was not satisfied with the performance of the apparatus, as no experiments with other gases were tried.]

Diminution of common air by phosphorus [Cavendish's title]. "July 30. Supposed th.[ermometer] 70° Bar. 30·09. A bit of dry phosph. was put into inverted bott. In a day's time it [the air] was a good deal diminished, but a drop of water having fallen on the phosph. prevented its acting; it was therefore fired by burning glass which was done with much smoke but no sensible flame. The phosphorus seemed intirely consumed. The original quantity of air was 3772 and the diminution was 651, bar. being then 30·02 and thermometer 71°. Its test was ·336."

[Neglecting the corrections for the slight change in temperature and pressure, this result would give 17·3 as the percentage volume of oxygen in air instead of 20·9. As all the phosphorus was consumed, its amount was probably insufficient to combine with all the oxygen.]

"Aug. 1. Some phosphorus was fired by burning glass in bott. with some common air in it. The original quantity of air was 3951, and, after the firing, was reduced to 3595. On burning another piece it was reduced to 3258. A little more phosph. was then put in and fired. It went out immed. and produced not much smoke. Next day it [the air] was found to be reduced to 3234. Allowance in all these trials being made for alteration of th. and bar. which was not great."

[This method of conducting the experiment was not likely to give accurate results. The data given point to the air containing 18·1 per cent. of oxygen. Nothing is stated as to how the successive pieces of phosphorus were introduced.

Phosphorus was an expensive article in 1780 and Cavendish was naturally very sparing of its use.

These are the only instances in the notes of its employment in the analysis of air.]

Air from plants. "Oct. 24, 1782. Sunshine. Some pond weed from Shepherd well was put in inverted jar and some more in large bott. Some chickweed also with some other water plants were put in a bott. In the evening the air was separated and the test tried in 2nd meth. was as follows:

Pond weed in jar	2·356
D° in bott.	2·528 [?]
Chickweed	2·278

The three parcels put together measured 11650 [grain measures] and lost 675 by washing with lime water. The next morning cloudy but now and then a little ⊙ [sun]. The 2 last were exposed again to ⊙ with fresh water and sufferd to remain all night and next day.

Oct. 26. Sunshine. Some more pond weed was exposed in 2 large jars and in the evening the air from them was collected and mixed together, and at the same time that from the old plants was collected and mixed together and all the botts and jars filled with fresh water and exposed. The test of the air from the $\left.\begin{matrix}\text{old}\\\text{new}\end{matrix}\right\}$ plants was $\left\{\begin{matrix}2·79\\2·547\end{matrix}\right.$. They did not lose much by washing with lime water.

Oct. 30. Some sunshine. Oct. 31. A good deal. In the evening the air was collected and the bottles filled with fresh water and exposed. The test of the air from the $\left.\begin{matrix}\text{old}\\\text{new}\end{matrix}\right\}$ plants was $\left\{\begin{matrix}2·97\\2·79\end{matrix}\right.$.

Nov. 4. The air was taken from the 2 bottles: its test was 2·553. The botts were then filled with beccæbunga[1] [?] the jars remaining as before.

Nov. 8. After 2 or 3 sunshiny cold days the air was taken. The test of that from the jars with pond weed was 2·670; that from the beccæbunga was 2·690.

[1] Veronica Beccæbunga.

Nov. 19. The plants had yielded a good deal of air but almost all that from the Beccæbun. was absorbed again. The test of that from the $\left.\begin{matrix}\text{Becca}\\ \text{pond weed}\end{matrix}\right\}$ was $\left\{\begin{matrix}\cdot 70\\ 2\cdot 706\end{matrix}\right.$.

All the above-mentioned except those got before Oct. 31, were mixed together and washed with milk of lime.

The test in $\left.\begin{matrix}1^{st}\\ 2^{nd}\end{matrix}\right\}$ method was $\left\{\begin{matrix}3\cdot 595\\ 2\cdot 710\end{matrix}\right.$, therefore its standard should be $\left\{\begin{matrix}3\cdot 48\\ 3\cdot 623\end{matrix}\right.$."

[This "air" was mixed with hydrogen and exploded. "21 [grains] of water was condensed which were very slightly acid to the taste but turned paper tinged with blue flowers evidently red." In a second experiment the "air" was mixed with a larger volume of hydrogen. 18 grains of water was formed. "It did not taste at all acid, nor turned blue paper at all red even when much diminished by evaporation."]

Air from mines. "Very bad air sent Jan. 22, 1783. 6700 [grain measures] of *B* 5 [probably the number of the bottle in which the sample was collected] was dimin. 90 or $\frac{1}{74}$ by lime water [1·34 per cent. of carbonic acid][1]. This air tested with 1st method was ·94. Standard ·86. Common air 1·08.

Jan. 25. Another bott. was tried and came out as follows $\left\{\begin{matrix}\cdot 923\\ \cdot 923\end{matrix}\right.$."

[Cavendish then compared the mine air with that in which a candle would no longer burn.]

		Test in 1st method	Standard
Air in which wax candle burnt out in receivers holding common air	*200000	·907	·827
	80000	·883	·803
	9500	·668	·581

* N.B. "The candle in the least jar burnt 10 secs., in the 2nd 70 secs. and in the largest either 2 or 3 mins., I am not sure which but I believe 2 mins."

Alteration in air by breathing. "Feby. 5, 1786. Some air was breathed as long as I well could by means of tin pipe from a glass jar holding 60000 gra. inverted into water. 57000 of this breathed air was drawn out into an exhausted globe and well washed with lime water by which 3880

[1] [This air, although undoubtedly very bad, is not as bad as that found occasionally by Angus Smith in mines. The largest amount of carbonic acid, 2·5 per cent., was found in a Cornish mine: the average of 339 analyses was ·785 per cent. by volume of carbonic acid (R. Angus Smith, *Air and Rain*, 8).]

[A candle will not burn with less than 18 per cent. of oxygen when there is 3 per cent. of carbonic acid present at the same time (*loc. cit.*).]

or $\frac{1}{18}$ of the whole was absorbed[1]. Its test after this in 2nd method was ·444; therefore its standard was ·40 and therefore the dephlog. air destroyed was $\frac{\cdot 6}{5} = \frac{1}{8\cdot 3}$ and therefore the fixed air is $\frac{83}{150} = \frac{166}{300} = \cdot 55$ of the dephl. air destroyed."

CAVENDISH ON CHEMICAL NOMENCLATURE

[Among the miscellaneous papers of the Chatsworth MSS. is a rough draft of a letter in answer to one from Blagden, dated Sept. 16, 1787. Blagden, who at that period was Secretary of the Royal Society, and intimately associated with Cavendish, had written from Dover on his way to France. He sends a short account of General Roy's operations at Dover in establishing a trigonometrical connexion between the observatories of Paris and Greenwich in order to determine the difference of longitude, a work in which the Royal Society was officially interested, and which they had initiated in 1784, but which had been delayed on account of the dilatoriness of Ramsden in supplying the instruments. General Roy had expressed a hope that he might have a visit from Cavendish. In the letter Blagden states that he had brought with him Sir Joseph Banks' presentation copy from Lavoisier of the *Nomenclature Chymique*, which he commends to Cavendish's attention.]

As the weather seems likely to be wet, I have given up all thoughts of coming to you at present, but be so good as to tell Gen. Roy that if the weather grows fair I shall very likely accept his offer and pay him a visit while he is about his operations, especially if the base is going on at the same time.

I was mistaken in supposing the angles could be reduced in Dr. Maskelyne's manner without taking into consideration the elevation or depression of the objects above the horizon. My mistake arose from supposing that if the objects were reduced to the level of the sea by lines drawn parallel to the direction of gravity at those places the reduced objects would subtend the same horizontal angle at the place of observation as the objects themselves which is true in the sphere but not in the spheroid.

Mr. Le Voisier has sent me the Nomenclature. I do not know whether you have seen the sequel of Saussures journey. The most remarkable circumstance is the effect of the rarity of the air on them which was such even after the fatigue of climbing was over that I wonder the French

[1] [This would be equivalent to 6·8 per cent. of carbonic acid. Air expired from the lungs contains from 3·3 to 5·5 per cent. by volume of carbonic acid. On the average, 4·78 vols. per cent. of oxygen is inhaled and 4·38 vols. of carbon dioxide is expired. The "respiratory quotient" obtained by Cavendish is too small.]

astronomers at Peru did not observe it, unless you suppose that this effect was made remarkably more sensible by their preceding fatigue.

He computed the height from his observations of the barom. both according to De Luc's and Trembley's rule; according to one it came out a little greater and by the other a little less than according to Sir G. Shuckburgh's measurement[1].

I have been reading La V.'s preface. It has only served the more to convince me of the impropriety of systematic names in chemistry and the great mischief which will follow from his scheme if it should come into use. He says very justly that the only way to avoid false opinions is to suppress as reasoning as much as possible unless of the most simple kind and reduce it perpetually to the test of experiment and can anything tend more to rivet a theory in the minds of learners than to form all the names which they are to use upon that theory.

But the great inconvenience is the confusion which will arise from the different hypotheses entertained by different people and the different notions which must be expected [to] arise from the improvements continually making. If the giving systematic names becomes the fashion it must be expected that other chemists who differ from these in theory will give other names agreeing with their particular theories, so that we shall have as many different sets of names as there are theories and in order to understand the meaning of the names a person employs, it will be necessary first to inform yourself what theories he adopts. An equal inconvenience, too, will arise from the necessity of altering the terms as often as new experiments point out inaccuracies in our notions or give us further knowledge of the composition of bodies. But to shew the ill consequences of what they are about, let them only consider what would be the present confusion if it had formerly been the fashion to give systematic names and that those names had been continually alterd as people's opinions alterd. The great inconvenience is the fashion which so much prevails among philosophers of giving new names whenever they think the old ones improper as they call it. If a name is in use and its meaning well ascertained no inconvenience arises from its conveying an improper idea of its nature and the attempting to alter it serves only to make it more difficult to understand people's meaning.

With regard to distinguishing the neutral salts of less common use by names expressive of the substances they are composed of the case is different, for their number is so great that it would be endless to attempt to distinguish them otherwise; but as to those in common use, or which are found naturally existing, I think it would be better retaining the old

[1] [Observations made in Savoy, to Ascertain the Height of Mountains by means of the Barometer; being an Examination of Mr De Luc's Rules, delivered in his Recherches sur les Modifications de l'Atmosphere. By Sir George Shuckburgh, Bart., F.R.S. *Phil. Trans.* 67, 1777, 513.]

names. But with regard to salts whose properties alter according to the manner of preparing them, such as corrosive sublimate, calomel, etc. I should in particular think it very wrong to attempt to give them names expressive of their composition.

As I think this attempt a very mischievous one it has provoked me to go out of my usual way and give you a long sermon. I do not imagine indeed that their nomenclature will ever come into use, but I am much afraid it will do mischief by setting people's minds afloat and increasing the present rage of name-making.

CAVENDISH'S PAPERS ON HEAT

[On leaving Cambridge in 1753, Cavendish, after a short tour on the Continent with his brother Frederick, appears, from a few fragmentary notes and observations to be found among his papers, to have assisted his father, with whom he resided, in the physical and meteorological inquiries which are known to have engaged Lord Charles Cavendish's attention. It was probably this circumstance which first led the son to engage in experimental investigation. As we have seen, Lord Charles, who seems to have possessed what at the time was a pretty extensive physical cabinet, was interested in thermometry, and occasional mention is made by Cavendish of the instruments which were used by him from time to time. He was thus induced, almost at the very outset of his career as an experimenter, to take up the study of heat, and to pursue the calorimetric inquiries which are to be found, more or less systematically summarised and described, among his unpublished manuscripts. The evidence contained in these papers leaves no room for doubt that Cavendish embarked upon his investigations at about the same period as Black, or possibly somewhat later, and that he worked independently, and to a large extent in ignorance of Black's observations and theories. When they were actually begun is impossible to determine, but it is reasonably certain, from the few dates scattered among the notes, that much of the experimental work was done in or before 1764. Black's earliest observations on heat were made in 1758, but nothing was published concerning them, or his subsequent work, except through his University Lectures at Glasgow and Edinburgh, until much later; and it is certain that Cavendish could have had no knowledge of Black's work through any printed source. How far it was known to scientific and university circles in England at about the period referred to is difficult to determine. Cavendish maintained no active connection with Cambridge, and although he was elected into the Royal Society in 1760 he published nothing until six years later; his talents were then unrecognised, and his shy retiring nature, taciturnity and reluctance to enter into conversation, must have largely prevented him from obtaining information orally even if it were available at the period.

Everything, therefore, goes to show that his work on heat was original and independent, and that he discovered for himself the facts concerning latent and specific heat, thermal expansion, melting points, heat of chemical combination, etc., contained in his manuscript remains. Some of these early observations were published many years subsequently, as incidental and confirmatory results, as, for example, in the paper on the freezing of mercury printed in 1783, and there are occasional allusions to them under such phrases "as I know by experience," but with no details.

It is probably useless to surmise why the young experimenter should have refrained from making known the results of his independent observations, at the time he made them. But Cavendish was no ordinary man, and ordinary conventions and rules of conduct are inapplicable to him.

"In truth," as Wilson points out, "with Cavendish, publication was the exception, not the rule, and he has left so many completed researches unpublished, that no special hypothesis is needed to account for those upon heat having remained in manuscript." "Perhaps," he adds, "a reluctance to enter into even the appearance of rivalry with Black, prevented him from publishing researches which might be thought to trespass upon ground which the latter had marked off for himself, and pre-occupied."

Be this as it may, there can be no question that had Cavendish published his observations at, or near, the time he made them he would have anticipated much of the credit which belongs to Irvine, Crawford and especially Wilcke.]

EXPERIMENTS ON HEAT

It seems reasonable to suppose that on mixing hot and cold water the quantity of heat in the liquors taken together should be the same after the mixing as before; or that the hot water should communicate as much heat to the cold water as it lost itself; so that if the expansion of the mercury in the thermometer is proportional to the increase of heat the difference of the heat of the mixture and of the cold water as measured by the thermometer multiplied by the weight of the cold water should be equal to the difference of heats of the hot water and mixture multiplied by the weight of the hot water, or the excess of the heat of the mixture above that of the cold water should be to the difference of heats of the hot and cold water as the weight of the hot water [is] to that of the mixture.

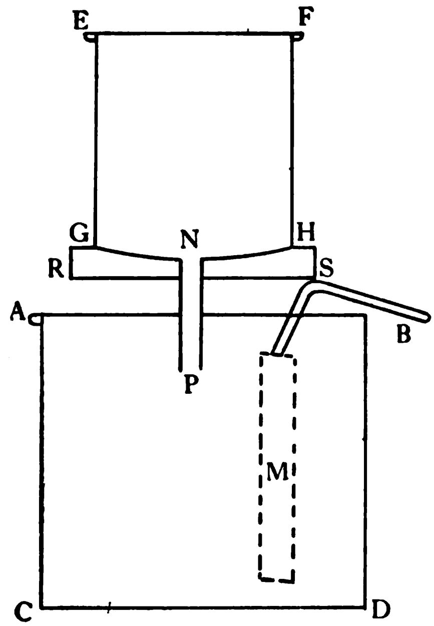
The following experiments were made with an intent to see whether the excess of the heats of the hot water and the mixture above the cold water really bore that proportion to each other or not.

The apparatus used in these experiments is such as is represented in the annexed figure.

ABCD is a tin cylindrical vessel about 10 inches in diameter and as much in depth holding about 400 oz. of water with a cover of the same metal fitted to it.

M is a slip of tin plate about 1½ inch broad, fastend to a bent piece of wire, passed through a small hole in the cover, serving to stirr the water in the pan.

EFGH is a cylindrical tin funnel, the pipe of which *NP* is about ½ inch in diameter and enters into the pan *ABCD* through a hole made in the cover. This funnel also is furnished with a cover and a stirrer of the same kind as that to the pan. Each cover also has a hole in it to put a thermometer in.



GHRS is a round piece of wood placed under the bottom of the funnel and kept by short legs at about an inch distance from the cover of the pan in order to prevent the water in the pan from being heated by the hot water in the funnel.

The pipe of the funnel is stopt up with a cork, which by means of a piece of wire fastend to it, and passed through a hole in the cover of the funnel, can be pulled out without taking off the cover. In trying the experiment, the cold water was put in the pan and the weight set down after its heat was ascertaind, which, as it differed but little from the temper of the air alterd very slowly. The thermometer was taken out and the funnel with the pipe corked up was put in its place, some hot water was then put into it, the cover put on, and the thermometer put in. When the heat of the water and its rate of cooling was sufficiently ascertaind, the water being kept stirrd all the time, the cork was drawn out of the pipe and the hot water let into the pan, the funnel was then taken off, the thermometer put into the pan and the heat of the mixture found, after which, by weighing the mixture, the quantity of hot water put in was known. By this method of proceeding the hot water was put into the pan without being exposed to the open air, and consequently without any evaporation, so that it was coold very little in passing into the pan and I was able in some measure to estimate how much that little was, whereas if it had been pourd in without this precaution it would most likely have been coold considerably by the evaporation and contact of the air, and I should not have been able to form any guess how much it was coold.

Experiment 1. At 5^{hr} 55′ the cold water in the pan was found to be at $48\frac{2}{3}°$ of heat and was found to increase about 1° in 50 minutes.

At $6^{hr.}$	$12^{min.}$	$10^{sec.}$	the hot water in funnel was at			197°
	12	50	,,	,,	,,	196
	13	25	,,	,,	,,	195
At 6	16	0	the mixture was at			99
	20	30	,,	,,		$98\frac{1}{2}$

The cork was pulled out of the pipe of the funnel at $6^{hr.}$ $13^{m.}$ $25^{sec.}$ and the hot water took up near $30^{sec.}$ to run intirely out of the funnel but run almost out in 15 or 20^{secs}. The weight of the cold water was $249\frac{8}{10}$ oz.; that of the mixture $383\frac{8}{10}$ whence the weight of the hot water was 134 oz.

But before we proceed to make this experiment with the rule it was intended to examine, it is necessary to make some corrections. First, as neither the heat of the hot or cold water or mixture were found at the precise time of making the mixture, there must be some corrections made to the heats shewn by the thermometer. Let us assume, therefore, $6^{hr.}$ $13^{m.}$ $40^{sec.}$, or $15^{sec.}$ after the cork was pulled out, as the time of making the mixture, and find what would be their heats at that time according to their observed rates of heating and cooling. The hot water coold 2° in about $1^{m.}$ $15^{secs.}$ or $\frac{4}{10}$ of a degree in $15^{secs.}$; so that its heat at $6^{hr.}$ $13^{m.}$ $40^{sec.}$ would be 194°·6. According to the same method of proceeding, the heat of the cold water at the same time would be 49°, and that of the mixture $99\frac{2}{10}$; so that $194\frac{6}{10}°$, 49° and $99\frac{2}{10}°$ may be looked upon as the true heats of the hot water, cold water, and mixture. Secondly, it must be observed that part of the effect of the hot water was spent in heating the pan, and therefore an allowance must be made on that account. The weight of the pan was 31·25 oz.; that part of the stirrer which was within the pan was 2·3 oz., and the cover weighed $9\frac{9}{10}$ oz. As the inside surface of the pan would be heated much faster by the water than the outside surface would be coold by the air, I believe we may suppose the tin pan would be heated almost as hot as the mixture within it, but that the cover being in contact with the air on both sides would be heated only to a mean heat between the cold water and mixture. Therefore 33·55 oz. of tin plate were heated to the heat of the mixture, and 9·6 oz. were heated $\frac{1}{2}$ as much, which comes to the same thing as if 38·35 oz. were heated to the full heat of the mixture. Whence we may conclude from the experiment that the heat of a mixture of 134 oz. of water whose heat is 194°·6 with 249·8 oz. of water whose heat is 49°, and 38·35 oz. of tin plate of the same degree of heat, will be 99°·2. But it will appear from an experiment, which will be mentiond hereafter, that cold iron filings added to hot water, cool it no more than the addition of $\frac{1}{8\frac{1}{2}}$ that quantity of water would do, and consequently the 38·35 oz. of tin plate in the foregoing experiment has no more effect in cooling the hot water than 4·5 oz. of water. Wherefore adding this to the weight of the cold water I think we may conclude that the true heat of a mixture of 134 oz. of water whose heat = 194°·6 with 254·3 oz. of water whose heat = 49° is 99°·2, which is exactly the same that it ought to be according to the forementiond rule that the difference of heat of the cold water and mixture is [to] the difference of heats of the hot and cold water as the weight of the hot water to the weight of the mixture.

It is plain that the allowance to be made for the effect of the pan and cover is very uncertain, as one can give but a very uncertain guess how much they will be heated, but if their effect had been intirely neglected the computed heat of the mixture, or its heat according to the above mentiond rule, should be but $\frac{6}{10}$ of a degree greater than I made, so that it should seem as if no error to signify could proceed from thence.

In trying the heat of water-mixture in this and all the following experiments of this kind, care was taken that so little of the scale of the thermometer should be out of the pan that no sensible error could proceed from thence.

The experiment was repeated in the same manner with nearly the same quantities of water. The heat of the mixture was $\frac{1}{10}$ of a degree less than it should be by computation.

Experiment 2. With about equal quantities of hot and cold water.

At 7.24.0 the cold water was at	48°
At 7.37.0 the water in funnel was at	193
38.30 ,, ,, ,,	192
40.0 ,, ,, ,,	191
At 7.42.0 the mixture was at	116¾
43.40 ,, ,,	116½
46.30 ,, ,,	116
The weight of the cold water	196·7 oz.
,, ,, mixture	387·2

The heats corrected as before are:

Cold water	48°·3
hot water	190·8
mixture	117·2

Making the same allowance as before for the effect of the pan, the heat of the mixture by computation should be 117°·6; consequently the heat of the mixture was $\frac{4}{10}$ of a degree less than it should be by computation.

By another experiment made with nearly the same quantities of water the heat of the mixture was exactly the same as by computation.

Experiment 3. With about 2 parts of hot water to one of cold.

At 6. 4.0 the cold water was at	47°
6.16.0 „ „	47½
At 6.23.50 the water in the funnel was at	203
25.0 „ „ „	202
At 6.28.20 the mixture was at	148
31.45 „ „	147
The weight of the cold water	131·5 oz.
„ mixture	392·9 oz.
The corrected heat of the hot water	201·8°
„ „ cold water	47·9
„ „ mixture	149·2

The heat of the mixture by computation should be 149°·1; therefore the heat of the mixture is $\frac{1}{10}$ of a degree greater than it should be by computation.

Experiment 4. The experiment was repeated with about equal quantities of hot and cold water in a rather different manner from before, namely, the hot water was put into the pan and the cold water into the funnel, the pipe of the funnel being made to enter into the cover of the pan near one side so that there was room to keep the thermometer in the pan and to stir the water therein while the funnel was in its place.

At 6$^{\text{hr.}}$ 58$^{\text{min.}}$ 30$^{\text{sec.}}$, the water in the pan was at 198°·5: at the same time the water in the funnel was at 45°½. The cork was then immediately taken out and the cold water let into the pan. At 7.1.20 the mixture was at 123° and at 7.7.10 at 122°. The weight of the cold water was 195 oz. and that of the mixture 394½.

As the heats of the hot and cold water were observed at the same time and immediately before the cork was drawn out, there is no need to correct their heats, but only to correct the heat of the mixtures by finding what would have been its heat at the time of drawing out the cork according to its observed rate of cooling, wherefore 123°·5 is to be looked upon as the true heat of the mixture.

If the same allowance is made for the effect of the pan as in the former experiment the heat of the mixture by computation is 123°·7.

But in reality the allowance ought to be rather different. Let us suppose that before the cold water was put in, that part of the pan which was in contact with the hot water was of the same heat as the water; and that the remainder of the pan and the cover was about a mean between the heat of the hot water and the air of the room, *id est* about the heat of the mixture; and let us suppose that after the mixture was made (as the pan was then near full) that the whole pan was of the same heat as the mixture, and the cover about a mean between the heat of the mixture and of the air. Therefore that part of the pan which was not in contact with the hot water was not coold at all by pouring in the cold water. The rest of the pan and the cover was coold in the same proportion as it was heated in the former experiment. As the pan was about $\frac{1}{2}$ full of hot water, that part which was not in contact with the water was about $\frac{2}{3}$ of the whole; whence the allowance for the effect of the pan, etc. comes out about 3 oz. and therefore the heat of the mixture by computation is $123°\frac{1}{2}$ which is exactly the same as the observed heat.

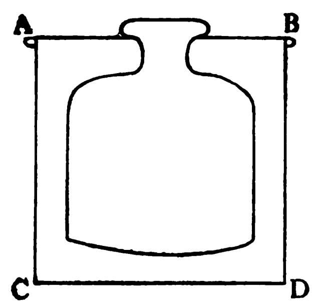
By another experiment tried in the same way with nearly the same quantities the heat of the mixture was $\frac{1}{2}$ a degree less than it should be by computation.

In all the foregoing experiments the observed heats of the mixture differ from the computed by not more than the different experiments do from each other, so that the above-mentiond rule seems perfectly conformable to experiment.

It must be observed that in the first method of trying the experiment the greater the allowance I made for the effect of the pan, the less would the computed heat come out, whereas in the 2[nd] way of trying, the greater allowance I made the greater would the computed heat turn out. Consequently as the computed heats agree equally well with the observed in both methods of trying the experiment, it appears that the allowance I made could not be sufficiently wrong to be of any signification.

Section 2. One would naturally imagine that if cold ☿ [mercury] or any other substance is added to hot water the heat of the mixture would be the same as if an equal quantity of water of the same degree of heat had been added; or, in other words, that all bodies heat and cool each other when mixed together equally in proportion to their weights. The following experiment, however, will show that this is very far from being the case.

The glass bottle used for making the mixtures in the following exper. was of the shape marked in the annexed figure and was blown thin and very regular the better to bear sudden alterations of heat and cold.



In order to prevent the liquor in the bottle from cooling so fast as it would otherwise do, the bottle was inclosed in a tin pan with a cover to it, *ABCD*, in such manner that only the mouth of the bottle was exposed to the air, all the rest being within the pan. The bottle was kept steady in the middle of the pan by tin rings and the spaces between the bottle and sides of the pan were filled with wool. In trying the experiment the hot water or other liquor was put into the bottle and when its heat and rate of cooling was sufficiently ascertaind, the cold (whose heat as being nearly of the temper of the air alterd very slowly) was put. The heat of the liquor in the bottle was found by putting the ball of the thermometer into it and keeping it there till it was come to the full heat of the liquor, the bottle being all the while gently shaken, after which the thermometer was taken out, the bottle corked up close and kept shaking till the heat of the liquor was tried again. The time during which it was requisite to keep the thermometer in the liquor before it acquired the full heat was different according to the nature of the liquor: in water it was requisite to keep it about [1]; in ☿ rather less; in spirits of wine somewhat more.

The bottle held rather more than 19 oz. of water, and weighed $9^{oz.}$ $18^{dwt.}$ 0^{grains}: the tin pan and wool weighd 11.15.4 [$11^{oz.}$ $15^{dwt.}$ 4^{grains}].

Exp. 5. Mixture of hot and cold water.

At 20 minutes the water in the bottle was at	158½°
23 ,, ,, ,, ,,	155
26 ,, ,, ,, ,,	152

The cold water was then immediately put in.

At 29 minutes the mixture was at	106½
32 ,, ,, ,,	106
35 ,, ,, ,,	105½

The weight of the cold water put in was $5^{oz.}$ $11^{dw.}$ $17^{gra.}$ and its heat was 51°½. The weight of the mixture was 11.3.19 and therefore the weight of the hot water 5.12.2.

Exp. 6. Mixture of cold ☿ with hot water.

At 43 minutes the water in the bottle was at	161¼°
46 ,, ,, ,, ,,	158
49 ,, ,, ,, ,,	155

The ☿ was then immediately put in.

At 52 minutes the mixture was at	108¾
55 ,, ,, ,,	108¾
58 ,, ,, ,,	108

The weight of the ☿ put in was 171.12.0 and its heat was 54°½: the weight of the mixture was 177.4.3 and therefore the weight of the hot water 5.12.3.

The difference of heat of the hot and cold water in the 5^{th} exp. was the same as the difference of heat of the hot water and ☿ in the 6^{th} exp. and the weight of the hot water was the same in both these experiments, and yet the difference of heat of the hot water and mixture was near as great in the 5^{th} exp. as the 6^{th}; so that the hot water was coold near as much by the addition of 5.11.17 of water as by that of 171.12.0 of ☿; that is, hot water is coold near as much by the addition of 1 part of cold water as by that of 30 parts of ☿ of the same heat.

[1] [Time not stated.]

In order to compare the effects of ☿ and water more exactly, let us assume 26½ in the 5th exp. or ½ a minute after the last time of observing the heat of the hot water, for the time of making the mixture, and find the heats of hot water and mixture at that time, as in the former experiments. By this means the corrected heats of the hot water and mixture come out 151°·5 and 106°·7. The quantity of cold water added was $5^{oz.}$ $11^{dwt.}$ $7^{gr.}$ and its heat was 51°·5. But by the above-mentiond rule 106°·8 is the heat of a mixture of 5.11.7 of water of 51°·5 of heat with 6.14.4 of water of 151°·5 of heat; so that the effect is the same as if the weight of the hot water had been 6.14.4 and the effect of the bottle and pan had been nothing, and consequently the effect of the bottle and pan is equal to that of 1.6.2 of water.

According to an experiment which will be related hereafter, that pounded glass added to cold water cools it as much as $\frac{1}{8{\cdot}073}$ its weight of water of the same degree of heat; consequently supposing the glass bottle to be heated to the same degree as the liquor within, which I think it must necessarily be, its effect must be equal to that of 1.4.12 of water; consequently there remains 2.2 for the effect of the pan, etc.

The heats of the hot water and mixture in the 6th exp. corrected as above are 154°½ and 109°·1, and as the same bottle and pan was used in this exp. as in the last, and as the quantity of hot water also was the same, though I do not know whether that is of any signification, it is plain that the quantity to be allowed for the effect of the bottle and pan must be the same in this exp. as in the last. Therefore the heat of the mixture is the same as if instead of the ☿, 5.15.21 of water of the same degree of heat had been added, for 54°·3, the difference of heats of the mixture and ☿ is to 45°·7, the difference of heats of the hot water and ☿, as 6.18.5. to 5.15.21; consequently cold water added to hot water cools it as much as the addition of 29·61 its weight of ☿ of the same degree of heat will do; or, in other words, the effect of water in cooling hot water is 29·61 times greater than that of ☿ of the same heat.

The experiment was repeated in the same manner, with different quantities of hot water.

Exp. 7. Mixture of cold water with hot.

At 2 minutes	the hot water	was at	151°
5	,,	,,	149
8	,,	,,	147

The cold water was then put in.

At 11 minutes	the mixture	was at	119
14	,,	,,	118½
17	,,	,,	117¾

The weight of the cold water added was 3·971 oz. and its heat was 51°¼. The weight of the mixture was 12·715 and consequently the weight of the hot water 8·744.

Exp. 8. Mixture of ☿ with hot water.

At 22 minutes the hot water was at			157½°
25	,,	,,	155
28	,,	,,	152¾

The ☿ was then put in.

At 31 minutes the mixture was at			123
34	,,	,,	122½
37	,,	,,	121½

The weight of the ☿ was 120·212 [oz.] and its heat 51°. The weight of the hot water was 8·719 oz. Proceeding as before, the corrected heats of the hot water and mixture in the 7th exp. are 146°·7 and 119°·5 and the effect of the bottle and pan is equal to that of 1·074 oz. of water. In the 8th exper. the corrected heats of the hot water and mixture are 152°·3 and 123°·6. Therefore the 120·212 oz. of ☿ coold the water as much [as] 3·871 oz. of water would do and therefore the effect of water in cooling hot water is 31·05 times greater than that of ☿.

Having thus found the proportional effect of cold water and ☿ in cooling hot water I next tried whether their effects in cooling hot ☿ would bear the same proportion to each other.

Exp. 9. Mixture of cold ☿ with hot.

At 7 minutes the ☿ in the bottle was at				167¼°
10	,,	,,	,,	163½
13	,,	..	,,	160

The cold ☿ was then put in.

At 16 [min.] the mixture was at			113
19	..	,,	113¼
22	,,	,,	111½

The weight of the cold ☿ was 125·437 [oz.] and its heat 54°⅓. The weight of the hot ☿ was 125·45.

Exp. 10. Cold water added to hot ☿.

At 42′ the ☿ in the bottle was at			168
45	,,	,,	164
48	,,	.,	160¼

The water was then put in.

At 51′ the mixture was at			113
54	,,	,,	111¾
57	,,	,,	110¾

The weight of the cold water was 4·115 [oz.] and its heat 52°¾. The weight of the hot ☿ is 125·425.

In the 9th exp. the corrected heats of the hot ☿ and mixture are 159°·4 and 114°·4; and the effect of the bottle is equal to that of 42·08 of ☿. In the 10th exp. the corrected heats of the hot ☿ and mixture are 159°·6 and 113°·9: wherefore the 4·115 of water coold the ☿ as much as 125·18 of ☿ and therefore water cools hot ☿ as much as 30·42 its weight of ☿ can do, or the effect of water in cooling hot ☿ is 30·42 times greater than that of ☿. By a mean of all 3 experiments the effect of water in cooling hot water or ☿ seems to be 30·1 times greater than that of ☿.

The effect of water in cooling hot water appeard to be 29·61 times greater than that of ☿ by one experiment and 31·05 by the other. Whence it appears that the effects of water and ☿ in cooling hot ☿ bear the same proportion to each other as their effects in cooling hot water.

I then tried whether the effects of hot water and hot ☿ in heating cold water bore the same proportion to each other as those of cold water and ☿ do in cooling hot water.

Exp. 11. Hot water added to cold.

The cold water, which was nearly of the temper of the air, was put in the bottle inclosed in the pan, and the hot water put in a separate bottle wrapt up in flannel to prevent its cooling so fast as it would otherwise do. This bottle of water with a thermometer immersed in the liquor was gently agitated till the thermometer sunk to 138°. The thermometer was then taken out and the hot water immediately poured into the bottle of cold water.

At 1 minute the hot water was pourd in.	
At 4 minutes the mixture was at	91°
6 „ „	90½

The weight of the cold water was 5·606 and its heat 54½: the weight of the hot water was 5·59.

Exp. 12. Hot ☿ added to cold water.

This exper. was tried in the same way as the former.

At 48′ the ☿ was pourd in.	
At 51 the mixture was at	92½
54 „ „	92
57 „ „	91¾

The weight of the cold water was 5·606 and its heat 56°½.

The weight of the hot ☿ was 171·6 [oz.] and its heat 138°.

In these 2 exper. let us suppose the heats of the liquors the same as they were found by the thermometer, and let us correct the heats of the mixture by finding what their heat ought to have been according to their observed rate of cooling at the time of putting in the hot liquor.

Therefore in the 11th exper. 91°·5 was the corrected heat of the mixture; the effect of the bottle is equal to that of 1·419 of water. In the 12th exper. the corrected heat of the mixture is 93° and the ☿ heated the water as much as 5·7 of water would; and therefore the effect of water in heating water is 30·11 times greater than that of ☿.

Hence, I think, we may conclude that the effects of hot water and hot ☿ in heating cold water bear the same proportion to each other as their effects when cold do in cooling hot water.

This conclusion might have been deduced from the former experiments, provided it is granted that the rule which was shewed to hold good with regard to hot and cold water, holds also good with regard to mixtures of hot and cold ☿; namely, that the difference of heats of the hot ☿ and mixture is to the difference of heats of the mixture and cold ☿ as the weight of the cold ☿ to the weight of the hot. For from these experiments it was deduced that the effects of cold water and ☿ in cooling hot ☿ bore the same proportion to each other as their effects in cooling hot water. Let the effect of cold water in cooling hot ☿ or water be n times greater than the effect of cold ☿; then will the heat of a mixture of one part of cold water with any number of parts (as A) of hot ☿ be equal to the heat of a mixture of n parts of cold ☿ with A parts of hot, which by the postulation is equal to the heat of a mixture of n parts of cold water with A parts of hot, or of 1 part of cold water with $\frac{A}{n}$ of hot. Therefore $\frac{A}{n}$ parts of hot water have the same effect in heating one part of cold water as A parts of hot ☿, or 1 part of hot water has the same effect as n parts of hot ☿, which is the thing to be proven.

On the other hand, if it is granted that the effects of hot water and hot ☿ in heating cold water bear the same proportion to each other as their effects when cold do in cooling hot water and hot ☿, it follows that the above mentiond rule does also hold good with regard to ☿; and also that the effects of hot water and ☿ in heating cold ☿ bear the same proportion to each other as their effects when cold do in cooling hot water and ☿.

Exp. 13. Cold spirits of wine added to hot ☿.

At 44′ the ☿ in the bottle and pan was at	148°¼
47 ,, ,, ,,	145
50 ,, ,, ,,	142

The spirits were then put in.

At 53′ the mixture was at	103°¼
56 ,, ,, ...	102¼
59 ,, ,, ...	101½

The weight of the hot ☿ was 125·45 [oz.]: the weight of the spts. of wine 5·275 and its heat 54°.

The corrected heats of the ☿ and mixture are 141°·5 and 104°; whence, by comparing this exp. with exp. 9th in which the quantity of hot ☿ was exactly the same, it appears that the spirits of wine coold the ☿ as much as 123·62 of ☿ would have done, and therefore the effect of spts. of wine in cooling hot ☿ is 23·43 times greater than that of ☿.

By another exper. of the same kind tried with the same quantity of ☿ and 7·85 of spts., the effect of spts. of wine came out 21·65 greater than that of ☿; and by another experiment with the same quantity of ☿ and 5·852 of spts. it came out 22·82 times greater.

I then tried whether their effects in cooling hot spts. bore the same proportion to each other.

Exp. 14. Cold spts. added to hot.

At 55 minutes the spts. were at			142°½
58	,,	,,	140
61	,,	,,	137½

The cold spts. were then put in.

At 64 minutes the mixture was at			101°¾
67	,,	,,	101
70	,,	,,	100½

The weight of the cold spts. was 6·404 [oz.] and their heat 56° and the weight of the hot spirits was 6·412 [oz.].

Exp. 15. Cold ☿ added to hot spts.

At 50′ the spirits were at		...	143°¾
53	,,	...	140¾
56	,,	...	138

The ☿ then added.

At 59 minutes the mixture was at				101°¾
62	,,	,,	,,	101¼
65	,,	,,	,,	100¾

The weight of the ☿ was 147·562 [oz.] and its heat 55°¾: the weight of the spts. was 6·406 [oz.].

In the 14th exp. the corrected heats of the hot spts. and mixture are 137°·1 and 102°·3 and the effect of the bottle and pan is equal to 2·1 of spts. In the 15th exp. the corrected heats of the hot spt. and mixture are 137°·54 and 102°·17; wherefore the ☿ coold the spt. as much as 6·481 of spts. would have done; whence the effect of spts. in cooling hot spts. is 22·77 times greater than that of ☿.

Hence, I think we may conclude that the effects of spts. and ☿ in cooling hot spts. bear the same proportion to each other as their effects in cooling hot ☿.

By a mean of all the experiments the effect of spts. of wine in cooling hot ☿ and spts. seems to be 22·7 times greater than that of ☿, and, consequently, 1·326 times less than that of water.

I also made an exp. to see whether the effect of spts. cooling hot water bore the same proportions or not to that of ☿. But it must be observed that there is heat generated by mixing spts. of wine with water; *id est*, when water and spts. of wine of the same degree of heat are mixed together the heat of the mixture will be greater than that of the liquors before mixing. Wherefore it is necessary to find how much the heat generated is, which renders the expt. more complicated and less exact.

Exp. 16. Cold spts. of wine added to hot water.

At 39′ water at	153°¾
42 "	151½
45 "	149¼

[The spts. of wine were then put in.]

At 49′ the mixture was at	123
52 " "	122½
55 " "	121½

The weight of the hot water was 9·088: the weight of the spt. was 7·631 and its heat 60°.

Exp. 17. Some warm spts. of wine were put in the glass bottle inclosed in the tin pan and some warm water was kept ready in another bottle wrapt in flannel with a thermometer in it.

At 52′ the spts. were at	114°¾
55 " "	113

The warm water was then immediately put in.

At 59′ the mixture was at	121°½
62 " "	120¼
65 " "	119⅓

The weight of the spts. was 7·587 [oz.]. The weight of the water added was 9·175 [oz.] and its heat at 55 minutes was 117°¾.

The corrected heat of the spt. in this last expt. is 112°·71, and, supposing the warm water to have coold as fast as the spts. its corrected heat is 117°·46, the corrected heat of the mixture is 122°·81.

If we suppose that the effect of warm water in heating spts. is 1·326 times greater than that of spts., the heat of the mixture would be 112°·71 if no heat was generated by the mixing, and as the difference of heat of the 2 liquors is so small, namely, only 4 degrees, the heat would not be much different though the proportion of the effects of water and spts. in heating spts. was considerably different from what we suppose: therefore the mixture was 7°·46 hotter than it would be if no heat was generated.

In the 16th experiment the corrected heats of the hot water and mixture are 148°·88 and 123°·88, and as the quantity of water and spts. is very nearly the same in this exp. as [in] the other, we may suppose that the heat of the mixture in this expt. as much exceeded what it would be if no heat was generated as it did in the other. Therefore the heat of the mixture would have been 116°·42 if no heat had been generated. This heat is the same as the heat of the mixture would have been if instead of the spts., 5·948 of water had been added. Therefore, allowing for the heat generated by the mixture, 7·631 of spt. cool water as much as 5·948 of water and therefore the effect of spts. in cooling water is 1·283 times less than that of water or 23·46 times greater than that of ☿. Therefore as far as can be determined by this exper. the effects of spt. and ☿ in cooling hot water bear the same proportion to each other as their effects in cooling spts. or ☿.

It should seem, therefore, to be a constant rule that when the effects of any 2 bodies in cooling one substance are found to bear a certain proportion to each other that their effects in heating or cooling any other substance will bear the same proportion to each other.

If this rule is true it is plain that in the foregoing experiments the effect of the bottle and pan in heating the cold liquor put in should be the same (or should be equal to that of the same quantity of water) of whatever nature the hot liquor in the bottle was. This, as far as can be expected, is conformable to experiment, for according to the 5th exper. the quantity to be allowed for the effect of the bottle and pan appeard to be 1·269 oz. of water; by the 7th exper. 1·241 oz. and by the 12th exper. it seemd 1·419 oz. According to the 9th exp. it appeard to be 39·21 oz. of ☿ or 1·303 of water; and by the 14th it appeard to be 2·099 of spts. or 1·583 of water. Though there is a considerable difference between the quantity of this allowance as found by the 7th and 14th exps., yet it does not seem greater than may be owing to the error of the exper., as this difference may be accounted for without supposing so great an error in the experiments as what is necessary to reconcile the 3 experiments in which cold spts. were added to hot ☿.

The true explanation of these phenomena seems to be that it requires a greater quantity of heat to raise the heat of some bodies a given number of degrees by the thermometer than it does to raise other bodies the same number of degrees.

I made some experiments also to find the effect of 2 or 3 other liquors and also of several solid bodies in heating or cooling other substances. The experiments upon the liquors were tried in the same glass bottle used for the former experiments by putting hot ☿ in the bottle and adding these liquors to it. Those upon solid substances were tried in a tin bottle nearly of the same shape as the glass one, holding 22·902 oz. of water and weighing 3·9 oz. and inclosed in the same tin pan as the glass bottle. The experiments were tried by putting hot water in the bottle and adding these substances to it. The heat of the solid substances was determined by keeping the bottle in which they were containd in a pail of water till it was supposed that they were arrived at the same heat as the water. In all other respects the experiments were tried in the same manner as the former. In order to find the allowance to be made for the effect of the bottle I made 3 experiments, the result of which is as follows:

	Weights of		Corrected heats of			
	hot water	cold water	hot water	mixture	Heat of cold water	Allowance for effect of bottle and pan
			°	°	°	
1st expt.	12·935	9·137	153·75	108·5	39·5	1·
2nd expt.	15·892	6·112	149·75	120·5	40·5	·85
3rd. expt.	17·171	4·035	155·25	134·5	41·5	·694

The following tables contain the results of all the experiments I have made on this subject as well as those made with the glass bottle as with the tin one.

Experiments tried with glass bottle.

	Weights of		Corrected heats of			
Name of liquor added	hot ☿ oz.	cold liquor oz.	hot ☿	mixture	Heat of cold liquor	Effect of these liquors in heating and cooling others: the effect of water = 1
			°	°	°	°
Saturated solution of sea-salt	125·46	5·506	145·46	105·37	57	$\frac{1}{1·21}$
Solution of pearl-ashes in 2ce their weight of water	125·46	5·5	154·13	111·12	54·5	$\frac{1}{1·32}$
Oil of vitriol mixed with an equal weight of water	125·46	5·5	156·63	115·58	57	$\frac{1}{1·43}$

In all these exper. I took care not to put so much of the solid matter, but what there was water enough above it to intirely cover the ball of the thermometer. These experiments, however, are much less exact than those with liquids as there was much more time spent in pouring in the solid substances than the liquids, and the thermometer was much slower in arriving at the heat of the liquor as I could not agitate the liquor about it so much; besides that, perhaps, that part of the liquor in which the thermometer was immersed was not exactly of the same temper as the rest of the water and the solid matters.

I made an attempt to find the effect of air in cooling water by blowing air from a smith's bellows through the worm of a still, and seeing how the water in the worm-tub was coold thereby. The nozzle of the bellows was fastend to the lower end of the worm and the ball of the thermometer was put into the upper end of it to see how much the air was heated in

Experiments tried with tin bottle.

Name of substance tried	Weights of hot water	Weights of solid substance	Corrected heats of hot water	Corrected heats of mixture	Heat of solid substance	Effect of substance in heating or cooling others
Iron filings	14·965	42	153°	128·5°	53·75°	$\frac{1}{8{\cdot}12}$
The same tried again	15·023	40·581	152¼	130½	52·75	$\frac{1}{9{\cdot}15}$
Lead shot	13·2	102·1	152	133·25	47·5	$\frac{1}{33{\cdot}6}$
The same tried again	13·354	102·03	156	136·5	50	$\frac{1}{32{\cdot}2}$
Tin reduced to lumps of about the size of peas	13·929	57·471	154·5	135·5	48·5	$\frac{1}{18}$
Silver sand	12·954	24·25	148	122·5	48·5	$\frac{1}{5{\cdot}1}$
White glass	11·904	28·892	148·25	126·75	50·75	$\frac{1}{8{\cdot}07}$
White marble	13·435	21·604	157	131·5	50·25	$\frac{1}{4{\cdot}856}$
Brimstone	12·756	17·502	150	131·25	50·75	$\frac{1}{5{\cdot}59}$
Newcastle coal	13·071	11·502	153·25	133	50	$\frac{1}{3{\cdot}42}$
Charcoal	13·583	3·987	156·25	148·5	49·5	$\frac{1}{3{\cdot}57}$

passing through it. The lower board of the bellows was confind in such manner that it could move only a given space so that the same quantity of air was forced into the upper partition of the bellows at each stroke. A mark was placed by the side of the upper board of the bellows, and as soon as the board sunk below that mark, a stroke of the lower board was given so as to keep the upper board always equally elevated. It was found by measuring the surface of the lower board, and the space which it was made to move through, that 283 cubic inches of air was forced into the upper partition at each stroke, supposing that no air escapes through the valve of the lower board. It was found also that only $\frac{955}{1000}$ of the quantity of air forced into the upper partition passed through the worm, the rest escaping by the valve of the bellows and through the pores of the leather as I found that in trying the experiment I was forced to give 1600 strokes of the bellows in 57·0 minutes so as to keep the upper board at the right elevation; whereas when the upper orifice of the worm was stopt up so that no air could pass through the worm, I was obliged to give 645 strokes in the same time.

The tub had a wooden cover to it, with a hole in it to put a thermometer in, and was furnished with a piece of wood by which I could stir the water without taking off the cover or taking out the thermometer. The exper. is as follows:

Time hr.	min.	secs.	Number of strokes given	Thermom. in tub	Do. in worm
				°	°
8	17	20	0	119½	118
	28	25	300	118½	117½
	46	20	800	117	116
9	14	30	1600	114¾	114

The water in the worm-tub was 2250 oz. and it was found by experiment that 323 oz. are to be allowed for the effect of the tub and cover and worm. The heat of the air in the room near the bellows was 52°. It was found that the water in the tub would have coold about 3°·79 in the 57 minutes that the experiment lasted if no air had been blown through the worm; so that it seemed as if the water was coold ·96° by blowing the air through.

The quantity of air blown through the worm was 216½ oz. and was heated 63° in passing through. Therefore it requires as great a quantity of heat to raise 216½ oz. of air 63° by the thermometer as to raise 2573 oz. of water ·96° and therefore the effect of air in heating and cooling other bodies is 5·51 times less than that of water.

By another exper. made in the same manner, its effect seemd 9·2 times less than that of water, but the quantity which the water was coold by blowing the air through the worm was so small in both these experiments that one can give but a very imperfect guess at how much its effect is.

Part II

As far as I can perceive it seems a constant rule in nature that all bodies in changing from a solid state to a fluid state or from a non-elastic state to the state of an elastic fluid generate cold, and by the contrary change they generate heat.

I shall first consider those cases in which bodies are changed from a non-elastic to an elastic state or from an elastic to a non-elastic state, and afterwards those in which they are changed from a solid to a fluid state or the contrary.

The reason of this phenomenon seems to be that it requires a greater quantity of heat to make bodies shew the same heat by the thermometer when in a fluid than in a solid state, and when in an elastic state than in a non-elastic state. It is plain that according to this explanation all bodies should generate as much cold in changing from a ∝ st. as they generate heat by the contrary change, which as far as I can perceive seems to be the case. There are 2 different ways by which fluids evaporate or are changed into the state of an elastic fluid, namely, first that species of evaporation which is performed with a less heat than that which is sufficient to make them boil, and which is owing to their being absorbed by the air; and, secondly, that species which we call boiling and which is performed independently of the air.

Dr. Cullen has sufficiently proved that most if not all fluids generate cold by the first species of evaporation. There is also a circumstance daily before our eyes which shews that water generates cold by the 2nd species of evaporation.

It is well known that water as soon as it begins to boil continues exactly at the same heat till the whole is boiled away, which takes up a very considerable time. No reason, however, can be assigned why the fire should not continually communicate as much or nearly as much heat to it after it begins to boil as it did when it wanted not many degrees of boiling, and yet during all this time it does not grow at all hotter. This, I think, shews that there is as much heat lost, or, in other words, as much cold generated by the evaporation as there is heat communicated to it by the fire. Thus, when the water is heated to the boiling-point, then as fast as it receives heat from the fire there is immediately so much of the water turned into steam as is sufficient to produce as much cold from the fire, so that the water is prevented from growing hotter, and, moreover, will not be intirely evaporated till it has received as much heat from the fire as there is lost by turning the whole of the water into steam. Whereas if no cold was produced by the evaporation, the water should either grow hotter and hotter the longer it boiled, or else it should be intirely converted into steam immediately after it arrived at the boiling-point.

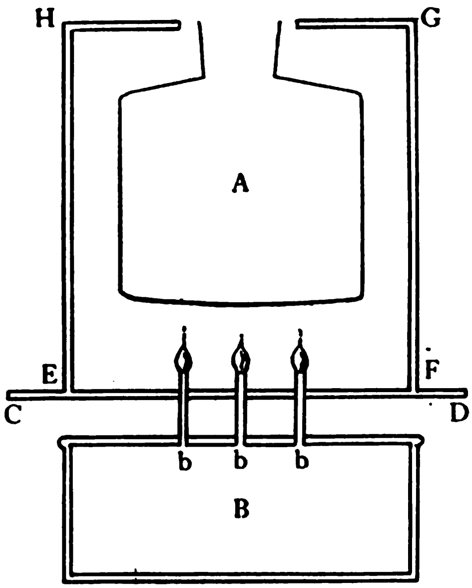
Perhaps it may be said that the cause of this phenomenon is that the steam is hotter than the boiling water, and by that means carries off the heat communicated to it by the fire. This, however, is by no means the case, as I have found by experiment that the steam is not at all hotter than the water it proceeds from. This I tried by putting a thermometer into a vessel of water inclosed on all sides, except a chimney to carry off the steam, the thermometer being placed so that very little of the ☿ appeard out of the vessel, when I found that the ☿ rose very nearly, but not quite to so great a height when the quantity of water was not sufficient to reach up to the ball of the thermometer as when it rose a little above it. But when the water was enough to rise a great way above the ball then the ☿ rose sensibly higher.

To understand this, it must be observed that the water near the bottom of the vessel will require more heat to make it boil than that above it, as being pressed by a greater weight. Now in all probability the water in all parts of the vessel is heated precisely to that degree which is required to make it boil, for it cannot be heated higher as in that case it would be instantly turnd into steam and if the water boils pretty fast it is not likely that it should anywhere be of a less heat than that. Consequently that part of the water adjoining to the ball of the thermometer would be a little hotter when the water rose a good deal higher than the ball than when it rose but a little above it. As the thermometer rose very near as high when exposed to the steam as when the water rose very little above it, the steam seems to be precisely of the same heat as the surface of the boiling water.

For the same reason we may conclude that all other liquors and also all solid bodies which are capable of being volatilized generate cold by being changed into an elastic fluid or vapor, for there is no substance but what takes up a great deal of time after it begins to distil strongly before it is all driven over.

I made some exper. on this principle, namely by heating some water over a lamp and finding how fast the water heated before boiling and how long it was a given quantity, for determining the quantity of cold produced by water in boiling all away (*sic*).

The apparatus is represented in the annexed figure, where *A* is a tin bottle for boiling the water in, the same that was used in the foregoing experiments. *B* is a spirit lamp furnished with seven small wicks: *bbb* are the wicks. *CD* is a brass plate, the use of which is to prevent the spirits in the lamp from being so much heated by the flame as they would otherwise be and *EFGH* is a tin frame surrounding the bottle and serving to keep in the heat. A thermometer was kept in the water all the while it was heating, and the space left between the mouth of the bottle and stem of the thermom. stopped up to prevent evaporation. When the water was near boiling the therm. was taken out and the mouth stopt. up again, all but a sufficient hole to suffer the steam to escape.



The results of the exper. are as follows. The bottle heated by 4 wicks of the lamp. [See Table on following page.]

The weight of the water before heating was 15·696 oz., it was taken off the lamp at $1^{\mathrm{hr.}}\ 40^{\mathrm{m.}}\ 15^{\mathrm{s.}}$ and was then found to have lost 1·469 in boiling.

It must be observed that on account of the length of the tube of the therm. which was out of the bottle the heat which was called 195° was in reality about [1]96°·1; that which was called 165° was 165°·7 and 95° was really what it appeard to be. Therefore, if we judge by the time in which the water rose from 95° to 195°, the quantity of heat communicated

EXPERIMENT I.

Heats shown by thermom.	Time hrs.	mins.	secs.	Difference secs.	Heats shown by thermom.	Time hrs.	mins.	secs.	Difference secs.
95°	0	55	2		155°	1	7	48	
				63					64
100		56	5		60		8	52	
				65					65
105		57	10		65		9	57	
				65					63
10		58	15		70		11	0	
				63					68
15		59	18		75		12	8	
				62					70
20		60	20		80		13	18	
				66					65
25	1	1	26		85		14	23	
				61					70
30		2	27		90		15	33	
				63					68
35		3	30		95		16	41	
				64					68
40		4	34		200		17	49	
				64					66
45		5	38		205		18	55	
				65					65
50		6	43		210		20	0	

to the water in seconds was just sufficient to raise it 1°; if we judge by the time in which it rose from 165° to 195° the heat communicated to it in was just sufficient to raise it 1°. From the rate at which the water heated, I think we may conclude that it began to boil at 1.20.22 consequently it continued boiling 19$^{m.}$ 53$^{s.}$. If we suppose that the lamp communicated as much heat to the water while boiling as it did before, and if we estimate that from the time in which it rose from 95° to 195°, it follows that the heat lost by converting 1·469 of water into steam was as much as would raise 15·696 of water degrees by the thermom. Consequently there is as much heat lost by converting any quantity of water into steam as is sufficient to raise that quantity of water 982°, or, in other words, there are 982° of cold generated by converting water into steam. If we estimate the quantity of heat communicated to the water by the time in which it rose from 165° to 195° the quantity of cold produced is 960°.

Exp. 2. Repeated in the same manner.

Exp. 3. Tried in the same manner except that the water was heated by 7 wicks.

Exp. 4. Tried in the same manner except that the water was heated by only 2 wicks.

Exp. 4 [5?]. The water being heated with 4 wicks but the cover *EFGH* being taken off.

I have been informed that Dr. Black has observed that in distilling water, the water in the worm-tube is heated thereby much more than it would be by mixing with it a quantity of boiling water equal to that which passes through the worm. Upon this principle I made some experiments to determine how much heat is generated by converting water from the state of an elastic to that of a non-elastic fluid.

The still used in these experiments was of copper of the usual shape and held about oz. or gallons. The worm-tub was coverd with a wooden cover made to fit close by means of list: it had a hole in it by which I could let down a thermom. into the water and had a piece of wood fastend to it by which I could stir the water without taking off the cover. The tub held upwards of 2300 oz. and weighd 712·2: the worm weighd 269·5, and the cover and stirrer 156. The thermom. was kept in the water all the while the experiment was trying and care was taken to immerge it to such a depth that very little of the ☿ in the tube should reach above the cover.

SKETCH OF THE OTHER EXPERIMENTS

The heat produced by the condensation of the vapours of boiling water by a mean of several experiments tried in the foregoing manner was about 920°. So that it seems likely that there is just as much heat produced by the condensation of steam into water as there is cold by the changing of water into steam.

An attempt was made to find whether any cold was generated by the emission of fixed air in dissolving alcaline substances in acids. The way I tried it was by finding how much more heat was produced by saturating sope-leys [potash solution], spirits of sal ammoniac made with lime [ammonia solution] and lime slaked with water (all which substances contain no fixed air) with spirit of salt than by saturating the same substances saturated with fixed air; that is solutions of pearl ashes [potassium carbonate], the mild spirits of sal ammoniac [ammonium carbonate] and whiting [calcium carbonate] mixed with water in the same acid. By a comparison of the experiments it seemed that the cold generated by the emission of the fixed air was sufficient to heat a quantity of water equal in weight to the fixed air emitted about 1000 or 1700 degrees.

EXPERIMENTS TO SHOW THAT BODIES IN CHANGING FROM A SOLID STATE TO A FLUID STATE PRODUCE COLD AND IN CHANGING FROM A FLUID TO A SOLID STATE PRODUCE HEAT

With regard to water and ice this may be proved by the same kind of argument as was used in page 34 [of the MS.] from the long time required to thaw ice or to freeze water. There is also a very curious experiment (I believe of Mairan's[1]) which shews that heat is produced by the freezing of water. Put a thermometer into a vessel of water, shut it up close from the air, and expose it to the cold. It will bear cooling some degrees below the freezing point without freezing: then on agitating the water it will begin to freeze and the thermometer in it will immediately rise to the freezing point.

It is evident according to this hypothesis that on the water beginning to freeze, the thermometer in it ought to rise and should continue to rise to the freezing point unless the water was coold so much below the freezing point as to harden into solid ice before it was heated up to the freezing point, and it is evident that it ought not to rise higher than the freezing point for when it is come to that point the water will cease to freeze.

What is the cause that water bears to be coold below the freezing point and then immediately begins to freeze on agitating it or dropping a little bit of ice into it, etc.? I do not at all know, but there are many other instances in nature of things of a like kind.

I made an experiment to determine the quantity of cold produced by the changing of snow into water. It was done by dissolving a given quantity of snow in warm water. The cold produced seemed to be about 170 degrees[2]. There seemed no difference between the cold produced by snow and by the same quantity of ice.

It is well known that on mixing salt or many other substances with snow or ice the snow dissolves, and a great increase of cold is produced. There can be no doubt that this increase of cold is owing to the melting of the snow. I made some experiments to determine the quantity of cold produced by mixing snow with the following substances; namely, a solution of sea-salt, a solution of pearl ashes, spirit of wine and *aqua fortis*. The

[1] [M. de Mairan was Secretary of the French Academy and wrote a number of philosophical treatises; among them Dissertations on Ice, on Phosphori, and on the Aurora Borealis. He died in 1771, aged 93.]

[2] [This is a fair approximation to the truth.]

quantity of cold generated was not very different from that produced by dissolving snow in warm water.

I find also that cold is generated by the melting and heat by the hardening of spermaceti. The cold produced by the melting of spermaceti is sufficient to cool a quantity of water equal to it in weight about 70 degrees, and nearly the same degree of heat is pı oduced by the hardening of spermaceti. It was tried by putting cold spermaceti into hot water and hot spermaceti into cold water. Spermaceti in cooling loses its fluidity at the heat of about 115°[1]. There is very little difference between the heat at which it ceases to be perfectly fluid and that at which it begins to be not at all fluid.

[Addendum.] Heat with which bees-wax melts. Therm. being immersed in melted bees-wax and kept constantly stirred about, the time[s] by the watch at which it arrived at different heats were as follows: heat of room supposed near 60°.

	min.	secs.		
	39	15	200°	
1.30	40	45	190	
2.0	42	45	180	
2.25	45	10	170	
2.45	47	55	160	
1.20	49	15	155	
1.40	50	55	150	Began to harden round edges
1.45	52	40	145	
1.50	54	30	143	
2.0	56	30	142+	A great deal hardend, remainder rather of a syropy consistence
8.30	65	0	142−	
12.0	77	0	141	
8.0	85	0	140	Much the greatest part hardend, remainder of consist. of very thick syrop
7.0	92	0	139[2]	

Some Tin and Lead were melted separately in a crucible and a thermometer put into them and sufferd to remain there till they were cold. The thermometer coold pretty fast till the metal began to harden round the edges of the pot. It then remained perfectly stationary till it was all congealed, which took up a considerable time. It then began to sink again. On heating the metal with the thermometer in it, as soon as the metal began to melt round the sides, the thermometer became stationary as near as I could tell at the same point that it did in cooling and remaind so till it was intirely melted.

On putting a thermometer into melted bismuth, the phenomena were the same, except that the thermometer did not become stationary till a good deal of the metal was hardend, unless I took care to keep the thermo-

[1] [Spermaceti melts between 106° and 120° F., depending on its purity.]

[2] [Beeswax melts between 144° and 151° and solidifies between 142° and 146° F. Cavendish's observations are therefore accurate.]

meter constantly stirring about. It then remaind stationary till it was almost all hardend. I do not know what this difference between Bismuth and the 2 other metals should be owing to except to its not transmitting heat so fast as them. I forbear to use the word conducting as I know you have an aversion to the word, but perhaps you will say the word I use is as bad as that I forbear. I did not suffer the thermometer to remain in the bismuth till it was hardend.

$$\text{The heat at which} \begin{Bmatrix} \text{Lead} \\ \text{Bismuth} \\ \text{Tin} \end{Bmatrix} \text{loses its fluidity is} \begin{cases} 612^\circ \\ 510 \\ 443^{1}. \end{cases}$$

All the following mixtures except the first differ considerably from the 3 simple metals in the manner in which they harden in cooling, as they begin to abate of their fluidity in a heat considerably greater than that in which they grow hard; whereas in the simple metals I could not perceive any difference between the heat in which they ceased to be perfectly fluid and that in which they hardend. In the trials I made the thermometer was kept constantly stirring about from the time the metal began to abate of its fluidity till there was a great deal hardend round the sides of the pot, and the rest had acquired as great a degree of stiffness as would suffer me to stir the thermometer about without breaking it, when I took it out. As soon as the metal began to abate of its fluidity the thermometer began to sink extremely slow in comparison of what it did before, and continued to do so till it was taken out, so that I think there can be very little doubt but what these metallic substances generate heat in hardening as well as the simple metals.

I think it seems likely that the reason why these mixtures begin to abate of their fluidity in a greater heat than that in which they harden is that the metals of which the mixture is composed begin to separate as soon as the heat is not sufficient to keep the mixture quite fluid. This is confirmd by the following experiment. The mixture of equal quantities of lead and tin was melted over again and sufferd to remain quiet till cold. It was then cut in two: the specific gravity of the upper piece was 8·801 and that of the lower 9·031, so that the upper piece appears to contain much less lead than the lower.

Mixtures	Heat at which it abates of its fluidity	Heat at which it grows rather stiff
lead 2, tin 3	351°	
„ 1 „ 1	379	362°
lead 1, bismuth 1	278	258
tin 1, bismuth 1	289	275
tin 1, bismuth 1, lead 1	275	251
„ 3 „ 5 „ 2	236	211

[1] [The true melting points of lead, bismuth and tin are variously stated by different authorities. Modern observations are, respectively, 617° F., 507° F. and 450° F. (Heycock and Neville). Both the melting and solidifying points are considerably affected by slight traces of foreign metals.]

THOUGHTS CONCERNING THE ABOVE MENTIOND PHENOMENA

There are several of the above mentiond experiments which at first seemd to me very difficult to reconcile with Newton's theory of heat, but on further consideration they seem by no means to be so. But to understand this you must read the following proposition.

[THE END]

BOILING POINT OF WATER

At the Royal Society, April 18, 1766

Experiments made to determine how much the height of the boiling point is affected by the water boiling fast or slow, or by the bulb being immersed in the water or only exposed to the steam when the whole thermometer, except a small part of the tube near the boiling point, is inclosed in a vessel of water shut up in such a manner as to leave no more passage than what is necessary to carry off the steam and consequently when almost all the ☿ in the tube is nearly at the same heat as that in the ball. The experiments tried first by a very quick thermometer with a cylindrical bulb about 2½ inches long, the top of the bulb about 6 inches below the boiling point. The brass scale to it being divided to 20ths of an inch and furnishd with a nonius. 1·175 inches on the scale answering to 3° on the thermometer, or 1 inch being equal to 2·55 degrees.

		Division on scale	Difference in degrees
The water reaching not quite to the bottom of the cylinder and boiling	very gently	1·43	0
	pretty fast	1·425	+ ·013
The top of the cylindrical bulb barely coverd with water boiling	gently	1·22	+ ·54
	faster[1]	1·255	+ ·45

The first column is the division on the scale which the thermometer stood at; the 2nd is the difference of height expressed in degrees, the mark + signifying that it stood higher. For example, it stood $\frac{54}{100}$ of a degree higher when just immersed in the water and boiling gently than when not at all immersed and boiling gently.

Experiments of the same kind made with another thermometer, the ball of which is 15 inches or about 250 degrees below the boiling point. A small

[1] It is not owing to any mistake in writing it down that the ☿ appears to stand a little lower when the water boild fast than when slow, as it was taken notice of at the time as extraordinary.

brass scale divided to 40ths of an inch being fastend to the tube to shew the alterations of the height of the boiling point. 10 degrees of the thermometer are equal to 22 divisions on the scale.

		Division on scale	Difference in degrees
The ball dipt a little way into the water in an open vessel filled almost to the top and boiling moderately, the tube held inclined so as to be as little heated as possible by the steam of the water		from 3	− 3·86
		to $4\frac{1}{2}$	− 3·18
Tried in a close vessel like that in which the former thermometer was tried, only deeper, namely 23 inches, with only 3 or 4 inches of water in it, so that the ball was out of water		$10\frac{3}{4}$	− ·34
The water rising 3 inches above the ball and boiling	gently	$11\frac{1}{2}$	0
	faster	$11\frac{5}{8}$	+ ·05
The water rising about 13 inches above the ball and boiling	gently	$12\frac{1}{2}$	+ ·45
	faster	13	+ ·68

The first of these thermometers was tried by me on April 16 in the same manner as above related.

Tried in the pot 23 inches deep water about even with the top of the cylinder of the thermometer or 19 inches deep and boiling	gently	1·32	+ ·13
	faster	1·30	+ ·18
Tried in the same pot with little water in it and consequently the cylinder a great height above the water boiling	gently	1·545	− ·45
	faster	D°	
In the shallow pot or that in which it was tried on April 18. Water just covering cylinder. Water boiling	gently	1·37	0
	faster	1·345	+ ·06
In the same pot with the cylinder intirely out of the water: water boiling	gently	1·53	− ·41
	fast	D°	

N.B. If the chimney was uncoverd the thermometer immediately sunk several degrees and rose to the same point as before on putting it on again.

Tried again the deep pot water rising a little above the top of the cylinder and boiling fast	1·32	+ ·13
The same pot: water rising $2\frac{1}{2}$ inches above the top of the cylinder and boiling fast	1·32	+ ·13

The 2nd of these thermometers had also been tried on April 17. A piece of paper with divisions on it answering to degrees being pasted on the tube. Tried in the deep-pot.

		Degrees on paper	Difference in degrees
Water not rising so high as the ball and boiling	gently	1·4	− ·55
	fast	D°	
Water rising a little above the ball and boiling	gently	1·95	0
	fast	D°	

		Degrees on paper	Difference in degrees
Water rising 12 inches above the ball and boiling	gently	2·5	+ ·55
	fast[1]	2·5 or	+ ·55 or
		2·7	·57

It appears from hence that if the vessel in which the thermometer is kept is sufficiently close:

First, there is scarce any sensible difference in the height whether the water boils fast or slow.

2nd. The thermometer stands about ½ a degree lower if the ball is exposed only to the steam than if it is immersed a little way in the water.

3rd. When the ball is exposed only to the steam there is no sensible difference whether it is raised a great way above the surface of the water or but a little.

4th. It stands about ½ a degree higher when the ball is 12 inches below the surface of the water than when it is very little.

5th. It stands rather higher if there is a great depth of water below the ball than if there is but little supposing the ball to be immersed in the water to the same depth in both cases.

Trials of boiling point of different thermometers at Royal Society

	Length	Degree at ball	
A thermometer of Bird marked *S*	...	− 40	213
„ of Adams	...	...	213¾
„ of Nairne	6	− 10	213 +
„ of Bird marked *SS*	9½	− 20	212
Large one of Ramsden's unfinished largest ball bott. mark	...	...	213 +
Bird's without mark	11¼	− 30	213½ +
Nairne's No. 5	15	− 70	215¼
— No. 3	16¼	− 50	213½
Ramsden's Mr C.	12·5	0	212⅓

Bar. in N[orfolk] L[ibrary] at	begin	30·12	
	end	30·11	mean 213·1
Therm. in D°		55	
Bar. in garden supposed		= 30·1	
Therm. in D°		= 69	

By means of all thermometers height 212° is same as if it had been adjusted in steam when bar. was 29·41.

[This little paper is of interest, not only from the intrinsic importance of the subject, which at that period in the history of thermometry was, of course, considerable, but also as an illustration of a custom which was then rapidly dying out, namely for the Fellows to make experiments in common

[1] The ☿ was unsteady, dancing slowly up and down.

in the Apartments of the Royal Society—one of their number being appointed to undertake the manipulative part, the operator in this particular case being Cavendish. In the early days of the Society this experimenting in common was the usual practice, more importance being attached to it than to the communication and reading of papers.

The "trials" in this case had their origin, probably, in an attempt to elucidate the cause of the difference in the indications of thermometers made by the best "artists" of that time, and which was suspected with good reason to be due to a diversity of practice in determining the upper fixed-point.]

THEORY OF BOILING

There are 2 species of evaporation: 1st that which is performed with a less heat than that of boiling water; and 2ndly that which is called boiling.

The first species of evaporation is owing intirely to the action of the air, and has been very well explained by Mr. Le Roy, who has shewn that air is capable of dissolving a certain quantity of water, just as water does salt; and that when it has acquired that quantity, it is incapable of dissolving more; and that the quantity which it can dissolve is different according to its heat. It follows from hence that water must evaporate very slowly in vessels almost closed or communicating with the outer air only by a long narrow pipe, unless it is heated enough to boil. For when the air within the vessel has absorbed as much water as it can dissolve, no more can evaporate till some more of that air is changed for fresh.

The 2nd species of evaporation or boiling may be performed without any assistance from the air. Its phenomena seem to depend on 4 principles. First, that water as soon as it is heated ever so little above that degree of heat which is acquired by the steam of water boiling in vessels closed as in the experiments tried at the Royal Society, is immediately turned into steam, provided it is in contact either with steam or air: this degree I shall call the boiling heat, or boiling point. It is evidently different according to the pressure of the atmosphere, or more properly to the pressure acting on the water.

But 2ndly, if the water is not in contact with steam or air, it will bear a much greater heat without being changed into steam, namely that which Mr. De Luc calls the heat of ebullition.

3rdly, steam not mixed with air as soon as it is coold ever so little below the degree of heat acquired by the steam of water boiling in vessels closed as above-mentiond is immediately turned back into water.

4th. There is a great quantity of heat lost by the changing of water into steam; and a great quantity of heat acquired by the condensing of steam into water.

My father many years ago made some experiments which seem not only to prove the truth of the 1st and 3rd principles, but also to show the heat of the boiling point answering to much smaller pressures of the atmosphere than can be found otherwise. Namely, he took a barometer with a ball at top and filled it well with ☿; he then introduced into it a small quantity of water well purged of air; on which the surface of the ☿ immediately stood considerably lower than it did before; though on inclining the tube so that the ☿ rose into the ball, the bubble of air left was very little greater than before the introduction of the water. The surface of the ☿ was not sensibly more depressed when the quantity of water introduced was large than when it was just visible, after making allowance for the weight of the water, and was always the same in the same heat of the air, but was very different in different heats; namely, when the air was at 78° it was 0·92 of an inch; when at 30° only 0·16 [1].

It appears from hence that when the pressure of the atmosphere on water is diminished to a certain degree (which is different according to the heat of the water) it is immediately turned into steam, provided its continuity is broke, that is, provided it is in contact with steam or air; and is immediately reduced back to water on restoring or increasing the pressure. Or, in other words, that whenever the heat of the water is increased ever so little above a certain degree (which degree is different according to the pressure of the atmosphere) it is immediately turned into steam; and is immediately restored back to its former shape on diminishing the heat. My father never tried this experiment with greater heats than those of the

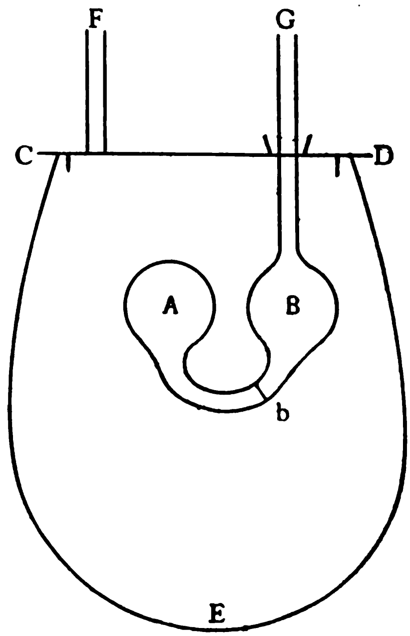
[1] [Among the Chatsworth MSS. is an interpolation table calculated by Cavendish, from the results of measurements made in conjunction with his father on the Tension of Aqueous Vapour, and which are referred to above. They appear to have been made about 1757 and are based upon a number of observations over a considerable range of atmospheric temperature and probably, therefore, at various seasons of the year. The following excerpt from the table shows the values as compared with the observations of Dalton and Regnault at the corresponding temperatures, and affords evidence of their degree of accuracy.]

Comparison of Lord Charles Cavendish's observations on Vapour Tension of Aqueous Vapour with those of Dalton and Regnault.

	Tension in inches of Mercury		
	Lord C. Cavendish	Dalton	Regnault
°F.	—	—	—
75	·84	·85	·87
70	·70	·72	·73
65	·58	·62	·62
60	·49	·52	·52
55	·41	·44	·43
50	·33	·37	·36
45	·28	·32	·30
40	·24	·26	·25
35	·20	·22	·20

atmosphere; so that though there was the utmost reason to think that this heat at which water was turned into steam was the same as that acquired by the steam of water boiling as above mentiond, yet there was no very direct proof of it. But since the writing of this paper I have made an experiment, which I think puts the matter out of doubt.

AbBG is a bent glass tube of the size of an ordinary barometer tube, with two balls *A* and *B*, each about 1½ inch in diameter. The ball *A* and the tube as far as *b* is filled with ☿, with a little water in the ball *A*. This glass is exposed to the steam of boiling water, or to the water itself in the glass vessel *CED*, coverd with the tin cover *CD*, with a chimney *F*; the tube *BG* being passed through a hole in the cover and secured with cork; and some woollen cloth being placed between the cover and the top of the glass vessel to make it fit the closer.



The event was that when the balls were raised above the surface of the water, so as to be exposed only to the steam, that then on giving the apparatus a little shake, so as to break the continuity of the water in the ball, the ☿ descended pretty quickly but not instantaneously, till it stood pretty exactly on a level in the 2 balls, where it remaind as long as the water continued boiling. But if the balls were sunk below the surface of the water, the surface of the ☿ in the inner ball *A* stood $\frac{6}{10}$ or $\frac{7}{10}$ below that in the ball *B*.

The circumstance of the gradual sinking of the ☿ in this experiment seems at first to disagree with that part of the first principle, which says that as soon as the water is heated above the boiling point it is *immediately* turned into steam. But in reality it does not; for as soon as any part of the water is turned into steam, the remainder must (by the 4th principle) be coold thereby; and consequently no more can be turned into steam till it has received so much heat from the steam or water surrounding the ball *A* as to make up that loss.

This experiment shows also, that the heat at which in my father's experiments the ☿ in the barometer is depressed a given depth, is the true heat which the steam of water boiling in vessels closed as above-mentiond, would acquire when pressed by that weight of the atmosphere. Suppose, for example, that when the heat is 61°, the ☿ in a barometer with a little water at top stands $\frac{1}{2}$ an inch lower than in one well filled, as is really the case; it follows that if it was possible to try the heat of boiling water in a place where the pressure of the atmosphere was only $\frac{1}{2}$ an inch, the steam would be found (supposing the vessels to be closed as above mentiond) to acquire a heat of only 61°.

In the above-mentiond experiment the water in the ball *A* was partly, but not imperfectly, purged of air; so that though a small shake was sufficient to break its continuity, yet as the quantity of water in the ball was small, and as that was deprived of great part of its air, the quantity of air in it bore an extreamly small proportion to the capacity of the ball; I think I may safely say not more than $\frac{1}{6000}$ part. This I found by putting the ball into cold water as soon as it was taken out of the steam, so as to cool it as expeditiously as possible, and consequently so as to leave as little time for the air to be absorbed as possible, and then before it was quite cold, measuring the diameter of the bubble.

But I made another experiment with a much greater quantity of air in the ball, namely such that its bulk when cold was about $\frac{1}{84}$ part of the space occupied by the vapour in the ball. I then found that when the apparatus was exposed to the steam of the boiling water, the ☿ in the ball *A* was depressed $1\frac{7}{10}$ inch below the level. It appears, therefore, that in this case the change of the water into steam was so much assisted by the absorption of the air that it was turned into steam, though the pressure was too great by $1\frac{7}{10}$ inch to have sufferd it to acquire that form without the assistance of the air.

In this experiment the quantity of air was found in a more exact manner; namely I pourd in ☿ till it rose above the ball *B* into the tube *BG*; and then found how much the surface of the ☿ in the tube was depressed by extricating the air.

The truth of the 3rd principle is confirmed by the experiment mentiond in the report of the Committee, p. 823 for year 1777[1]; namely that the heat of the steam in a pot 27 inches deep and $4\frac{1}{4}$ in diameter was not sensibly greater near the surface of the water, than near the top of the pot, that is at more than 18 inches above the surface. For if the steam could bear a less heat than that shewn in this experiment without being condensed into water, the steam at the top of the pot could hardly fail of being cooler than that in the lower part; whereas if this principle is true, this circumstance is not at all extraordinary; for while there is any steam remaining uncondensed, it must be as hot [as] at the boiling point.

As steam appears to be incapable of sustaining a degree of heat at all less than what I call the boiling point, or, in other words, as it is condensed

[1] [The Report of the Committee appointed by the Royal Society to consider of the Best Method of Adjusting the Fixed Points of Thermometers; and of the Precautions necessary to be used in making Experiments with those Instruments. *Phil. Trans.* Vol. LXVII. 1777, 816.]

into water the instant it is coold ever so little below that point; so in like manner it seems reasonable to suppose that water whose continuity is broke is incapable of sustaining a heat at all greater than the boiling point, or that it is turned into steam the instant it is heated at all above that point.

The truth of the 2nd principle has been sufficiently established by Mr. De Luc.

As to the 4th principle Dr. Cullen has sufficiently proved that heat is lost or cold generated by the first species of evaporation; and there is a circumstance daily before our eyes which shews that a vast deal of heat is lost by the 2nd species of evaporation. It is well known that water as soon as it begins to boil continues exactly or very nearly at the same heat till the whole is evaporated, which takes up a very considerable time. No reason, however, can be assigned why the fire should not continually communicate nearly as much heat to it after it begins to boil as when it wanted not many degrees of boiling; and yet during all this time it does not grow at all hotter. From hence we may conclude that there is a great deal of heat lost by the evaporation to compensate that communicated to it by the fire. For if no heat was lost by the evaporation, the water should either grow hotter and hotter the longer it boild, or else it should be intirely converted into steam immediately after it arrived at the boiling point.

On this principle I made some experiments to determine the quantity of heat lost by evaporation; namely by heating water in a metal vessel, in the form of a bottle, over a spirit lamp made so as to give as uniform a heat as possible and finding in what time it acquired a given number of degrees of heat before boiling and how much of the weight was lost by boiling a given time. It appeard that the quantity of heat lost by evaporation was 900 or 1000 degrees of Fahrenheit; that is as much heat was lost by evaporation as was sufficient to cool 100 times the weight of water evaporated 9 or 10 degrees.

Dr. Black found that the water in the worm tub of a still is heated much more than it would be by the addition of a quantity of boiling water equal to that distilled; and from thence computed the quantity of heat generated by the condensation of steam. I am not acquainted with the result of his experiment; but I repeated it myself and found the quantity of heat generated to be about 900°. So that there seems to be as much heat generated by the condensation of steam as there is cold by the production of it, and both to be about 900°.

From some experiments I have made the quantity of heat lost by the first species of evaporation seems to be much the same as by the 2nd species.

Mr. De Luc has also given a very clear proof that heat is lost by the 2nd species of evaporation in Art. 1062.

By the help of these principles the chief phenomena of boiling water may be readily explaind. When water is set on the fire and begins to boil, the lamina of water in contact with the bottom of the pot is heated till either small particles of air are detached from it, or till bubbles of steam are produced by ebullition. As these particles or bubbles ascend, the water in contact with them, if at all hotter than the boiling point is immediately turned into steam.

2ndly. These bubbles during their ascent through the water can hardly be hotter than the boiling point; for so much of the water which is in contact with them must instantly be turned into steam that by means of the production of cold thereby, the coat of water next to the evaporated coat of water, and which thereby comes in contact with the bubbles, is no hotter than the boiling point; so that the bubbles during their ascent are continually in contact with water heated only to the boiling point.

3rdly. Though the bubbles of steam can not be hotter than the boiling point, yet the water in general may be considerably hotter and most likely almost always is so in a small degree. For though the coat of water immediately in contact with the bubbles during their passage is not hotter than the boiling point, yet the rest of the water has not time to communicate much of its heat to that coat before the bubble is past. For this reason when the water boils with a vast number of small bubbles its heat ought in general to exceed the boiling heat less than when it boils with large bubbles succeeding one another slowly.

The excess of the heat of the water above the boiling point is influenced by a great variety of circumstances. The quantity of air in the water has a very great influence; for the more air it contains, the less heat will the water in contact with the bottom be capable of receiving, and the greater number of bubbles will be discharged. It is this which seems to be the reason of the difference between water beginning to boil and long boild, and between pump water and rain water.

Rain water contains only a small quantity of air and that common air, much the greatest part of which is discharged before the water begins to boil. But pump water besides this contains a calcareous earth suspended [dissolved] by the means of fixed air; and the calcareous earth detains the fixed air in such manner that it cannot be all separated without a vast deal of boiling.

It seems likely, I think, that the excess of the heat of the boiling water above the boiling point should be greater when the heat is less violent and applied to a greater surface than when more violent and applied to a less surface; and also when the application of the heat is more uniform, like that of oil, than when irregular like that of a common fire, that is acting with greater intensity on one point than another.

4th. It was before said that the bubbles of steam at their issuing from the water can hardly be hotter than the boiling point, and that if the vessel is properly closed, as in the experiments at the Royal Society, the steam can nowhere be colder than that point, and therefore the heat of the steam must be the same in all parts of the vessel.

5th. From what has been said it should seem that steam must afford a considerablymore exact method of adjusting the boiling point than water. I do not see indeed any cause which should produce an alteration in the heat of the steam except the vêssel being not shut sufficiently close and there being more or less air discharged from the water. To understand this it must be considerd that air is capable of dissolving a certain quantity of water, which is greater the greater is its heat. Now it seems likely that if the air is heated almost to the boiling point, it may be capable of absorbing a weight of water many times greater than itself. Suppose, for example, that air heated to within $\frac{1}{8}$ of a degree of the boiling point is capable of dissolving 100 times its weight of water; and suppose that the quantity of air discharged from the boiling water is $\frac{1}{100}$ part of the weight of the steam; or, in other words, that the vapours discharged consist of 1 part of air to 100 of steam, I think it seems likely that so much more of the water in contact with the bubbles will be turned into steam than would be if no air was discharged, that the bubbles and coat of water in contact with them will be coold $\frac{1}{8}$ of a degree below the boiling point, instead of being exactly of that heat, as they would otherwise be; and consequently the heat of the steam in all parts of the pot will be $\frac{1}{8}$ of a degree below the boiling point[1].

As to the heat of the water, it will be less on 2 accounts: 1st as it will approach nearer to the heat of the bubbles as was before said; and 2ndly as the heat of the bubbles will be less.

If the cover does not fit close, but lets in a little air, and the vapours within the pot thereby consist of one part of air to 100 of steam, I think it seems likely that the heat of the steam should be $\frac{1}{8}$ of a degree below the boiling point, as before. For the steam will bear being coold to that degree without being condensed; and in all probability the evaporation from the surface of the water will be so much increased as to cool the steam to that point. But the heat of the water ought in all probability to be very little affected thereby, except close to the surface.

But if the vessel is quite open, the surface of the water will most likely be many degrees cooler than the boiling point; and therefore it is likely that the water may be sensibly cooller than the boiling point, even to a considerable depth below the surface.

It should seem, therefore, that the heat of the boiling water should be considerably more regular in close vessels than in open ones. For the vessel being close or open can affect the lower parts of the water no otherwise than by the effect which it has on the water at the surface. Now in close vessels the water at the surface must in all cases be exactly at the boiling point; but in open vessels it will be much cooller, and its heat will be very different according as the surface is more or less exposed to the air, and as the water boils faster or slower.

[1] What is said in this paragraph is confirmed by the experiment in which a good deal of air was left in the ball *A*.

The hypothesis which I said I had about the cause of the difference of the heat of boiling and ebullition is as follows.

I suppose that within a certain very minute distance the particles of water repel each other; and that beyond that distance they attract each other; and that the repulsive force increases and diminishes as the heat increases and diminishes; but that the attractive force is either the same in all heats, or that it diminishes as the heat increases, or at least that it increases in a much slower proportion than the repulsive force; and also that the distance to which the attraction extends is many times greater than that to which the repulsion extends.

Thus let *PAap* (fig. 2) represent the section of a flat plate of water. Let *Nn, Mm* etc. represent parallel planes. Let *MG* and *GF* be equal to the distance to which the repulsion extends; and let *GN* and *MeE* equal that to which the attraction extends; that is, let a particle be repelled by any particle whose distance from it is less than *GM* or *GF*, and attracted by any whose distance is between *GM* and *GN* or between

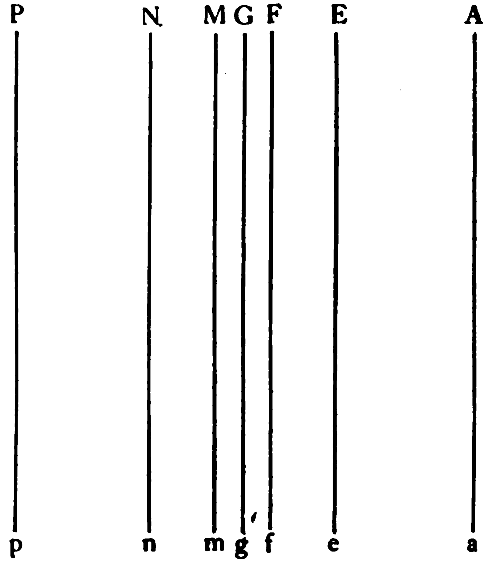


Fig. 2.

GF or *ME*. Then does the repulsion of the thin lamina *GFfg* on *GMmg* tend to separate the part *GAag* from *GPpg*; but it is counteracted by the pressure of the atmosphere on *Aa* and *Pp*; and also by the attraction of *GEeg* on *MNnm*. Suppose now the part *FAaf* to be taken away, so that the plane *Ff* may now become the outside surface of the water. Then is the force which tends to separate the lamina *GFfg* from *GPpg* the same as before; but the force which counteracts it is less; for the counteracting force is only the attraction of *GFfg* on *MNnm*, added to the weight of the atmosphere; and the attraction of *GFfg* on *MNnm* is evidently less than that of *GEeg* on the same. Whence it appears that the repulsion of *GFfg* and *GMmg* on each other may be sufficient to separate *GFfg* from *GPpg*, when *Ff* is the outside surface of the water, though it is not when there is a sufficient thickness of water on each side of it; and consequently that the repulsive force of the particles may be sufficient to separate a thin lamina from the outside of the water, and turn it into steam, though it is not sufficient to separate the water in the middle.

The phenomena of capillary tubes shew that the attraction of water to glass, and in all probability to metals also, is greater than that of water to water; so that it should require a still greater heat to produce a separation in that part of the water which is in contact with the sides of the pot, than in that which is at a little distance from it.

TENSION OF AQUEOUS VAPOUR

Among the Cavendish manuscripts concerned with heat is an account of a long and elaborate investigation on the relation between the temperature and elastic force of steam. The account is complete, and, together with certain associated matters, is written out in great detail on about 85 quarto folios. The inquiry involved many hundreds of measurements and considerable labour in reduction and computation.

As already stated, the subject was among the earliest of Cavendish's experimental inquiries. It would appear that the observations which he associates with his father were, as a matter of fact, made by himself under the direction, presumably, of Lord Charles Cavendish. They consisted in comparing the height of an ordinary barometer with that of a barometer containing a small quantity of water, placed side by side, and noting at the same time the temperature of the mercurial column—a method subsequently employed by Kämtz in observations extending over some years. In both sets of experiments the range was, of course, confined within the extremes of atmospheric temperature.

Although this is nowhere explicitly stated by him, it may be surmised that Cavendish was led to take up the subject again, not merely on account of its scientific interest, but by reason of the rapidly growing importance of the steam engine, and the desirability of obtaining accurate data concerning its theory. As a historical fact the problem was then engaging the attention of many experimenters, although, in all probability, Cavendish was unaffected by that circumstance, even if he was aware of it. Watt had attacked it as far back as 1764 and had made a series of observations in the winter of 1773–74 but nothing was publicly known of his work, and no account of it appeared until much later when he communicated the results to Robison who published them in his *Mechanical Philosophy*, Vol. II. p. 29, *et seq.* Robison's own results were first stated in his article on Steam in the *Encyclopædia Britannica.* Zeigler's observations made with a Papin's digester, and published at Basle in 1759, were probably unknown to Cavendish who would certainly have compared the results with his own had he been aware of them. Betancourt's memoir *Sur la Force Expansive de la Vapeur* was not printed until 1792, and an account of Southern's experiments, made by direction of Watt, first appeared in Brewster's edition of Robison's Works.

There can be no question, therefore, that Cavendish's inquiry was wholly original and independent, and altogether uninfluenced by any previous or contemporary work on the subject. He seems to have begun the new inquiry in the spring of 1777. It occupied him during much of the following year and until the early part of 1779. The experiments are described under the two main sections of observations below and above 212° F., as the methods employed in the two series are dissimilar to a slight extent.

The observations above 212° F. are first dealt with. The arrangement used in this series is illustrated by the figure on [p. 356].

It is thus described: *AbBG* is a bent glass tube of the size of an ordinary barometer tube with two balls *A* and *B* each about 1½ in. in diameter. The ball *A* and the tube as far as *b* is filled with ☿ [mercury] with a little water at the top of the ball. This glass is exposed to the steam of boiling water, or to the water itself, in the glass *CED*, covered with the tin cover *CD*, with a chimney *F*, the tube *BG* being passed through a hole in the cover, and secured with cork, and some woollen cloth being placed between the cover and the top of the glass *CED* to make it fit the closer. A hole was also made in the cover through which one of the short thermometers used in the experiments for the boiling-point [p. 356] could be passed.

The capacities of the bulbs and tube were ascertained by calibration with known weights of mercury, and the results are embodied in a table showing the capacity of the tube at successive intervals of an inch from the top of the outer tube.

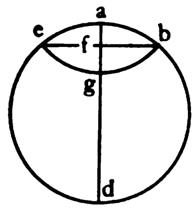
The calibration was necessary as the amount of mercury in the apparatus varied during the course of the experiments, more or less being added, or withdrawn from time to time, as required, in order to keep the mercury in the shorter or inner limb at approximately the same level, and also below the top of the outer tube.

Some preliminary experiments were made to determine the effect of small or varying proportions of air to vapour upon the tension, the observations being made at the boiling point of the water in the "pot," which was heated, in this case directly over the fire, or in a sand tray. The difference of level in the limbs was estimated by means of "a level ruler, with small sliding brass rule." The volume of the air and its ratio to the known content of the bulb were attempted to be ascertained by measuring the size of the bubble left after the apparatus was cooled by immersion in cold water, according to "the rule given in 'Experiments on Barometers'[1] for finding the bulk of bubbles."

Cavendish was aware that the method might not be strictly applicable to this case. He says: "But it must be observed that that rule was found by experiments in which no water was in the ball, and therefore can not hold good in experiments in which there is."

The bulk of residual air would be affected also to a small extent by its solubility in the cooled water in the bulb, but as the quantity of water was very small, its effect on the bulk was probably not greater than the unavoidable error of measurement. A number of experiments were made and the results, although only approximate, when combined with the known effects of determinate volumes of air in depressing the barometer, afforded some information as to the influence of varying amounts of air

[1] The rule here alluded to is given as follows in a short paper in MS. headed "Barometers."



Let the figure represent the section of the ball and bubble. The curvature of *egb* may be supposed nearly the same whatever is the diameter of the globe provided *eb* is small in respect of *ad*. The content of *eabf* is to that of globe as $3fb^4 : ad^4$.

If ratio *efgb* to 1 glob. inch be called *r*

$$fg = \frac{r}{3fb^2}.$$

1 glob. inch ☿ = 1800 grains.

Hence it appears that when diameter of bubble $= \begin{cases} \cdot 66 \\ \cdot 44 \\ \cdot 245 \\ \cdot 16 \end{cases}$

$$efba = \begin{Bmatrix} 53\cdot5 \\ 10\cdot6 \\ 1\cdot0 \\ \cdot185 \end{Bmatrix} \text{ grains and } efgb = \begin{Bmatrix} 17\cdot4 \\ 10\cdot2 \\ 2\cdot53 \\ \cdot833 \end{Bmatrix} \text{ grains}$$

$$fg = \begin{Bmatrix} \cdot03 \\ \cdot039 \\ \cdot0315 \\ \cdot0241 \end{Bmatrix} \text{ inch.}$$

Hence I conclude that the value of *gf* answering to bubbles of different diameters is as follows:

diameter bubble	·10	·13	·16	·2	·25
gf	·018	·021	·024	·028	·032
diameter bubble	·3	·4	·5	·6	·7
	·037	·039	·039	·034	·028

The number of grains of ☿ equal in bulk to bubble

$$= 1350 \times fg + eb^2 + 337 \times \frac{eb^4}{ad},$$

eb and *ad* being expressed in inches; and the bulk of the bubble is to that of the ball as

$$\frac{3fb^4}{ad^4} + \frac{3fb^2 \times fg}{ad^3} \text{ to } 1.$$

on the apparent elastic force of steam. It will be seen later that Cavendish subsequently detected how the effect of the air could be determined, thereby anticipating a discovery we associate with Dalton.

The observations for measurement of vapour tension were now begun. A small quantity of water, "well purged of air" by boiling, was introduced into the bulb *A*, and the apparatus immersed in a solution of pearl-ash, contained in the vessel *CED*. "The heat was estimated by the standard thermometer *S* of Nairne." It had been standardised by the method prescribed by Cavendish's Committee of the Royal Society already referred to. The readings of the temperature were also corrected, when necessary, for that portion of the stem of a lower temperature than that of the bulb, by the method introduced by Cavendish and adopted in the Report of the Committee (*Phil. Trans.* 1777, LXVII. 816).

"The different height of the ☿ [mercury] in the tube and in the ball *A* being estimated by the weight of ☿, by means of the foregoing measures," i.e., the measurements embodied in the calibration table before mentioned.

The following excerpts from the tables of details of the first series of observations will serve to illustrate their character.

May 6, 1778. Barometer = 29·93.

	Weight of ☿ in apparatus	Heat, ° F.	Level of ☿ in tube below top	Level of ☿ in ball below top of tube (1)	Force with which vapour is compressed (2)
	16·968 oz.	229·6	17·1	29·854	42·559
	,,	230	16·8	·872	42·877
tried again	18 oz.	229·1	17·2	·710	42·315
	,,	231·8	15·6	·803	44·008
	,,	232	15·35	·819	44·274

N.B. (1) Expansion of ☿ by heat is allowed for in this computation.

(2) About 7½ inches of the column of ☿ by which the vapour was compresse was heated to 212°, in consequence of which the pressure of that column was diminished about ·125 inch, according to De Luc's rule, which is allowed for in this computation.

July 4 [1778]. The same experiment repeated with a stronger solution of pearl-ash. Bar. = 30·08.

	Weight of ☿ in apparatus	Heat ° F.	Level of ☿ in tube below top	Force with which vapour is compressed
	24·812 oz.	247·4	·95	58·59
		247·7	·64	58·91
		248	·4	59·16
The solution in *CED* weakened by addition of water		246	2·5	56·95
		246·3	2·05	57·43
		246·5	1·85	57·64

The whole quantity of ☿ in the tube was much heated: perhaps to 120°.

For the higher temperatures an oil-bath was employed. The oil was contained in a copper vessel provided with a cover.

"The oil," we read, "was stirred by a semi-circular horizontal tin-plate, filling near ½ the area of the pot, and raised and sunk perpendicularly. It was heated by a spirit lamp of 9 wicks, kept burning during the observations, more or less of the wicks being lighted so as to make the oil heat and cool slowly. The pot [*CED*] was encompassed with a piece of thin brass, covered with flannel and paper, forming a kind of flue. The 2 standard thermometers A. and Br. for 300° were placed in the vessel on different sides of the ball of the apparatus, their balls being on a level with the center of the ball of apparatus and near 4 inches below the cover of the pot. 2 thermometers were placed, the ball of one near the middle of that part of the column of ☿ in the standard thermometers which was out of the pot, the ball of the other near the middle of that part of the column of ☿ of apparatus which was out of the pot."

As the radiation from the heated "pot" would doubtless affect the volatility of the spirit of wine in the lamp below, which might prove troublesome and even dangerous, it was diluted with water so as to minimise this risk. "The specific gravity of the spirits used for the lamps when the thermometer was at 63° was ·8803 [circa 66 per cent. of alcohol by weight], the flame being found not to increase much by the heating of the spirits, when they were no stronger than that."

The following table shows the general character of the measurements. Readings of the thermometers and of the levels of mercury in the apparatus were taken as the temperature slowly rose and again as it fell on cooling and the mean taken, these being subsequently corrected for the lower temperature of the emergent columns.

Sat. Aug. 1, 1778. Bar. at 4.40 p.m. = 30·32, glass ☿ = 11 oz. 13 dwt. 15 gr., heat tube thermom. = 81°, heat column ☿ = 71°.

Time	Thermometers A.	Thermometers Br.	☿ below top	Pressure
hr. m.	°	°	°	
5.32 P.	230·7	230·5	15·75	44·050
33	1·3	1·	15·6	44·209
34	1·8	1·5	15·25	44·789
36	2·3	2	14·8	45·054
37	2·7	2·5	14·4	45·476
38	3·2	3	14	45·898
40	3·2	3	13·65	46·268
42	2·8	2·5	13·95	45·950
43	2·3	2	14·35	45·528
44	1·8	1·5	14·75	45·106

Heats 2°·5 in 6 min. = 1° in 2·4 mins.
Cools 1°·5 in 4 min. = 1° in 2·7 ,,

	A.	Br.
	°	°
Corr. for heat tube thermom.	1·94	2·14
Supposed true heat corrected	2·17	2·12

For the higher temperatures and pressures a similar apparatus was constructed with a much longer outer tube. It is distinguished in the account as the 11 foot tube and was subsequently employed in an inquiry on the thermal expansibility of air. It, together with the ball, was carefully calibrated by mercury as before, the details being set out at length in the account.

With this apparatus, we read,

the experiments were made just in the same manner as the foregoing, except that a tin cover was placed...about 3·8 inches above the top of the pot to prevent danger in case the glass [tube] was to break. A thermometer was placed to show the heat of that part of the thermometer and tube which was between the cover and pot, and another to shew the heat of that above the cover.

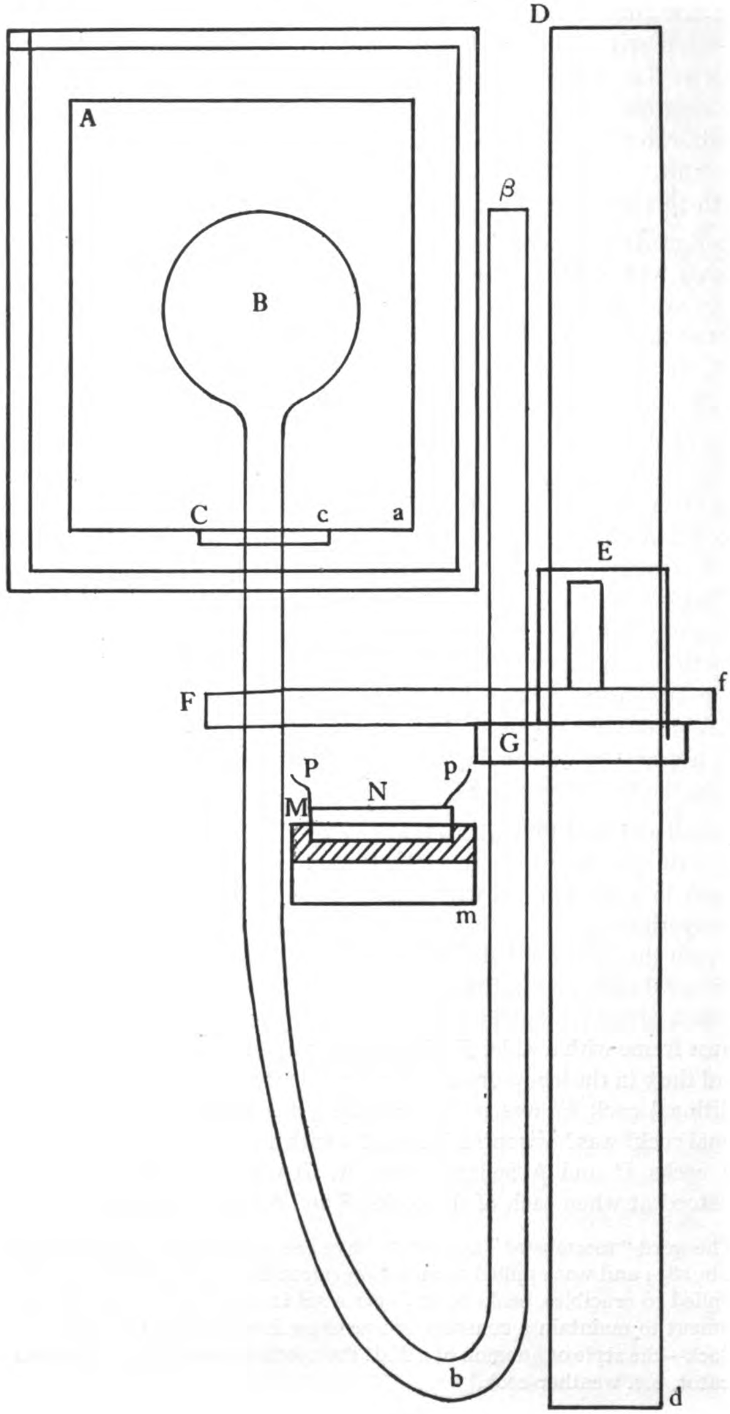
In all 17 series of observations were made involving several hundred readings of the thermometers and levels of the mercury. It is unnecessary to give further examples of their character: they were exactly similar to those already given. In each case a series of readings was taken at about the particular point determined upon first as the temperature slowly rose and again as it fell, a mean of the whole being taken. Thermostats were unknown in Cavendish's time[1]: his method of controlling the temperature of the bath was to extinguish one or more of the wicks of the spirit lamp. The methods of reduction and computation, and the details of corrections for inequalities of temperature, expansion of mercury, etc. are described in full and the final values are given in the form of synoptical tables.

The apparatus "for trying the force of steam at heats less than 212°" is shown in the figure on p. 368. It is thus described:

Bbβ is a ball and bent tube with a brass plate *Cc* cemented on it to which may be fastend a tin pot *Aa*, which, to make the containd water keep its heat longer, is inclosed in a wooden box with wool between. The same stirrer is used as in former experiments, and 2 thermometers are placed in it, their balls being on a level with the center of ball [*B*]. The tin pot is supplied with hot water from another vessel with a cock, the pipe of which enters into *Aa*. The vessel *Aa* also is furnished with a cock and a pipe to carry off the overflowings. *Dd* is a graduated brass frame with a slider *E* with a nonius and a brass cock *G*, to observe the height of the ☿ in the leg *bβ* or outer leg. In order to observe the ☿ in the inner leg, an additional cock *Ff* was used lying upon the former. To see whether this additional cock[2] was horizontal, a box *Mm* with a board *N* floating in ☿ was used with 2 cocks *P* and *p* fastend upon it. It was observed what division the nonius stood at when each of the cocks *P* and *p* were even with the top of the

[1] [The word "thermostat" appears to have been introduced into the literature by Heeren in 1834 and was applied to an arrangement devised by him for regulating the heat applied to crucibles, beakers, etc. over a spirit lamp. Its use in the sense of an arrangement to maintain a constant temperature is of much later date.]

[2] [Cock—the style or gnomon of a dial: the needle or index of a balance: a pointer or indicator, e.g. weather-cock.]


D
A
β
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C
c
a
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additional cock; the box was then turned end for end and the observations repeated by which means it was found how much one of the floating cocks was higher than the other, and how much that part of the additional cock, answering to one floating cock was higher than that answering to the other.

A slight scratch was made on the inner leg and its position on the graduated scale when the apparatus was in position was ascertained: its distance from a similar scratch near the top of the outer tube was thereby determined.

The bulb and tube were calibrated, as before, by mercury

"The degree of $\left\{\begin{matrix}52\\50\end{matrix}\right.$ on thermometers $\left\{\begin{matrix}L\\S\end{matrix}\right.$ [the thermometers employed] was even with the tin cover and 78 even with the top of the wooden cover. It is supposed, therefore that each thermometer was of the full heat as far as 65 [degree mark], and from thence of the temper of the air." When the apparatus was set up and "the ball and tube well filled [with mercury] without any water in them on Dec. 9, the ball and tube was measured [calibrated] and at the same time it was tried how well it was filled. The heat during the experiment was 50½. The height of the barometer did not alter sensibly during the time."

By comparisons with the barometer, the depression from any vapour [or air] contained in the ball in one experiment "cannot much exceed ·0011 and in the latter ·0034."

After the introduction of the boiled water, observations were made, *de die in diem*, during the latter part of December, 1778, and the early part of January, 1779. Neither Christmas Day, nor New Year's Day, nor Sundays were allowed to interrupt the work. For some reason, which is not apparent, the greater number of the observations were made in the late afternoon and evening, and often far into the night, so that the readings must have been taken by candle- or lamp-light. At that period Cavendish's laboratory was over the stables of his father's house in Great Marlborough Street, and it is possible that the apparatus was so placed that the comparatively feeble light of a mid-winter's day was insufficient for the purpose.

As an example of the observations an extract may be given from the table of measurements made on Dec. 25th. A column is given showing the results of Lord Charles Cavendish's measurements (L.C.C.) at the corresponding temperatures.

Friday, Dec. 25, 1778.

	Thermometers				Tension	
Time	*L* °	*S* °	☿ in outer leg	Barometer	H.C.	L.C.C.
6.57 p.m.	71·6	71·7	4·690	30·781	·740	·747
7.2 ,,	71·3	71·5	·680	·782	·731	·739
8 ,,	62·3	62·3	·485	·783	·530	·532
13 ,,	62·2	62·3	·480	·784	·526	·530
27 ,,	52·3	52	·325	·785	·364	·368
32 ,,	52·3	—	·323	·786	·363	·368

As in the first series, the whole of the observations are set out in the form of tables, with all the details of the computations and corrections, so that each step of the work may be followed.

The following table, compiled from the final results as stated by Cavendish, shows the corrected values for temperature and tension, expressed, of course, in Fahrenheit degrees and inches of mercury. As an indication of their accuracy they are compared with Regnault's values.

Tables showing the tension of Aqueous Vapour between 11° *and* 308°, *expressed in degrees Fahrenheit and inches of mercury, as observed by Cavendish, compared with the corresponding values determined by Regnault.*

° F.	Cavendish	Regnault	° F.	Cavendish	Regnault
11·0	·065	·071	122	3·573	3·621
12·0	·074	·074	132	4·690	4·755
22·2	·118	·119	142	6·074	6·183
26·8	·140	·145	152	7·816	7·925
31·0	·174	·174	162	9·934	10·07
36·7	·195	·216	172	12·531	12·74
44·8	·284	·288	182	15·647	15·92
52·0	·384	·388	224·2	37·56	38·19
62·0	·548	·556	229·4	41·30	41·90
71·6	·760	·762	234·0	45·03	45·76
82·0	1·067	1·094	239·3	49·57	50·29
92·0	1·467	1·500	244·3	54·21	55·16
102·0	1·998	2·039	248·6	58·37	59·42
112	2·683	2·725			

Tension of Aqueous Vapour at temperatures above 212° F.

"Experiments made with 11 foot tube. Observed heats (corrected) answering to different pressures: taken from a mean of the observations themselves" (Cavendish).

° F.	Cavendish	Regnault
239·05	50·20	50·08
248·72	59·64	59·52
258·31	70·87	70·31
268·38	83·02	83·32
277·97	97·14	97·40
287·88	113·37	113·91
298·22	132·22	133·33
308·07	153·25	154·40

The first approximately accurate measurements of the tension of aqueous vapour published in England were made by Dalton, by a method "recommended by an elegant simplicity." An account of them appeared in 1805 in Vol. v. of the *Memoirs of the Literary and Philosophical Society of Manchester*. They extend from the freezing to the boiling point of water under ordinary atmospheric pressure. From the rate of increase in tension between these limits Dalton computed the values at temperatures above 212° F., but, as Young from theoretical considerations, and Ure, from the results of direct observation, pointed out, Dalton's method of extrapolation was wholly erroneous, furnishing numbers some 25 or 30 per cent. too low at 50° or 60° above the ordinary boiling point. Cavendish's method, on the other hand, although far more laborious than that of Dalton, and involving many more measurements and much time and trouble in evolving the necessary corrections, afforded in his hands, as the comparison with Regnault's values proves, much more accurate results. This is especially true at the higher temperatures where the values were obtained by direct experiment, and are therefore independent of all extrapolation on hypothetical assumptions.

But, in the absence of other data, Dalton's *Tables of the Force of Aqueous Vapour*, in spite of the criticisms of Biot, were long regarded as authoritative, especially in this country, and as such appeared in practically every text book during the first third of the 19th century, when they were superseded by the work of Dulong and Arago. It is to be regretted therefore, that Cavendish should have refrained from publishing the results of his labours of 1777–1779 on this subject. "Erroneous observations," said Darwin, "are in the highest degree injurious to the progress of Science, since they often persist for a long time. But erroneous theories, when they are supported by facts, do little harm, since everyone takes a healthy pleasure in proving their falsity."

It may be doubted, however, whether this last remark is applicable to Cavendish. Nowhere does he manifest "a healthy pleasure" in dealing with a false theory or unsound hypothesis. He studiously avoided controversy; he seemed, in fact, almost nervously afraid of it. In this, as indeed in other respects, he resembled Newton, who in consequence of the objections which were raised to his theory of light and colours, confessed that he had been imprudent in publishing it, since by catching, as he said, at the shadow he had lost the substance, namely his own peace and quiet. Throughout his long life there was only a single occasion on which Cavendish was, for a brief time, led into controversy when he was provoked to reply to certain strictures by Kirwan; and he is quite apologetic to the Royal Society for the necessity of troubling it with polemical matters. If we are driven to seek a reason for his withholding from publication the results of his long and laborious work on aqueous vapour tension, it may possibly be found in the fact that it would inevitably bring him into conflict with Deluc. As the manuscript shows, the direction of the investigation was to some extent affected by Deluc's theory of the thermometric scale, and the reduction of the observations is encumbered with calculations which prove the unsoundness of Deluc's views. Deluc's "rules" led to a progression of elasticity and a scale of temperature which were found to be wholly inconsistent with observation. Cavendish sets out the numbers establishing this fact, but he nowhere comments on the disparity between theory and experiment. He simply leaves the numbers to speak for themselves.

Deluc, as is well known, held that a mixture of equal weights of hot and cold water had a temperature lower than the arithmetic mean of the initial temperatures. Thus, according to Deluc, a mixture of equal weights of water at 212° F. and 32° F. had the temperature of 119° instead of 122°. Cavendish, for years past, was aware that such a statement was erroneous. His early experiments, made with all the precision and scrupulous care of modern methods, would have enabled him to refute it. But so far as can be gathered, he took no steps to make known the facts. He contented himself with satisfying his own curiosity in the matter. As it was, the error persisted down to the time of Dalton, who not only shared it, but sought to support it by the assumption of a false hypothesis concerning the law of the thermal expansion of liquids.

Accompanying the account of these experiments, and paged up with it, are descriptions of two other inquiries which, it may be presumed, Cavendish considered as connected with, or supplementary to, the main investigation, and which had this been published would, in all probability, have appeared at the same time. The descriptions are headed "Compressibility of Air" and "Expansion of Air." The experiments on the compressibility of air appear to have been made during the last ten days of November and first week of December, 1778, before the final set of experiments on aqueous vapour tension at temperatures below 212° were undertaken. The object of the inquiry, apparently, was to ascertain how far air saturated with moisture obeyed Boyle's law, and whether the partial pressures of the air and aqueous vapour could be differentiated.

The experiments on the thermal expansion of air were not made until the December of the following year.

COMPRESSIBILITY OF AIR

After former experiments with 11 foot tube were finished it was washed with water and on Nov. 20 [1778] was measured while wet. Thermometer about 57°. Thermom. about 57°.

Quant. ☿ [oz. dwt. gr.]			Weight of ☿ which by former measures would fill ball and tube to that height [oz. dwt. gr.]
9. 13. 22	in short leg, 2·52 from bott.		
10. 10. 16	to a point 2·00 from last		
11. 19. 18	in long leg 3·09 from bott.	= 127·51 from top	12. 0. 2
64. 12. 14	,, ,, ,,	= 9·13 ,,	64. 13. 13
67. 11. 18	,, ,, ,,	= 2·00 ,,	67. 11. 18

Top tube = 130·6 from bottom, bottom = 3·75 from mark.

On Nov. 21 about 7.30 P. all the ☿ was pourd out of tube and 1. 0. 17 of ☿ pourd in

surface water	1·09 above bottom
surface ☿ in short leg was	·89 ,,
surface ☿ in long leg	1·71 ,,
surface water in long leg	5·44 ,,

It was then set in large glass of water and at 11.0 P.

$$\begin{matrix}\text{Surf. ☿ in long leg was} \\ \text{Thermometer in water ,, ,,}\end{matrix}\left\{\begin{matrix}1{\cdot}72 \text{ above bottom.} \\ 5{\cdot}50 \quad ,,\end{matrix}\right.$$

Water being at that time 48°·3. The surface ☿ and water in short leg could not be measured but we may conclude from former measure that they were $\left\{\begin{matrix}1{\cdot}08 \\ {\cdot}83\end{matrix}\right.$ above bottom.

64 oz. 8 dwt. 9 gr. more ☿ was then pourd in making in all 64. 9. 2; and at 11.20 P. thermom. in water same as before.

$\left.\begin{matrix}\text{Surface ☿} \\ \text{,, water}\end{matrix}\right\}$ was $\begin{matrix}4{\cdot}00 \\ {\cdot}61\end{matrix}$ below top, and at 11.36 P. $\left\{\begin{matrix}4{\cdot}12 \\ {\cdot}60\end{matrix}\right.$ below top; thermom. being still the same as before.

Bar. at 12.10 P. (?A.) was 30·02 and next morning 29·98, heat ☿ therein being about 50°.

In the exp. with short col. empty space was = 10·298 oz. ☿: pressure was = press. atm. + ·84 + $\frac{3{\cdot}58}{13{\cdot}5}$ = 1·105. Height Bar. reduced to heat 57° was 30·04 and allowing for 10½ feet greater pressure 30·052. Therefore total pressure = 31·157 inches. Depression of barom. by water at heat 48°·3 = ·315. Therefore if all the moisture was separated from air it would have sustained pressure of 30·842, when of that bulk ☿ filling ball and tube to 4·12 from top = 66·707 oz. ☿ actually in = 64·454. Bulk water in short leg = ·081: therefore empty space in last trial = 2·17. The surface ☿ in ball when there is that quantity of empty space was found by trial to be 4·09 above bottom. Therefore pressure = pressure atm. + 126·48 + $\frac{3{\cdot}52}{13\frac{1}{2}}$ − 4·09 = 152·751, the press. atm. being 30·04, and therefore pressure which it would have sustained if perfectly dry 152·44. Therefore the empty spaces are to each other as 4·746 to 1, and the pressures as 4·943 to 1.

Dec. 1, 1778, the experiment was repeated.

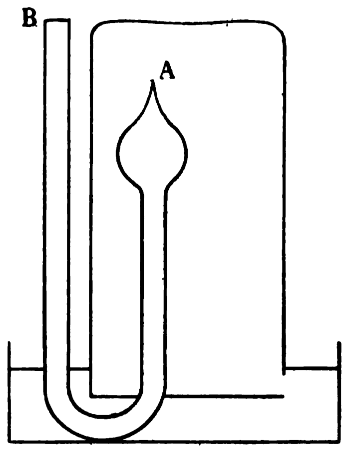
$\left.\begin{matrix}\text{Surface water} \\ \text{,, ☿}\end{matrix}\right\}$ in short leg = $\left.\begin{matrix}1{\cdot}6 \\ 1{\cdot}3\end{matrix}\right\}$ above bottom.

Surface ☿ in long leg = 1·68 above bottom, being then set in water surf. $\left.\begin{matrix}\text{☿} \\ \text{water}\end{matrix}\right\}$ in long leg = $\left.\begin{matrix}1{\cdot}63 \\ 2{\cdot}04\end{matrix}\right\}$ above bottom and consequently $\left.\begin{matrix}\text{water} \\ \text{☿}\end{matrix}\right\}$ in short leg $= \left.\begin{matrix}1{\cdot}65\\1{\cdot}35\end{matrix}\right\}$ above bottom, thermom. being 44° and bar. = 29·94. The ☿ in app. being then made = 59. 9. 17 $\left.\begin{matrix}☿\\ \text{water}\end{matrix}\right\}$ in long leg was $\left\{\begin{matrix}16{\cdot}28\\15{\cdot}87\end{matrix}\right.$ below top, thermom. being the same. The air was then got out of the ball without losing any of the ☿, the ☿ in apparatus being then made = 63. 6. 13 surf. water was 11·85 below top.

In the 1st experiment, empty space was 10·060 oz. Pressure was 30·23 ☿ filling ball and tube to 16·28 from top or 110·57 from mark is by 1st measure = 61·763, bulk water in short leg = ·084. Therefore empty space = 2·195. By expelling the air the empty space appeard = 2·203. The pressure was 140·20. The depression of the barometer is ·27; therefore the pressures which same quantity dry air would have sustained are as 139·93 to 29·96 or as 4·666 to 1 or as 10·060 to 2·156.

EXPANSION OF AIR

Dec. 19, 1779.



A bent glass like the figure was filled with ☿ by being placed in tin vessel with the capillary *A* and the end of leg *B* passing through holes in the cover. The air in the vessel was heated to about 90°. The ☿ was heated considerably hotter and was pourd in through a capillary funnel entering into *B*. When it was filled the capillary *A* was sealed and the greatest part of the ☿ in the outer leg pourd out. The glass was then fastend to the brass scale used in the preceding experiment [Tension of Aqueous Vapour], and the outer leg measured by pouring in ☿.

Glass empty = 7 oz. 8 dwt. 5 gr.

Division on scale answering to ☿ in outer leg	Glass filled with ☿ to that mark	Do. in decim. oz.	Diff. for 1 inch	Log. do.
2·895	29. 14. 22	29·746		
8·41	31. 12. 18	31·637	·343	9·5351
13·97	33. 9. 21	33·494	·334	·5237
20·59	35. 12. 8	35·617	·321	·5061

Heat [of] air about 50°.

Dec. 22. Some air was let into the ball in this manner: The ball was placed under a jar inverted to glass of water as in fig. the air being at 32° and the barom. at 29·13, and kept so for about an hour that the whole might be of the temper of the air. The glass jar was then taken off, the end of the capillary broke off[1] and the jar put on again. After it had staid so for about 15′ some ☿ was drawn out of outer leg by syphon and thereby some air let into ball. The jar was then immediately taken off and the capillary sealed. Measures were taken on different days with the apparatus fastend to the scale as before.

The height of ☿ in ball below the top of the capillary was taken by an additional cock lying on the other in the same manner as in the preceding experiment [on tension of aqueous vapour]. The height of the top of the capillary was found by resting a level ruler on the cock used for finding the height of the ☿ in the outer leg, and moving this cock till the ruler touchd the top of the capillary.

Day 1779	Glass with ☿ oz. dwt. gr.	Height ☿ in out. leg	Surf. ☿ in ball below capill.	Height top capill.	Heat
Dec. 23	16. 6. 16	6·76	2·41	18·225	38°
,, 24	20. 15. 13	11·435	2·22	18·223	36½
,, 25	22. 11. 0	13·535	2·155	18·225	32½
Jan. 1	24. 8. 4	16·26	2·095	18·216	37
,, 2	14. 5. 4	4·14	2·515	18·212	37
,, 6	22. 11. 6	13·71	2·14	18·20	37
,, 7				18·20	

Experiment made with this apparatus immersed in glass vessel of water 21½ inch. deep and 9 in. diam., the appar. being immersed to such a depth that the top of the ball was 2 or 3 inches under water. The surface of ☿ in outer leg and the divisions on scale being seen through the glass. A tin cover was fitted to the glass and the standard thermom. *S* let down through a hole in the cover so that its ball was on a level with the ball of the appar. The water was stirred in the same manner as in the preceding exper. and the glass was supplied in the same manner with hot water; the water in the glass was let off by a syphon. The brass scale was fastend firmly to a board by that part which reached above the vessel of water, and the vessel of water was put up without disturbing that fastening, so that the inclination of the brass scale to the vertical line (if any) was the same as before it was immersed in the glass vessel, and consequently the point of the scale on a level with the top of the capillary was the same as before it was immersed in the glass vessel.

[The actual measurements of the thermal expansion of the air were begun in the late afternoon of Dec. 25, 1779, and continued on that day until late at night. They were resumed on the following morning, and were continued on successive days until January 5th.

In principle the method was similar to that adopted in the measurements of vapour tension. It involved reading the heights of the mercury in the two limbs of the apparatus, and noting the height of the barometer

[1] The height of the ☿ in the outer leg was such that no air enterd into the ball thereby, but on the contrary some run out.

at each reading, together with the temperature of the water in which the apparatus was immersed, known weights of mercury being added or withdrawn when necessary, so as to keep the levels in the two limbs at convenient heights for measurement. The range of temperature was between 33° and 150°. All the details of the observations are given, together with those of their computation and reduction. Cavendish's object was not merely to ascertain the thermal expansion of air, but to determine the effect of aqueous vapour on its expansibility. Accordingly on January 6, after it had been ascertained that the weight of mercury in the apparatus agreed with the calculated amount to within 4 grains, some water "purged from air" was introduced into the apparatus containing the air, and on the evening of January 7, the observations were resumed and continued day after day until January 12, when the weight of mercury in the apparatus was checked as before. In this second series the range of temperature was from about 32° to 127°.

The results of these observations showed, to quote Cavendish's words, that "the expansion of air by 1° is $\frac{1}{500}$ of its bulk at $38°\frac{11}{12}$ or $\frac{1}{481}$ of bulk at 0° or $\frac{1}{511}$ of bulk at 50°."

He further found

> that the increase of pressure which perfectly dry air will sustain by the addition of water, supposing the bulk to remain unaltered, is equal to the depression of the barometer by water [vapour]. The depression of the barometer by water [vapour] at 32° = ·174. So that the increase of pressure which the air bore in this experiment, on the addition of water, fell short of the excess of the depression of the barometer at the observed heats above that at 32 by from ·04 to ·07, so that it should seem as if the moisture in the apparatus [and in the assumed dry air] before the addition of the water was sufficient to depress it from 4 to 7–100ths of an inch.

Of course, it need hardly be stated, attempts to determine the expansibility of air by heat had been made prior to the time of Cavendish. The necessity of knowing its amount and rate was perceived long ago, originally, no doubt, on account of the use of air as a thermoscopic agent, as also from the recognition of its bearing on astronomical refraction and on the determination of heights by barometric measurements. These attempts date from the end of the 17th century and are connected with the names, amongst others, of Amontons, Nuguet and La Hire in France, Hawksbee, Shuckburgh and Priestley in England, Musschenbroek in Holland, and De Luc in Switzerland. But the results obtained by these observers were widely different, partly from the imperfection of the methods employed, and partly from the unsuspected influence of moisture in the air or the apparatus. Cavendish's results, although only approximately accurate, are certainly nearer the truth than those of the majority of his predecessors, and compare not unfavourably with those of Dalton and Gay Lussac made nearly 30 years afterwards. Cavendish's values were doubtless affected by the circumstance that the air, although cooled to 32° F. before its introduction into the apparatus was not wholly free from moisture. This was recognised by him, as he clearly states, and he gives an approximate estimate of the amount of moisture which was probably present. He had no means of thoroughly drying the air, as desiccating agents other than dry pearl-ash and filter paper were unknown, or at least unused, at that period.

But the interesting fact clearly appears that he definitely recognised that the increase in volume and pressure of a given bulk of air, which may be due to the moisture contained in it, is equivalent to the tension of aqueous vapour at the corresponding temperature, as measured by the depression of the barometer, whence the volume of the dry air may be ascertained. He thereby anticipated by nearly a third of a century, a discovery which we associate with Dalton.]

Expansion of different airs by heat. The app. consists of a barometer, ball [bulb], and tube about inc. long[1] with a piece of a thermom. scale [of wood or ivory] fastend to it and inverted into a glass of ☿ [mercury] which is placed in jar of water in which the water by means of syphon is kept always at same height.

[He begins by assuring himself that the thermometer scale is accurately divided, by measuring lengths of successive intervals of 10°.]

Measure of divisions.

		Diff.
40° to 50°	·67 inc.	
		·66
60	1·33 ,,	
		·65
70	1·98 ,,	
		·65
80	2·63 ,,	
		·64
90	3·27 ,,	
		·63
100	3·90 ,,	

☿ [mercury] required to fill ball and tube to a given division.

	☿	Div.	Diff. of ☿	Diff. of div.
	6644	97·8		
	6828	73	184	24·8
	7070	41	242	32
Do. repeated.	☿			
	6648	98		
	6854	70		
	7076	40		

[1] The length is not stated. If measured, it was omitted to be inserted in the space left vacant for it.

Whence the capacity [in grain measures] answering to even divisions is as follows:

		Diff.			Diff.
100°	6634		60°	6928	74
		73			
90	6707		50	7002	74
		73			
80	6780		40	7076	74
		74			
70	6854				

These exp. agree with the first to less than 6 gra.

By mean, 1 inch of tube holds 111 of ☿. Diam. glass into which it is inverted varies from 2·63 to 2·65: by mean 2·64; and 1 cyl. inc. ☿ of that diam. = 18800, and therefore 1 inc. tube holds as much as ·0059 of the glass and 10 div. hold as much as ·00383 of glass.

When ☿ in tube is at 40, ☿ in glass is 1·03 below that divis. and the surface of the water in the large glass [jar] is 8 in. above the ☿ in the glass, and the pressure of this 8 inches = ·59 in. of ☿; and therefore when ☿ in tube is at 40, the press. on air is less than height barom. by 1·03 − ·59, or ·44 inc. and therefore pressure at different divisions is the less than height barom. by quant. in following table:

Div.	Quant.	Diff.	Div.	Quant.	Diff.
40	·44		80	3·09	66
		67			
50	1·11		90	3·73	64
		67			
60	1·78		100	4·36	63
		65			
70	2·43				

[Based on the means of the differences respectively in capacities and pressures, the foregoing tables are then expanded as follows:]

Div.	Capacity	Prop. parts	
100	6634	1	7·4
90	6707	2	14·8
80	6780	3	22·2
70	6854	4	29·6
60	6928	5	37·0
50	7002	6	44·4
40	7076	7	51·8
		8	59·2
		9	66·6
		10	74·0

Div.	Press.	Div.	Press.	Prop. parts	
40	·44	75	2·760	1	0·065
45	·775	80	3·090	2	·13
50	1·110	85	3·410	3	·195
55	1·445	90	3·730	4	·26
60	1·780	95	4·045	5	·325
65	2·105	100	4·360	6	·390
70	2·430			7	·455
				8	·520
				9	·585
				10	·650

[The "airs" experimented upon were: common air; nitrous air (nitric oxide); fixed air (carbon dioxide); heavy inflammable air (presumably gas from charcoal); dephlogisticated air (oxygen); phlogisticated air (presumably nitrogen); inflammable air (presumably hydrogen). No details are given concerning the preparation of these "airs," nor how they were introduced into the apparatus; presumably they were collected as prepared over water in the customary manner and were therefore saturated with moisture. Nothing is stated of the way in which the "airs" were heated nor are any details given of the mode in which the observations were made. Presumably the heating was effected by pouring hot water into the jar in which the thermometer was suspended and adjusting the constant level of 8 ins. from the level of the reservoir of mercury by the syphon. The experiments seem to have begun in the early part of December (year not stated) and carried on, with interruptions, into the following January. The following tables are transcribed exactly as they appear in the notes.

The results are uncorrected for the expansion of the glass "ball" (bulb), and of the mercury at the different temperatures. It is unlikely that these corrections were overlooked by Cavendish but he probably considered their influence was negligible in view of the much larger errors of observations.]

Exp. with common air.

	Th.	Div.	Bar.	Capac.	Press.	Log. capac.	Log. press.	Sum. log.	Log. expans.	Expans.	Diff.	Do. for ·1°o	Exp. for 1°
Dec. 5	80[1]	40	28·85	7076	28·41	8498	4535	3033	·0324	1·078			
	66	50	—	7002	27·74	8452	4431	2883	·0174	1·041	37	26	$\frac{1}{390}$
	52	60	—	6928	27·07	8406	4325	2731	·0022	1·005	36	26	
	41	68	28·87	6869	26·57	8369	4244	2613	·9904	·978	27	25	

[1] The number is set down 85 in the minutes but must certainly be a mistake for 80.

Nitrous air.

										Expans.	Do. if unif.	Expan. for 1°
Dec. 7	83·5	44·5	29·37	7043	28·63	8470	4568	3046	·0059	1·014	1·017	
3.30 P.	60·7	62·5	—	6910	27·43	8395	4382	2777	·9790	·953	·959	
Dec. 8	54·2	67·6	29·52	6872	27·25	8371	4354	2725	·9738	·942	·942	
9.50 A.	43	76·5	—	6806	26·67	8329	4260	2589	·9602	·913	·913	$\frac{1}{388}$
	60·7	64·3	—	6896	27·46	8386	4387	2773	·9786	·952	·950	
	68·3	58·5	—	6939	27·84	8413	4447	2860	·9873	·971	·977	
	89·5	40·5	—	7072	29·05	8495	4631	3126	·0139	1·033	1·033	
	68·3	58·5										

With fixed air.

	86·5	40·2	29·54	7075	29·09	8497	4637	3134	·0387	1·093	1·094	
Dec. 8	66·4	57	—	6950	27·97	8420	4467	2887	·0140	1·033	1·038	
0.30 P.	46·3	71·8	29·56	6841	27·01	8351	4315	2666	·9919	·982	·982	$\frac{1}{358}$
2.0 P.	68	56	—	6958	28·05	8425	4479	2904	·0157	1·037	1·042	
	86·8	40	—	7076	29·12	8498	4642	3140	·0393	1·095	1·095	

Heavy inflammable air.

Dec. 8	44	70	—	6854	27·14	8359	4336	2695	·9886	·974	·974	
6.0 P.	65·8	54·5	—	6969	28·16	8432	4496	2928	·0119	1·028	1·032	$\frac{1}{375}$
	84	40	29·57	7076	29·13	8498	4643	3141	·0332	1·080	1·080	

Dephlogisticated air.

	Th.	Div.	Bar.	Capac.	Press.	Log. capac.	Log. press.	Sum. log.	Log. expan.	Expan.	Do. if unif.	Expan. for 1°
Dec. 31	94	40	29·9	7076	29·46	8498	4692	3190	·0526	1·129	1·129	
2.0 P.	93	40·8	29·9	7070	29·41	8494	4685	3179	·0515	1·126	1·126	
	68	63·5	29·93	6902	27·92	8390	4459	2849	·0185	1·044	1·054	
5.0 P.	45	80	30·00	6780	26·91	8312	4299	2611	·9947	·988	·988	$\frac{1}{347}$
	68·8	63·3	30·00	6904	28·01	8391	4473	2864	·0200	1·047	1·056	
	70·2	62·4	30·00	6910	28·06	8395	4481	2876	·0212	1·050	1·060	
	94·5	40·6	30·01	7072	29·53	8495	4702	3197	·0533	1·131	1·130	

Nitrous air.

	Th.	Div.	Bar.	Capac.	Press.	Log. capac.	Log. press.	Sum. log.	Log. expan.	Expan.	Do. if unif.	Expan. for 1°
7.40 P.	91·3	40·5	—	7042	29·57	8477	4709	3186	·0485	1·118	1·121	
	69·5	59·5	30·04	6932	28·29	8409	4516	2925	·0224	1·053	1·059	
	44	78·3	—	6793	27·07	8321	4325	2646	·9945	·987	·987	
	44·5	78	—	6795	27·09	8322	4328	2650	·9949	·988	·988	
	67·5	61·5	—	6914	28·16	8397	4496	2893	·0192	1·045	1·054	$\frac{1}{350}$
	92·5	40	—	7076	29·60	8498	4713	3211	·0510	1·125	1·125	
	92	40·3	30·04	7074	29·58	8497	4710	3207	·0506	1·124	1·123	
Jan. 1	67·5	58·7	29·79	6936	28·11	8411	4489	2900	·0197	1·047	1·054	
10 A.	67	59	—	6935	28·10	8410	4487	2897	·0196	1·046	1·052	

Phlogist. air.

	Th.	Div.	Bar.	Capac.	Press.	Log. capac.	Log. press.	Sum. log.	Log. expan.	Expan.	Do. if unif.	Expan. for 1°
Jan. 7	86	40	29·76	7076	29·32	8498	4672	3170	·0413	1·099	1·099	
0.10	65	58·2	29·72	6941	28·08	8414	4484	2898	·0141	1·033	1·042	
1.20 P.	46·5	69·5	29·69	6858	27·29	8362	4360	2722	·9965	·992	·992	$\frac{1}{369}$
	65	56·5	29·66	6954	28·13	8422	4492	2914	·0157	1·037	1·042	
	85·3	40	29·66	7076	29·22	8498	4657	3155	·0398	1·096	1·097	

Inflammable air.

	Th.	Div.	Bar.	Capac.	Press.	Log. capac.	Log. press.	Sum. log.	Log. expan.	Expan.	Do. if unif.	Expan. for 1°
Jan. 1	67·5	40·3	—	7074	29·39	8497	4682	3179	·0164	1·039	—	$\frac{1}{385}$
9.30 P.	52·5	51·5	29·85	6991	28·64	8445	4570	3015				

More air taken out.

	Th.	Div.	Bar.	Capac.	Press.	Log. capac.	Log. press.	Sum. log.	Log. expan.	Expan.	Do. if unif.	Expan. for 1°
Jan. 2	59·8	61·5	30·01	6917	28·13	8399	4492	2891	·0040	1·009	1·014	
9.30 A.	42·6	73·3	30·01	6830	27·37	8344	4373	2717	·9866	·970	·969	
	43	73·2	30·01	6830	27·37	8344	4373	2717	·9866	·970	·970	$\frac{1}{377}$
	60	61·5	30·02	6917	28·14	8399	4493	2892	·0041	1·040	1·015	
	86	40·5	30·03	7072	29·56	8495	4706	3201	·0350	1·084	1·084	

[All the coefficients of expansion are much too large, but the results must have at least served to indicate, as indeed Cavendish would appear to have recognised, that the rate between the limits he observed is practically uniform. Moreover, he was justified in inferring that it is sensibly the same for all gases, the difference between the several gases not being greater than that found on a repetition of the experiments on the same gas. With that meticulous sense of accuracy which was characteristic of him, he refrains, however, from explicitly stating this conclusion, but his claim to have demonstrated it is at least as valid as that of "Citizen" Charles, who also never published the results of his experiments, which seemed, if we may judge from Guy Lussac's statement, to have been made subsequently to those of Cavendish.]

EXPERIMENT PROPOSED FOR DETERMINING THE DEGREE OF COLD AT WHICH ☿ BEGINS TO FREEZE[1]

(Shewn to Sir J. BANKS in 1781.)

The way I would propose, is to fill the cylindrical glass of one of [the] thermometers with the ivory scales, to the top of the swelled part, so as to cover the ball of the inclosed thermometer and keep it in a freezing mixture till almost all the ☿ in the cylinder is frozen; and observe what heat is shewn by the inclosed thermometer. This must evidently be the cold with which ☿ begins to freeze; for as in this case, the ball of the thermometer will be surrounded for some time with ☿, part of which is actually frozen, it seems impossible that the thermometer should be sensibly above the point of freezing ☿; and while any of the ☿ in the cylinder is unfrozen, it is impossible that it should sink sensibly below that point.

From the Petersburg experiments of freezing ☿ I think it appears clearly, that ☿ contracts in the act of freezing, or that ☿ takes up less room in a solid than in a fluid state, and that the very low degrees to which the thermometers sunk in that experiment, was owing to this contraction of the ☿ during freezing and not that they produced a degree of cold anything near equal to that shewn by the thermometer; and from some circumstances of the experiment, I have little doubt but that the degree at [which] ☿ freezes is less than 200° below nothing; for which reason I thought it unnecessary making the ivory thermometers reach more than 2 or 300 degrees below nothing.

If a thermometer is put into a glass of water, and exposed to a cold sufficient to freeze it, it will remain perfectly stationary from the time that the water begins to freeze, till it is intirely frozen; and will then begin to sink again. In like manner if a thermometer is dipt into melted tin or lead, it will remain perfectly stationary (as I know by experience) from the time that the metal begins to harden round the edges of the pot, till it is all hardend, when it will begin to descend again; and there seems no reason to doubt but what the same thing will obtain with regard to ☿.

The method which I would use in trying the experiment is as follows. Put the ivory thermometer prepared as abovementiond, into the freezing mixture, along with one of the thermometers with wooden scales; taking care that none of the mixture gets into the cylinder. Then as soon as the ☿ in the cylinder begins to freeze, the ivory thermometer will become

[1] [Cf. p. 57 and p. 145.]

stationary, but the wooden one will still continue to descend, on account of the contraction of the ☿ in it by freezing. Write down this stationary degree of the ivory thermometer, which as was before said, is the heat at which the ☿ begins to freeze; and keep the thermometers in the mixture till either the ivory thermometer again begins to descend, or till the wooden one becomes stationary. If the ivory thermometer begins to descend before the wooden one becomes stationary, take it out and see whether the ☿ in the cylinder is frozen. But if the wooden thermometer should become stationary before the ivory thermometer ceases to be so, as will most likely be the case, it will be necessary to refresh the freezing mixture or put the thermometers into a fresh mixture. For the wooden thermometer becoming stationary shows that the mixture is no longer cold enough to freeze ☿. In this manner you must proceed till either the ivory thermometer again begins to descend, or till you have reason to think from the time which it has continued stationary, and the very low degree to which the wooden one has sunk, that a great part of the ☿ in the cylinder is frozen.

It is not impossible but what the ivory thermometer may rise suddenly some degrees on the ☿ in the cylinder beginning to freeze, and then remain stationary. At least this is what happens on exposing a thermometer in a glass of water to the cold. But this would cause no inconvenience in the experiment as the point at which it remaind stationary would still be the freezing point.

The only circumstance I can think of which can prevent the success of the experiment (except that of not being able to produce a sufficient cold) is that possibly the ☿ in the ivory thermometer (as being in vacuo) may freeze with a less degree of cold than that in the cylinder, which is compressed with the whole weight of the atmosphere. I have no reason to think that this will be the case; but if it was it would intirely destroy the success of the experiment; but it would not lead to error; as the ☿ in the ivory thermometer would never become stationary. If this was found to be the case, it would be proper to break off the top of the stem of the ivory thermometer, so as to let the air into it; and try it over again.

Experiment 2. The above experiment gives the degree of cold, as shewn by a ☿ thermometer, at which ☿ begins to freeze. But it is not unlikely that it may require a much less alteration of cold to make a ☿ thermometer sink a given number of degrees, when coold almost to freezing, than when it is of a less degree of cold; and consequently that the real degree of cold at which ☿ freezes may be much less than that shewn by the thermometer. The best way I know of finding whether this is the case or not, will be as follows:

Take a thermometer with a ball of a very large diameter; and keep it in a freezing mixture till it is coold very near to that degree at which it is found by the former experiment that ☿ begins to freeze; then take it out, wipe off slightly, and as quick as you can, so much of the mixture as adheres to it, read off the division; and plunge it into a glass of ☿ of about the temper of the air in the room, and whose heat must be previously found; and keep it stirring about till you have reason to think that the thermometer and glass of ☿ are both of the same heat; and write down this heat, or the heat of the mixture as I shall call it. Next repeat the experiment in the same manner; only making the thermometer about as much warmer than the glass of ☿, as it before was colder; and find the heat of the mixture as before. Then if by this means you find that the difference of heat of the mixture and glass of ☿, bears a less proportion to the difference of heat of the thermometer and glass of ☿ in the 1st experiment, than in the 2nd, it will shew that ☿ expands more by a given alteration of heat, in a very great degree of cold than in a more moderate one. Suppose, for example, that in the $\left\{\begin{matrix}\text{1st}\\ \text{2nd}\end{matrix}\right.$ trial, the heat of the ☿ in the glass was $\left\{\begin{matrix}-50^\circ\\ -52^\circ\end{matrix}\right.$; the heat of the thermometer before plunging in $\left\{\begin{matrix}-153^\circ\\ +31^\circ\end{matrix}\right.$; and the heat of the mixture $\left\{\begin{matrix}-70^\circ\\ -30^\circ\end{matrix}\right.$; consequently the difference of the heat of the thermometer and glass of ☿ is $\left\{\begin{matrix}103^\circ\\ 83^\circ\end{matrix}\right.$; and the difference of heat of the mixture and glass of ☿ $\left\{\begin{matrix}20^\circ\\ 22^\circ\end{matrix}\right.$. Then the real difference of heat of the thermometer and glass of ☿, in the 1st experiment is to that in the 2nd as 20 : 22; that is as its effect in altering the heat of the mixture. Therefore if we suppose that the differences of heat shewn by the thermometer in heats greater than -53° are proportional to the real differences of heat, the real difference of heat of the thermometer and glass of ☿ in the 1st experiment is $83 \times \frac{20}{22} = 75^\circ\frac{1}{2}$; and therefore the true heat of the thermometer in the 1st experiment is $-50 - 75\frac{1}{2} = -125^\circ\frac{1}{2}$, instead of 153° as shewn by the thermometer.

N.B. It will be proper to repeat this experiment with very different heats, both of the thermometer and glass of ☿, in order to judge by the agreement or disagreement of the experiments the degree of accuracy which can be expected from them.

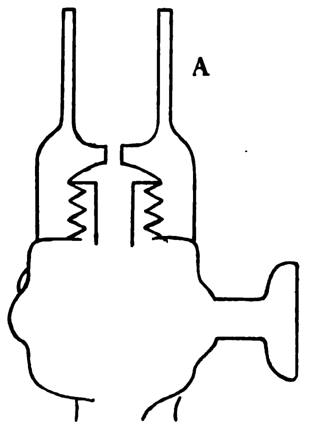
In degrees of heat between freezing and boiling water, the differences of heat shewn by a ☿ thermometer seem to be actually in proportion to the real differences. For if equal quantities of hot and cold water are mixt together, I am convinced that the heat of the mixture is very exactly the mean between the heats of the hot and cold water. Mr De Luc, indeed, thinks otherwise; but in several experiments which I made with as much accuracy as I could, and in different manners of trying the experiment, the event was always as I mention; and Mr Smeaton has also tried the experiment in a different manner, and with the same event.

[This statement appears to have been drawn up by Cavendish for the information of the President of the Royal Society in connection with a request from the Society that the Hudson's Bay Company might permit one of their servants to make the suggested observations.]

COLD PRODUCED BY RAREFACTION OF AIR

Feb. 25, 1783.

Over the cork of condensing glass of air pump was screwed a brass cap *A* as in fig., the bore of the hollow cylinder being not much more big enough to receive the ball of the thermometer *G* having a small hole at bottom by which the condensed air escaped on opening the cock and blew on the ball of the thermometer.



Having then forced one additional atmosphere into the receiver, the thermometer sunk 7 or 8° on letting out but began to rise again before the air was all run out.

A copper globe was prepared 12 inches in diameter to be used instead of condensing glass, and also a wooden cap like *A*.

Apr. 14. With small thermometer and brass cap *A* globe condensed to 30 inches, thermometer sunk 14°½, and a 2nd time 15°½. It was found that the air ran ½ out in 33″.

Apr. 15. Thermometer *G* sunk 14°, air being let out immediately after it was condensed, but being tried again after waiting 10′ before it was let out it sunk 15°½. Air run ½ out in 25″.

With small thermometer, sunk 15°½, waiting 10′: air run ½ out in 24″.

In order to see whether thermometer *G* or small thermometer coold fastest, they were both heated in hand to 88° and exposed to air [of] room which was about [1]. *G* cooled to 70° in 70″ and did the same in a 2nd trial: small thermometer coold as much in 50″ in 1 trial and in 60″ in another.

[1] [Not stated.]

HEAT AND COLD BY EXHAUSTING AND CONDENSING OF AIR

Tried with Nairne's thermometer: ball = ·28 inch in diameter: inclosed in condensing glass.

Time, March 10	Heat air	Heat therm. before	Do. after
8.40 P.		55°	49 on exhausting
9.30	55½	55	59 letting in air
9.40	56	55½	49 exhausting
11th 9.30 A.	48	48	52 letting in air

N.B. It rose about ½° out of this 4° after air was let in.

2.40 P.	55¼	56	62½ condensing 1 atm.
3.10 P.	55½	57	51 letting out air

Tried with same thermometer in a thinner glass intended only for exhausting.

8.30 P.	55	53½	58 letting in air after having been long exhausted
9.35 P.	53	53	49½ on exhausting

N.B. It sunk near ½° after the exhaustion was finished.

11.15 P.	54½	53	57½ letting in air
12th 8.30 A.	47½	47½	44½ on exhausting
9.30 A.	50	49½	51½ gage sunk to 16½
10.30 A.	51	50½	53 letting in remainder

Tried in same glass with Thermometer *G* ·25 in. diameter.

11.40	52	52	47 on exhausting

Being exhausted to within ·53.

0.0	52	52½	not altered on depressing gage by 1·4
0.6 P.	52	52½	54½ gage sunk ½ way
0.15 P.	52½	52½	55 letting in remainder of air

The same glass with same thermometer in it was exhausted and thermometer in it stood long stationary at 52°½. The large copper globe heated before fire was then screwed on, and a small thermometer placed in the side hole till it became stationary which it did at 95°. This thermom. was then taken out, the hole screwed up, and the air let out of the globe into the exhausted glass so that it was filled with air of 95° hot. The included thermometer rose to 57°½.

March 12. The same receiver being exhausted, with same thermom. in it, and kept so till it remained stationary at 49° and then air let in in common manner, thermom. rose 5°.

The mouth of one of the large globular glasses was stopt with a cork through which a piece of barometer tube passed reaching to near bottom of glass; a thermometer was also fastend to the cork so that the bulb was near the center of the glass. This glass was heated before the fire and then suspended over the receiver of the air pump in such manner that the tube enterd into a hole in a piece of wood screwed to the cock of the receiver and fitted tight into it. The receiver had been previously exhausted, and the inclosed thermometer stood stationary at $49^\circ\frac{1}{2}$; the warm air from the glass was then let into it, the thermometer in which was at 110°, on which the thermometer in the receiver rose to $54^\circ\frac{1}{2}$.

2 glass tubes were prepared with brass caps with a kind of gimbals fastend to them by which they could be stretchd. Both tubes were about ·22 in. bore, the glass of one being about ·11 and the other about ·03 thick. A thermometer was also prepared with a ball ·21 in. diam. and a scale like Ramsden so that it would go within these tubes. The thick tube with this thermom. inclosed in it, and each end stopt loosely by a cork, was then stretchd with a weight of $\frac{50{,}000 \times 31}{4{\cdot}18} = 375{,}000$ grains which is about as much as a globe of glass of that thickness 6 inches in diam. would be stretchd by 3 atmospheres. I could not perceive the thermometer to be at all affected by that stretching or taking away the stretch. No alteration neither could be perceivd by stretching the thin tube with the same weight.

The same thermometer was inclosed in a tube of the same size and thickness made so as to fit on to the air pump so that the air in it could be condensed. It seemed to rise about $\frac{1}{4}^\circ$ on forcing in 1 atmos. and to sink as much on letting it out. If this thermom. was placed in a cup of oil it alterd not more than $\frac{1}{8}^\circ$ by the same operation, so that it seemd as if it alterd rather more than it would have done by the simple pressure on its ball, but is very doubtful.

A cylind. glass bottle about 5 inches in diam. and $7\frac{1}{2}$ high in the cylind. part with a brass cap to fasten it to the air pump was prepared, the glass being much thicker than usual. A receiver also was prepared. A receiver also about $6\frac{1}{2}$ in. diam. and $10\frac{1}{2}$ high was prepared.

June, 1783. This bottle with the small thermometer used in the last experiment in it was screwed on to air pump and the air condensed 1 atmos. The thermometer rose 5° on condensing and sunk $2^\circ\frac{1}{2}$ on letting out the air: the event being the same in 2 trials.

It was then exhausted 2^{ce} running, to within 3 inch. when it sunk $3^\circ\frac{1}{8}$ in 1st trial and 4° in 2nd and rose $5^\circ\frac{1}{4}$ on letting in in both trials.

The receiver was then placed over the bottle, the bottle with the thermometer in it remaining as before, so that the air in the receiver was not altered. The thermometer then rose $5^\circ\frac{1}{4}$ on condensing and sunk $2^\circ\frac{1}{4}$ on letting out the air. It also sunk $3^\circ\frac{1}{2}$ on exhausting, and rose 5° on letting in.

The thermometer was then placed between inner and outer glass, the bottle as before so that the air between the glasses still remaind unaffected. It was not at all alterd by exhausting or condensing the air in the bottle or letting the air in or out. The thermom. was then placed in bottle left open and inclosed within receiver, the air in both being condensed and exhausted. It then sunk 6° on exhausting and rose 5°¾ on letting in. It also rose 5° on condensing and rose [sunk] 9°¾ on letting out.

The bottle with the thermometer in it was then shut, so that air in it was unalterd; the air in outer receiver being exhausted or condensed the thermom. was not at all alterd thereby.

The thermom. was then placed between the two glasses where the air was condensed and exhausted, the bottle being closed. It then sunk 1°¼ on exhausting and rose 2° on letting in air. It also rose 2°½ on condensing and sunk 1°½ on letting out the air.

The tube of the thermometer being broke in this experiment and consequently the tube remaining open, it was inclosed in bottle and that screwed on on plate. It then rose 5°·6 on condensing and sunk 2° on letting out. It also sunk 2°·1 on exhausting and rose 5°·6 on letting in, which is pretty nearly the same as it did before it was broke.

TABLE SHOWING DETAILS OF FOREGOING OBSERVATIONS

Bottle screwed on with thermometer in it.

Time		Heat
9.51		65°
52	condensed 30 inches	70
56		66
57	let out	63½
10. 1		64¾
2	condensed 30 inches	69¾
6		66
7	let out	63½
14		64½
16	exhausted to 27	61
23		63½
24	let in	68¾
28		65
30	exhausted to 27	61
35		63½
36	let in	68¾

The receiver was then placed over it.

45		65¼
46	condensed	70½

Time		Heat
10.50		67°
	let out	64¾
55		65¼
	exhausted	62¼
11.3		65½
	let in	70½

Thermometer placed between inner and outer glass.

4.31		66
33	inner exhausted	do.
33		do.
	air let in	do.
	condensed	do.
	let out	do.

Thermometer placed in inner bottle included in receiver in which last the air is condensed or exhausted.

5.19	bottle open	66
22	exhausted	60
27		65
	let out	70¾
35		66¾
37	condensed	71¾
	let out	62
	bottle shut	
8.35		63¾
45		64
47	condensed	do.
50		64
	let out	do.
9.12		64⅛
16	exhausted	64¼
	let in	64

Thermometer placed between inner and outer glass, air between them condensed and exhausted: inner glass closed.

10.18		67⅓
29		67½
31	exhausted	66¼
43		67¾
	air let in	69¾
45		69
52		68⅓
	condensed	70¾
57		69
	air let out	67½

The tube of thermometer was then broke and consequently remained open: it was then enclosed in bottle and screwed on on plate.

Time		Heat
4.35		11·7°
	condensed	17·3
4.43		12
	let out	10
4.58		10·4
	exhausted	8·3
5.2		11
	let in	16·6

TO TRY WHETHER DAMP AIR IS OF SAME SPEC. GRAV. AS DRY

[Suggested experiment.]

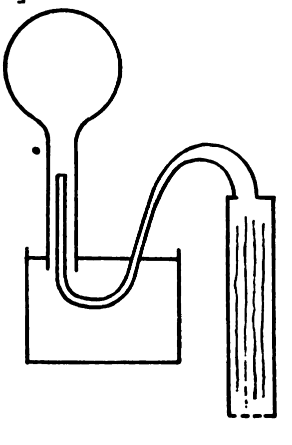
In room in which air is very dry fill a flask with the air of a room by blowing air through it with bellows and weigh it carefully. Then fill it with damp air by blowing air into it in same room through a cylinder filled either with fresh water or water more or less impregnated with salt, or filled with wet paper and weigh it again.

N.B. For fear the water, etc. in the cylinder should be cooled by the evap. it will be more accurate if the cyl. is bent so as to be immersed in water.

TO SEE WHETHER BULK OF PERFECTLY DRY AIR IS INCREASED BY SATURATING WITH MOISTURE IN RATIO OF DEPRESSION AS THAT HEAT BY WATER TO PRESSURE OF ATMOSPHERE ON IT [*sic*]

[Suggested experiment.]

Fill barometer carefully and in cold weather. Let in air through cyl. of wet paper by app. in figure. Try it carefully in different heats. Then let in a little water and repeat the experiments. It may be calculated how much the depression on the addition of water should be according to this hypothesis and consequently whether it agrees with hypoth.



INSTRUCTIONS TO THE CLERK OF THE ROYAL SOCIETY CONCERNING THE METEOROLOGICAL OBSERVATIONS TO BE MADE ON BEHALF OF THE SOCIETY AT THE SOCIETY'S HOUSE IN CRANE COURT[1]

That he [Mr Robertson] observe the heat of the thermometer and barometer as early as he conveniently can in the morning not later than 7 o'clock in summer and 8 in the winter, and also about 2 in the afternoon which is supposed to be about the hottest part of the day.

That he measure the rain every morning about the same time as the thermometer except when the quantity is trifling, and in particular on the first morning of each month, and that he set down the sum of all the quantities measured in each month except on the 1st day together with that measured the 1st day of the succeeding month as the whole quantity fallen that month, and that he also set down the quantity fallen each year.

That he set down each day the direction of the wind and its strength together with any other observations relating to the weather that he may think fit. It will be sufficient to set down the wind once each day, namely at the time of the afternoon observation of the thermometer, and to distinguish its strength into 3 degrees, namely, calm, brisk and violent.

That during one fortnight of the year he observe the horizontal and dipping needle 5 times a day, namely, as early in the morning as convenient, about 2 in the afternoon, a little before he goes to bed, and about ½ way between each of those times and also to set down the mean variation and dip during that time. As to the dipping needle it must be observed that it will be necessary always to observe whether the needle is truly ballanced before he begins his course of observations, but if it is then found to be truly adjusted, I imagine there can be no danger of its requiring to be readjusted during that fortnight in which it is proposed to observe it, provided care is taken not to handle it, or take it out of the case compass box or to move it about during that time. But as to this, I shall be able to speak more positively in a month or 2. If this is found to be the case I could wish the Council would always before the fortnight of observation desire some one of [the] Society to examine whether the needle was adjusted, and if not to make it so, and also to examine at the end of the fortnight whether it continued so. At the same time he might examine the position of the horizontal needle. I would propose also that the Clerk be required to remind the Council of it every year in proper time.

It is proposed that the greatest and least heat of the thermometer in each month, together with the mean morning and midday heat of each

[1] [Cf. p. 53 and p. 112.]

month and each year; the greatest and least height of the barometer in each month; the quantity of rain in each month and each year; and the mean variation and dip of the needle in each year be printed at the end of the last part of the Transactions for each year.

The thermometer is proposed to be placed out of one of the windows of the Norfolk Library[1] and as in this situation the sun will shine upon it in the morning in summer it is proposed to fitt a piece of board as a screen. It could not be placed so as to be defended from the sun in summer unless it was placed out of one of the windows of the lower story where it is imagined the situation would be too confined to show the heat truly. It is supposed to be unnecessary observing the thermometer indoors.

The rain gage is proposed to be placed on the leads of the house not very far from the middle and raised $3\frac{1}{2}$ feet above them, that is on a level with the wooden rails. There is no chimney which will be elevated above it in an angle of more than   degrees.

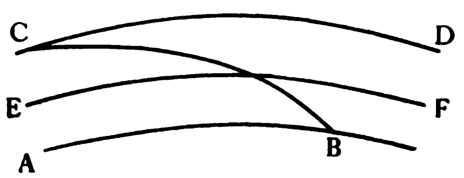
A weathercock is proposed to be placed either on one of the chimneys of the house, or on the wooden rails which surround the leads, which may conveniently be seen from the garden.

The barometer, variation compass and dipping needle, are proposed to be placed in the Norfolk Library and a mark is proposed to be placed on the South Wall of the Museum which by means of a telescope fixed to the compass will serve to place it in the proper position.

ATMOSPHERIC REFRACTION

Some time ago, in discoursing with you about the extraordinary refractions which Wales observed in Hudson's Bay, I said that if the radius of curvature of a ray by refraction was less than that of the earth, I did not see how it was possible for there to be any visible horizon; but on considering the matter further I find that it is possible.

To explain this, let AB represent the surface of the sea, and CD an arch of a concentric circle. Suppose that the radius of curvature of a ray of light passing nearly horizontally through the air, at a small height above the sea, is less than the radius of curvature of the earth; and suppose that



the greater the height of the ray of light above the sea, the greater

[1] [The Arundel or Norfolk Library, *Ex dono Henrici Howard Norfolciencis*, was presented to the Royal Society in 1666–7, as a New Year's gift, at the instigation of John Evelyn. It was collected by Thomas, Earl of Arundel, during his embassy at Vienna, and consists chiefly of first editions of books, published soon after the invention of printing (Maitland). It may be regarded as the nucleus of the Society's library. See Weld's *History of the Royal Society*, vol. I. 196.]

its radius of curvature, as it is natural to suppose; so that at the height of the line *CD* above the sea, its radius of curvature shall be exactly equal to that of the arch *CD*. It is plain that no ray of light passing horizontally at a greater height than that of *CD*, can ever fall on the line *AB*; but a ray passing horizontally ever so little below that height, will fall on it. Therefore let *CB* represent the path of a ray of light passing horizontally at an infinitely small distance below *CD*: it is plain that no object on the earth can appear elevated above the horizon in a greater angle than that of *CBA*; and all objects beyond a certain distance will appear elevated in very near that angle; and consequently the visible horizon will appear elevated by the angle *CBA*. Moreover if the height of the line *CD* (and the refracting power of the air below it) is such, that a ray of light coming (to the observer's eye) from an object at no very great distance (from him) may pass near *CD*, the horizon may appear distinct; but if it is such that no ray of light can pass near *CD* but what comes from a very distant object then the horizon must appear very indistinct; just as it does to an observer situated on a very high mountain. 2ndly, the higher the observer is above the sea, the less will the visible horizon appear elevated; for drawing the concentric circle *EF* between *AB* and *CD*, the line *CB* cuts *EF* in a less angle than it does *AB*. 3rdly, though the observer is near land the visible horizon will appear uniform, as in the open sea; for as objects of very different distances and colours will all be crowded into the same point, they will exhibit an uniform appearance; but objects at such a distance as to appear much below the visible horizon may appear distinct; therefore an observer at sea near the shore should see the objects on the shore distinct, and above them he should see the visible horizon appearing like that of a sea placed beyond the shore.

N.B. What is said in this article goes on a supposition that the height of the line *CD* above the level of the sea, is the same over the land as over the sea, which is not very likely to be the case. 4thly, if a distant object appears at the same height as a nearer object to an observer near the surface of the sea, it must appear above it if the observer is placed at a greater height, as would be the case if there was no extraordinary refraction; so that this will not account for Wales's seeing the fort on the deck, but not at the mast-head. As for what Wales says about the islands of ice I do not thoroughly understand him.

THE REFRACTION ON A MOUNTAIN SLOPE

The following paragraph is extracted from a paper by Sir Joseph Larmor, on "The Influence of Local Atmospheric Cooling on Astronomical Refraction," *Monthly Notices, R. Astronomical Soc.* vol. LXXV, 1915:

After the above was written, it was found in looking through Henry Cavendish's extensive MS. calculations relating to the planning by the

Royal Society, between 1772 and 1774, of the Schehallien experiment for the determination of the Earth's mean density, in which Cavendish took a very prominent part, that he had then considered the problem of accidental refraction-error due to the mountain, substantially as here. If the summit is colder than it is at the same level directly above the foot of the mountain, the strata of equal atmospheric density tilt upward towards the summit; and the apparent position of a star, chosen near the zenith to avoid ordinary refraction, will be deflected away from the mountain, thus making the observed effect of attraction of the vertical towards the mountain too small. Cavendish calculated roughly that for a defect of temperature of $12\frac{1}{2}°$ F. at the summit, which he took as an extreme estimate, and for a hill sloping at 21° to the horizon, the error would be about $0''\cdot 6$ on each side of the hill, depending, as we have recognised, only on the temperature difference and not on the height. The formula (A) gives for a local temperature change of this total amount, from a mountain of that slope to the air above it, a value of about $0''\cdot 5$ for z small, if the strata are taken parallel to the slope, whereas Cavendish made them inclined to the slope so that the temperature fell along it. In the Schehallien observations the effect of the attraction of the mountain came out as $11''\cdot 6$, which Cavendish's extreme estimate for refraction would increase by 10 per cent., thus, as it happens, leading to consistency with modern determinations. There does not appear to have been any attempt to apply such a correction, either to Maskelyne's Schehallien observations, or to the later series by Sir Henry James at Arthur's Seat, where the result was 3 or 4 per cent. too small, while owing to flatter conditions the refraction error would be much less: see the account in Poynting's Adams Prize Essay of 1894 on *The Mean Density of the Earth*, pp. 15–22.

Note added by Sir Joseph Larmor. The Schehallien determination of the Earth's mean density was published in *Phil. Trans.* 1798, and the result of the Michell-Cavendish vibration experiments in *Phil. Trans.* 1798. They are fully described by Airy in his treatise on the "Figure of the Earth" in *Encyclopaedia Metropolitana*, 1830. He remarks on the "various contrivances of practical calculation" in Hutton's determination of the attraction of Schehallien, and he adds in a footnote, "Most of these, it appears, were suggested by Mr Cavendish. And it appears that nearly all the preliminary calculations of the attraction of Skiddaw, etc. [which had been suggested for the determination] were made by Mr Cavendish," as he had learned from inspection of the Cavendish Papers.

METEOROLOGICAL OBSERVATIONS AT MADRAS

(Extract from a letter by Cavendish.)

In England the heat of the water in deep wells or quick springs is very nearly equal to the mean heat of the air, and it seems well deserving inquiry whether it is the same in other countries; for if it is so, it would afford the readiest way of comparing the mean heat of different climates. As your correspondent's observations, if continued, will tell us the mean heat of the air at Madrass I should be very glad if he would also try the heat of the water in wells of the place, the deeper and less exposed to the air the better. It will be sufficient to try it once or twice at opposite seasons of the air [*sic*], but if he would be so good as to try it on 2 or 3 different wells it would be the better. If it is a draw well, it will be sufficient to draw up a bucket of water and try the heat with his thermometer. If it is a pump I would recommend to him to pump a few minutes before he tries the heat, as the water which comes first is what is contained in the body of the pump, which perhaps may be of a different heat from that in the well.

I am informed that the usual way of cooling their water at Madrass is to expose it to the open air in porous earthen vessels. I should be very glad if he would now and then try the heat of the water in them at different times of the day, and different seasons of the year, and also set down the heat of the air at the same time, so as to shew how much the water is cooled by the evaporation.

If there should happen to be any Taffoon (typhoon) while he is there, I could wish that he would observe the alterations of the barometer and wind and weather as closely as he conveniently can from the time of the first presages of it to the end, and that if he has an opportunity he would endeavour to collect how far it extended and at what time it began and ended at different places; and that he will set down any other circumstances which may occur to him that he thinks will tend to make us better acquainted with the nature of those extraordinary phenomena.

The exposition of your correspondent's thermometer without doors, which was placed under a shady tree, seems not quite unexceptionable, as I am afraid that the air under the tree may be cooled by the evaporation from the leaves. What confirms me in this opinion is that the thermometer without doors was seldom hotter than that within doors in the middle of the day, and was commonly considerably cooller in the morning, whereas if it had not been for that cause I imagine the thermometer without doors would commonly have been considerably hotter than that within doors in the middle of the day.

All the portable barometers of Ramsden which I have seen have a Vernier division by which we may observe the height to 100ths and 200ths of an inch, and have a screw at the bottom by which the quicksilver in the cistern may be adjusted to the proper height. If it would not be too much trouble to your correspondent, it would be better if he would set down the height of the barometer to hundredths of an inch, and it would be more satisfactory if he would mention whether he frequently adjusts the quicksilver in the cistern or whether he trusts to its remaining always the same. I need not say that if a person would be accurate in his observations he should examine the height of the quicksilver in the cistern frequently.

CAVENDISH'S REGISTERING THERMOMETER

In Wilson's *Life of Cavendish*, p. 477, is a description, with illustrations, of a Registering Thermometer contrived by Cavendish. The account is as follows:

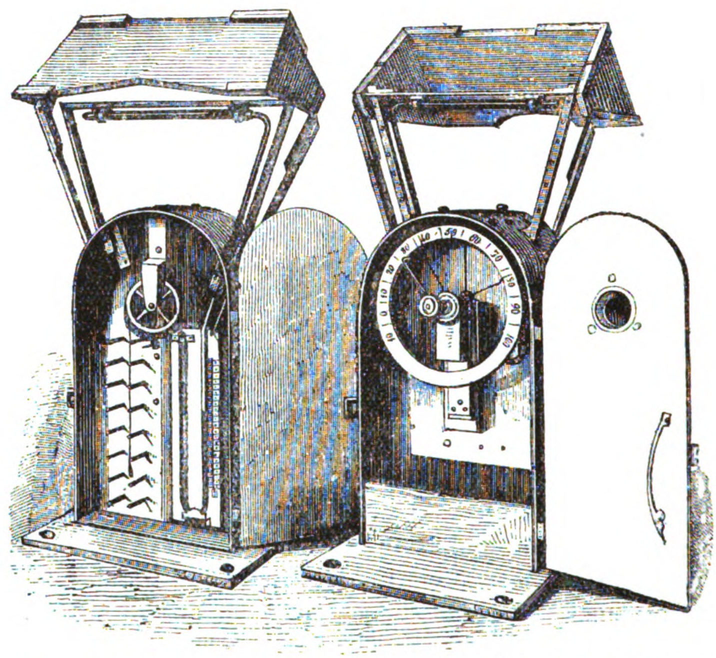
"The above figures [p. 396] represent a front and back view of Cavendish's Register Thermometer. This instrument was presented by Sir Humphry Davy to Professor Brande, and is included in the collection of old apparatus in the Royal Institution.

This instrument consists essentially of a large glass tube containing alcohol, the expansion and contraction of which acts upon mercury contained in the recurved or inverted syphon-termination of the tube. A considerable portion of the alcohol tube is exposed to the atmosphere, but it is sheltered from the rain by a roof-like cover. The only opening in this tube is at the top of the left-hand limb of the syphon. The surface of the mercury here carries an ivory float, from the top of which proceeds a silken line, and this, passing twice round the periphery of a wheel grooved for the purpose, falls down and hangs loosely, with a small balance weight at its extremity.

The register is performed in the following manner: The axis of the wheel which carries the cord carries also a light index hand: this hand moves in a vertical plane some distance behind the graduated circle; but near the top of this hand projects a short horizontal piece carrying a vertical needle which in the right hand figure is now pointing at 50°. On either side of this index is a friction needle, which accompanies the index to the extreme limit of its range, and stops there. In the figure we may suppose the index hand to have advanced to nearly 80° on one side, and, having pushed the friction hand thus far, the alcohol began to contract, the index to recede, thus leaving the friction hand to record the highest temperature that had been attained. The alcohol continuing to contract, the index would recede until it came in contact with the friction hand on the other side: here the extreme limit appears to have been something below 40°; the temperature then beginning to rise, the index hand had reached 50° at the time of observation.

In order to make a new observation, a bent lever (which in the figure appears to be pointing near 30°) is turned round by means of a central thumb screw, first on one side and then on the other, so as to place the two friction hands in contact with the index hand at the point indicated at the time of setting. The instrument is then left to itself, and after

REGISTER THERMOMETER.



some hours, or next day, the friction hands will be found separated as before.

The two faces are glazed with plate glass, and a hole is made in the glass in the right hand figure for allowing the thumb-screw to pass out. The lower part of this glass is covered with tin-foil to the height of about 2 inches. Both faces are moreover provided with doors, which close with a spring. These doors, and the outer case, are of sheet and bar iron, and the whole is very heavy. The whole height, from the base to the ridge of the cover, is about 18 inches; the height of the glazed part is $11\frac{1}{2}$ inches, and it is 6 inches across.

The brass back of the instrument (left-hand figure) is furnished with a number of projecting brass pegs: the top now consists of 4 pegs, the two outer ones of which have sharp points; all the others being blunt or rounded. I have not been able to discover the object of those pegs, but Professor Brande informs me, that when the instrument came first into his possession a number of pieces of bibulous were stuck upon them, the object of which was probably to keep the interior of the instrument dry.

The instrument is now quite out of order: the alcohol appears to have oozed out, probably by capillary action; the glass is also in some places corroded by the mercury, as we often see in old instruments in which impure mercury has been used."

Among the Cavendish MSS. preserved at Chatsworth is an account of the calibration of this instrument, which seems to have been devised in

Float Thermometer.

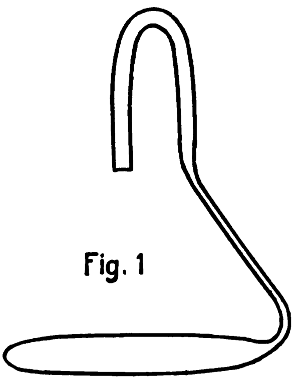


Fig. 1

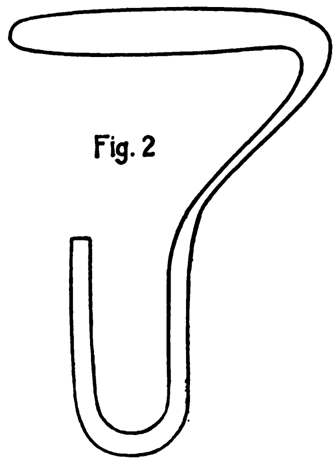


Fig. 2

1779, whilst he was still living with his father at Great Marlborough Street. The account is, of course, only of historical interest, but it is here reproduced, with the accompanying figures in facsimile, as another illustration of the love of accuracy and scrupulous attention to detail which characterised all Cavendish's experimental work.

"June 12, 1779. The proportion of the bore of the 2 legs to each other tried by filling the th. with spts. with some ☿ in the legs with the brass frame fastend on and heating the cylinder in sand in the position of fig. 2."

[The heights of the mercury in the two limbs of the U tube were then read to 500ths of an inch as the position of the mercury was caused to fluctuate by the expansion and contraction of the spirit, whereby the alteration in the bore of the tube in which the float was to be placed could be ascertained. The observations extended over several days. On June 17, we read, "The ☿ was got out and a proper quantity of spt. put in and compaird in freezing mixt. [ice and salt?] with the standard [thermometer] *S* in position of fig. 1."

A large number of comparisons were made at different temperatures by the same thermometer and after certain corrections for expansion of the scale, etc. had been made a table was constructed showing the height of mercury in the leg corresponding to the true temperature.]

It was found by putting on the float and observing what division on the scale the bottom of the float answered to when the hand stood at a certain mark on the dial plate and when it was turned round one revolution that 1 revol. answered to 5·258 on scale or 5·516 inches. A piece of tin with two holes in it at a proper distance was fastend to the scale to enable the eye to look perpend. and avoid parallax; therefore 1 divis. on scale answers to 7·832 mins. Therefore the angles answering to different degrees on divided circle are as follows:

Deg.	Angle	Diff.
−10	0° 0′	
		19° 11′
0	19 11	
		19 43
10	38 54	
		20 14
20	59 8	
		21 16
30	80 24	
		21 41
40	102 5	

Deg.	Angle	Diff.
		21° 40′
50	123° 45′	
		22 42
60	146 27	
		22 51
70	169 18	
		23 14
80	192 32	
		24 9
90	216 41	
		24 40
100	241 21	

MATHEMATICAL AND DYNAMICAL MANUSCRIPTS

Arranged and Described by SIR JOSEPH LARMOR, F.R.S.

It is difficult to advance any definite dates for this part of Cavendish's activity. One thing that emerges clear is that the elucidation of physical and astronomical phenomena by mathematical thought was one of his main permanent interests: and if he had no other claim to renown he would be entitled to rank high among the theoretical physicists of his period, though more deeply versed (as was then usual in England) in the physical significance than in the formal perfection of the analysis.

His more special occupation with Physical Astronomy and with the Dynamics and Figure of the Earth may fairly be held to centre around the year 1773; for at that time he was very actively engaged in forwarding the enterprise of the Royal Society for the determination of the mean density of the earth, and was indeed the guiding spirit in that project. This period is usually considered to have been barren as regards physical astronomy in Britain: but though the improvement of analysis and the conduct of surveys fell mainly to the share of France, these papers incidentally afford evidence that the Schehallien determination of the Earth's mean density was not an isolated episode. The dynamical principles guiding the rising science of Geodesy, and their physical outcome, as distinct from mathematical method—the practical discussion of the relation of the geometrical form of the Earth to gravity surveys by pendulum, the dynamical variation of latitudes, the investigation and control of atmospheric refraction—were elucidated with knowledge and insight worthy of the secular observations, involving the discovery of Precession and Nutation and of Aberration, organised by the previous generations of British workers. And it is to be noted that though the scientific discussions in the papers here described were never given to the world, and Britain seems to have been then largely isolated except in mechanical construction, yet they were in the hands of Maskelyne and Hutton and the other writers who compiled the published accounts, and of the committees with which they worked, and were in fact drawn up for their use.

The time and concentration required for the effective prosecution of such studies over so wide a range, especially when combined with the fundamental experimental determinations in other sciences that went on in the same years, were amply sufficient to account for the strengthening of Cavendish's habits of reserve and isolation. He was a natural philosopher so profound and universal as to have no time to be anything else.

FRAGMENTS ON MATHEMATICS

Cavendish seems to have spent much time perhaps at an early period on mathematical investigation: and like other great physicists he was equally at home in algebra and in arithmetical calculation. The manuscripts appear to be in no case mere copies from other sources, but reflect the writer's own thought. Though hampered in analysis by the fluxional method, it is clear that his mathematical powers were of a high order.

It will be sufficient as an indication of the range of his activity to give brief references to some of the more striking papers. He seems to have been making preparations at one time for a book on trigonometry. There exists a preliminary syllabus for a treatise on the principles of mechanics.

A fragment on the axioms of geometry: "If a line be of such a nature that no part of it can be made to pass through different points while the extremities of it remain fixed, it is called a straight line: otherwise it is called a curve line." "...A straight line is shorter than any other line which can be drawn between 2 points; for if this is denied it must be said that there may be drawn some curve line between the 2 points which is either as short or shorter than any line which can be drawn between those points, 'but that has been shown to be impossible.'"

Problems of geometrical maxima and minima, e.g. the circle has greatest area for given perimeter, developed at considerable length. "On the best proportion of the circles for the carpenter's oval."

"Surface of a cone cut by oblique plane" expressed by a series. "That sections of spheroids and parabolic cones are ellipses."

Dissection of a parallelogram so as to form another parallelogram.

"To find the shape of a parallelopiped from its appearance to the eye."

A summary of spherical trigonometry. "As to supplemental triangles in sph. trig."

A discussion of the form of triangles for which errors of measurement entail least results: "Fluxions of plane triangles."

A detailed discussion of the theory of deviation from a mean, in the form—Let A and B whose chances of winning are as $a : b$ play a very great number (n) of games together, to find the probability that A's gains neither exceed nor fall short of the probable estimate by more than $p\sqrt{n}$ stakes.

The ways of paying a given sum in coins of x, y, and z units.

A number of problems in algebra, sometimes worked out in extensive detail, such as "To find 3 or 4 numbers the sum, sum squares, sum cubes and sum fourth powers being given."

Newton's rule for interpolations: with numerical table headed "Interpolating by second differences, correction to be added or subst. to number formed by 1st diff."

OPTICAL FRAGMENTS

There is a discussion "of the figure of glasses necessary to bring rays to a focus and of the aberration of rays"; also on the error of deviation due to a prism being placed slightly out of the plane of symmetry and the position of symmetrical deviation.

There is a calculation "on the aberration in reflecting telescopes used in Herschel's manner"; and another with numerical values "on the aberration of rays passing through a spherical lens."

There is a discussion of the best conditions for vision of the wire micrometer.

He took an interest in the chromatic aberration of light, and made many experiments on the compensation of prisms of various materials solid and liquid, with mathematical calculations, between 1770 and 1790. It does not appear that any distinct conclusions emerged. There is a fragment on the condition for an achromatic object glass: Dollond, who perfected the achromatic telescope, died in 1761.

In other fragments the problem of astronomical refraction is discussed in general terms: especially he considered that the effects of moisture and temperature of the air required separate discussion and investigation. His estimate of the effect of temperature on observations on a mountain slope is reprinted below (p. 406). As regards local refraction in an observatory:

If the strata of air near the opening of the observatory and within the observatory were horizontal it would certainly be right to place the thermometer by which you estimate the heat as near as possible to the object glass: but in reality I imagine that they will be far from horizontal, and consequently there is so little dependance upon that part of the refraction which the ray suffers in passing from the outside of the observatory to the object glass that it seems indifferent whether the thermometer is placed within or on the outside of the observatory, from which I think the only way to be exact is to endeavour to make the air inside the observatory as nearly of the same heat as that outside as possible, though I imagine astronomers will not think this a very pleasant method in winter.

He discusses the correction of the refraction for variations of the thermometer and barometer.

An estimate is made of the amount of light stopped in passing across a shower of rain.

ATTRACTIONS AND GEODESY

There is a set of carefully prepared manuscripts mainly discussing the conditions of choice of a mountain for observation of deflection of the plumb-line, preparatory to the Schehallien experiment, probably for the use of the Committee of the Royal Society (1772–4). One of them is entitled in another hand "Mr Cavendish's rules for computing the attraction of mountains on plumb-lines."

There is a manuscript "On the choice of hills proper for observing attraction: given to Dr Franklin," which begins as follows: "The Royal Society are desirous of making some experiments to examine whether hills are capable of exerting any sensible attraction, and how great it is, as a means of determining the mean density of the Earth: and they are in search of hills proper for the purpose...."

Another packet is entitled "Computation for Skidda." Near the end of his "Figure of the Earth," Airy refers to Hutton's calculations (*Phil. Trans.* 1778) for the reduction of the Schehallien observations: "Indeed the various contrivances of calculation in this Paper will be found well worth the attention of any practical person." In a footnote he adds "Most of these it appears were suggested by Mr Cavendish. And it appears that nearly all the preliminary calculations of the attraction of Skiddaw, etc. were made by Mr Cavendish. This we have ascertained from an inspection of his papers, which we have had an opportunity of examining through the kind permission of his Grace the Duke of Devonshire."

There is a manuscript entitled "Paper given to Maskelyne relating to attraction and form of the Earth" to which the following letter refers. It is addressed

To the Hon^ble Henry Cavendish
at Lord Charles Cavendish's
Great Malbro' Street.

Greenwich Jan. 5 1773.

Dear Sir,

Inclosed I return you your rules and directions for the choice of hills having a considerable attraction; which I have taken the liberty to take a copy of: I think them well calculated to procure us the information that is wanted. According to your Table, I should estimate, that the valley called Glent-Tilt, lying on the N.W. side of the mountain Ben-Glae in Scotland, should produce a defect of attraction on the two opposite sides of 36″, supposing the mean depth 1000 yards, the shape spheroidical and the length of the valley 8 miles, the breadth 4 miles (I believe it is less) and the angle which the direction of the valley makes with the meridian 50°. Col. Roy, from whose account these dimensions are taken, says it makes an angle of 50° or more with the true meridian. If the mean density of the earth exceed that at the surface 5 times[1], there will still remain 7″ attraction. I think the dimensions of this extraordinary valley deserve a more particular inquiry. I shall be obliged to you for a line to acquaint me, whether you found any thing material in those papers of the late Mr Robins, which you examined that have not been printed; as the proof of Mr Call's paper (making mention of them at the end of his account of the draught of the 12 signs in an Indian Pagoda) is now in my hands; and I would add a note about the papers. I am Sir,

Your very humble Servt,

N. Maskelyne.

[1] [As regards this excessive estimate, cf. p. 404.]

The deviation of the plumb-line by the tides was under discussion as the two following extracts show:

Suppose that the tide in the Bristol channel rises 50 feet that the channe is 10 mile broad (which I imagine must be its utmost breadth in any part where the tide rises to that height) and that it is extended infinitely both ways in length in a straight line: suppose too that at high water the sea touches the bottom of the cliff and retreats 50 feet from it at low water: the difference of attraction at high and low water on a particle of matter at the foot of the cliff is 797 feet, which would make the plumb line deviate 1¾ seconds if the mean density of the earth is the same as that of the surface.

If the sea retreats 400 feet from the cliff at low water everything else being the same the difference of attraction is 588 feet.

If the sea is 60 mile broad and the tide rises 40 feet the high water mark touching the cliff and the low water mark 50 feet distance the difference of attraction is 780 feet.

Since I saw you I have looked again into Boscovich's book (De litteraria expeditione &[a].) and find that what he says about the attraction of the tides does not differ so much from what I said in the paper I gave you as I imagined: he supposes the arm of the sea to be 100 mile broad in which case he says the plumb line will deviate 2″·38‴ if the mean density of the earth is the same as that of the sea [of the surface?]. I supposed the sea to be 10 miles broad and found according to one supposition that it should make the plumb line deviate 1″¾.

There is a paper entitled "Rules for computing the error caused in measuring degrees of latitude by the attraction of hilly countries." By calculating for a section of Italy perpendicular to the Apennines he estimates that on an arc of 2⅛ degrees the length of a degree would be shortened by 210 toises, supposing the mean density of the Earth to be that of the surface.

He considers a section through the middle of the degree that had been measured in Pennsylvania, and perpendicular to the Alleghanies which are inclined at 37° to the meridian, and makes out that the attraction of the hills increased it by 24 toises. "The whole effect of want of attraction of the Atlantic = 1·376 which should diminish the length of a degree by 119 toises." An estimate for the degree measured at the Cape of Good Hope is a shortening of 174 toises due to the proximity of the ocean and a lengthening of 33 due to a range of hills. The attraction of the Cordillera, near Quito, is treated in detail on the basis of the diagrams in Bouguer's *Figure de la Terre*; as pendulum observations were available[1].

The following conclusions of a carefully prepared manuscript may be of

[1] [For historical detail as to all these surveys see Airy's *Figure of the Earth*, a very complete and important treatise written for the *Encyclopaedia Metropolitana* in 1830 before he left Cambridge for Greenwich.]

historical interest as indicating how far advanced the ideas of gravitational geodesy were in Cavendish's hands at this early date.

The attraction of the Cordillera on the pendulum at Quito will be much the same as if it was extended infinitely of the same level in all directions: only in order to allow for the attraction of Pinchincha and the other mountains rising above Quito which rather diminish the gravity let us suppose Pinchincha to be a segment of a sphere its altitude being 968 toises and the radius of its base 4800 toises. The height of Quito above the level of the sea is 1466. Whence the mean density of the earth should be 4,44 times that of the surface. If the observed difference of lengths had been $\frac{1}{100}$ of line less the mean density would have come out 3,83.

If there should be a greater quantity of matter under the Cordillera of less than the usual density of the surface of the earth (which is not unlikely considering that all the hills in have probably been volcanoes) the length of the pendulum at Quito or Pinchincha will be less than it would otherwise be which would make the mean density of the earth appear greater than it really is.

It is likely that the mean density of the earth should be several times greater than that of the surface though the internal parts of the earth are composed of the same materials as the surface, as the density of the internal parts may very likely [be] increased many times by the great pressure which they suffer.

These results, which he compares with estimates based on Canton's measure of the compressibility of water, are of course greatly in excess of the strikingly correct guess of Newton. A previous estimate led him to a ratio less than 3.

I know but 2 practicable ways[1] of finding the density of the earth first by the going of a pendulum in the foregoing manner and secondly by finding the deviation of the plumb line at the bottom of a mountain by taking the meridian altitudes of stars. The first way is by much the most easy but the latter seems much the most satisfactory.

If the mean density of the earth is the same as that of the surface the attraction of a conical hill will accelerate a pendulum at the top of it per day 32″,4 seconds into the versed sine of the angle which the side of the hill makes with the perpendicular. [Height not specified.]

The attraction of a hill in the form of a small segment of a sphere will make the plumb line deviate at its ft. 32,8 seconds. So that in this respect the latter method seems rather more exact. But the main point is that in the latter method the result seems much less affected by any irregularity in the density of the internal parts of the earth than in the former method.

Let us suppose for example that the earth consists of a nucleus *abd* covered with an outer crust of a considerably different density and let us suppose that

[1] [This was of course written long before Michell's plan by torsional vibration was known, which Cavendish carried out in 1798. The date of the Peru arcs is 1736, of Clairaut's *Figure de la Terre* is 1743, of Canton's experiments on compression of water is 1762, of the Schehallien experiment is 1778.]

this nucleus is of rather an irregular figure and consequently that the outer crust is much thinner in some places as for example in A than in others, it is plain that the force of gravity on the surface at A and at a height above the surface will not be to each other in the inverse duplicate ratio of their distances from the center as they would otherwise be and consequently no certain conclusion could be drawn from experiments of the pendulum at the top and bottom of a mountain in such place. Whereas in the latter method of finding the density of the earth I do not imagine that the result will be sensibly affected by any irregularity of this kind. That there is some such irregularity in the structure of the earth seems likely I think from observations of the pend. in different places, which differ more than I should think could be owing to the error of experiment particularly if you compare...

There follows a numerical discussion of the consistency of the observed variations of gravity with the latitude. Similar discrepancies appeared in a discussion by Bouguer: this subject is emphasized by Airy, *loc. cit.*

If the earth is a regular spheroid the length of the pendulum swinging seconds increases in going from the equator to the pole in proportion to the square of the sine of latitude or in proportion to the versed sine of 2^{ce} the latitude. The 4th column of the following table contains the length of the pendulum computed according to this rule on a supposition that the difference of length between the equator and pole is 2,3 lines, the length at the equator being chose such that the sum of the numbers in the 5th column (which are the excesses of the observed lengths above the foregoing) shall be nothing; the 6th column gives the mean of the 6 first and of the 4 last excesses in the foregoing column and the 7th, 8th and 9th columns are the same things on a supposition that the difference of the length between the equator and pole is 2,5 lines.

It appears from hence that in general if you suppose the difference of length between the equator and pole to be 2,3 lines the observed lengths fall short of the computed by more in the lower latitudes than in the higher, whereas if you suppose the difference to be 2,5 lines they fall short more in the higher latitudes than in the lower, as appears more plainly by the 6th and 9th columns in which are given the mean excess for the 6 first places (which may be looked upon as the lower latitudes as the versed sines of the doubled latitudes of those places are considerably less than the mean) and of the 4 last places which may be considered as the higher latitudes; whence it seems as if the true difference of length was between 2,3 and 2,5 lines or $\frac{1}{184}$ of the whole length.

It has been demonstrated by Clairaut that if the earth consists of spheroidical strata not much different from spheres and that the density is the same in all parts of the same stratum that the difference of the 2 axes divided by the whole axis exceeds $\frac{1}{230}$ as much as the difference of gravity divided by the whole force of gravity falls short of it, whence it should seem as if the difference of the axes was about $\frac{1}{308}$ which is a quantity which will agree much better with the precession of the equinoxes than a greater difference. The measures of a degree agree rather better with the supposition of $\frac{1}{230}$ than with that of $\frac{1}{300}$ but the difference is not great[1].

I think it would very well become the admiralty to send a vessel to observe the longitudes of such places as are most frequented by our ships as till then the method of finding the longitude at sea will be of very imperfect use. If they should do so it will tell us the length of the pendulum in many different climates without any addition trouble. I suppose you will think the most convenient way of finding the longitude will be by taking the difference in time of the transits of the moon and some fixed stars by a transit instrument. If they dont propose to stay long in any place I should think the best way would be not to endeavour to fix the instrument in the meridian but to point it to the pole star a little before the first observation and mark the time then to take the transit the moon and 2 or more stars one of which should if possible be before the moon and the other after it and the nearer to the same parallel of declination the better. This by an easy calculation will determine the going of the clock and the times of the transits so that the longitude might be determined with tolerable accuracy even in a single night: after the observ. is over you may see whether the instr. has altered its place by seeing how many revol. of the adj. screw it requires to make the instr. point again to the pole star.

It is needless saying that the instrument ought to stand on the ground and not to touch the walls of the observatory: there should also be a floor to the observatory which should be supported by the walls of the observatory and should by no means touch the instrument or rest on the ground near it as without these precautions it is impossible that the instrument should be steady.

Finally there is his estimate of the error from refraction on a sloping hill-side, probably in connexion with Schehallien, as follows (see *supra*, p. 393).

On the irregular refraction caused by the heat being different near the side of the hill from what it is at a distance from it.

Let the top of the hill be B feet above the place of observation, and let it be elevated above the horizon in an angle whose tangent is T; suppose that there is N degrees difference between the heat of the air at the top of the hill and at the same height perpendicularly over the place of observation. The alteration caused in the density of the air by N degrees of heat is the same as is caused by $N \times \frac{28800}{480} = 60N$ feet of altitude; 26800 feet being the height of an uniform atmosphere; Therefore if a line be drawn through the summit of the hill, in such manner that the density of the air shall be the same in all parts of it (or

[1] [See the section on Precession of the Equinoxes, under the heading Astronomy (p. 436), for further consideration of these questions, in relation to the internal constitution of the Earth. In connexion with the there following section on Tidal Friction, and its argument which only needed the consideration of angular momentum (by Kelvin and Darwin) to complete it, it may be noted that Kant's theory on this subject dates from 1754, and Thomas Wright's views on cosmogony from four years earlier.]

a line of uniform density as I shall call it) the tangent of inclination of that line to the horizon will be $\frac{60 \times NT}{B}$ seconds. If we suppose that the heat of the air at any intermediate height on the side of the hill differs from the heat at the same height perpendicularly over the place of observation by a quantity which is to N degrees as the height of that place above the place of observation is to B, the line of uniform density will be equally inclined to the horizon at all heights less than that of the top of the hill; but at a little height above it I suppose the line of uniform density will be nearly horizontal. Now the refraction of a star at no very great distance from the zenith = $57'' \times$ tan. zenith distance; and its refraction in passing through a portion of the atmosphere answering to a difference in the height of the barometer equal to D inches is $57'' \times \frac{D}{30} \times$ tan. zen. dist. whence we may conclude that the irregular refraction of a star near the zenith caused by this inclination of the line of uniform density to the horizon will be $57'' \times \frac{D}{30} \times \frac{60NT}{B} = \frac{114DNT}{B}$ which as $D = \frac{28B}{26800}$ nearly (the mean height of the barometer on the side of the hill being supposed 28 inches) equals $'',12 \times NT$.

If we suppose the inclination of the side of the hill to be $21°.50''$ and therefore T to be $= ,4$ and $N = 12°\frac{1}{2}$ which in all probability is on the outside of the truth the irregular refraction will be only $\frac{6}{10}$ of a second.

MECHANICS AND DYNAMICAL THEORY

There is a list of titles, possibly in Dean Peacock's writing, with a memorandum "Sent to Lord Brougham March 28, 1845, Theory of the Kite, and On Flying."

The early paper endorsed "Remarks relating to the Theory of Motion" has been printed *verbatim*, p. 415. It is very clearly written, with numerous precise erasures as the composition proceeded. It is in fact an argument for the conservation of mechanical and thermal energy, which the writer proposes to name "mechanical momentum" in distinction from "ordinary momentum" which depends on direction[1]. After a demonstration of the conservation of mechanical energy, and a specific introduction of the idea of potential energy, he infers with great distinctness that heat must be the "mechanical momentum" of the internal vibrations of bodies: but in proceeding to the energies and heats of chemical affinity he grasps the

[1] [Compare Young's discussion of the subject in his *Lectures* (1807), VIII, On Collision, in which the term energy was first introduced. The tacit avoidance by Cavendish of the term *vis viva*, made classical by Leibniz and D. Bernoulli, is noteworthy, and possibly arose from a connotation then more prominent than now: the absence of knowledge of the more analytical writings of d'Alembert would be natural in a British physicist of his time.]

new features and concludes that "this way of explaining it is insufficient[1]." Incidentally there is a discussion of a tinfoil crusher-gauge for transient pressures. The manuscript probably belongs to an early period: but the only clue to a date is a reference to *Phil. Trans.* No. 477, which is unavailing as that part was published in 1745. The controversy which he elucidates, as to whether force is measured properly by change of momentum or by "mechanical momentum," is supposed to have been composed at any rate from the point of view of the Continent where it was mainly carried on, by d'Alembert in 1743 in his *Traité de Dynamique.*

Various special problems relating to moving dynamical systems are solved in separate papers: but the work contains nothing remarkable.

A manuscript of considerable length developed on geometrical lines "On the motion of a solid of revolution from its centrifugal force."

A calculation entitled "Catenaria blown by the wind" with an estimate of "resistance of air on chain": also an investigation "On the form of the Catenaria in which the strength of the chain is everywhere proportional to the tension."

A mathematical and experimental investigation, with extensive calculations, of the strength under flexure of wooden bars of various sections sawn from logs, and their vibrations when one end is fixed, with a view

[1] [It is sometimes stated that Cavendish's views on the question whether heat is a substance were ambiguous. No such epithet can apply to the very remarkable corollaries at the end of this paper, which so clearly anticipate the modern doctrine of energy. It is unfortunate that there is no means of assigning a date to these expository essays; one is inclined to refer them mainly to the early period of study, before he joined the Royal Society in 1760 at 29 years of age. The unsystematic form of his special dynamical reasoning makes it unlikely that as regards it Cavendish was indebted to previous writers, and so is in favour of an early date. The earlier electrical manuscripts, largely experimental, of which "the style...leaves no doubt that they were intended to form a book" belong to the years 1771–3 (see Clerk Maxwell's very illuminating *Introduction*).

The heat as well as the pressure of air had been referred mathematically, in a general way, to the *vis viva* of the free motions of its particles or molecules by D. Bernoulli, *Hydrodynamica*, as early as 1738 (cf. Cor. 1 *infra*); and he extended widely the principle of Vis Viva, as Cavendish does in this paper, in *Berlin Memoirs* ten years later (cf. Lagrange, *Méc. Anal.*).

In Maxwell's *Theory of Heat*, p. 72, there is an interesting discussion of the implication in Black's term latent heat, that heat is an imponderable substance, as distinguished from ponderable gases which Black was himself the first to recognize as distinct kinds of matter. "The analogy between the free and fixed states of carbonic acid and the sensible and latent states of heat encouraged the growth of materialistic phrases as applied to heat: and it is evident that the same way of thinking led electricians to the notion of disguised or dissimulated electricity, a notion which survives even yet, and which is not so easily stripped of its erroneous connotation as the phrase 'latent heat.' It is worthy of remark that Cavendish, though one of the greatest chemical discoverers of the age, would not accept the term 'latent heat.'" Maxwell goes on to quote a footnote (see p. 151 *supra*) of *Phil. Trans.* 1783 as from J. D. Forbes, *Encyc. Brit.*, Dissertation VI.]

to computation of the tapering lines for masts: also a discussion of the "vibrations of a straight uniform spring left to itself."

An investigation of the form of a solid of given volume so that its attraction at a point shall be maximum. [See Todhunter's *History*.]

A manuscript apparently intended to be sent to Dr Hutton entitled "Explanation of Mr Hutton's solution of Maseres' problem about vibrating string."

Form of arch for various modes of loading.

Fragments on the resistance of the air to projectiles, in connexion with Newton's *Principia*.

A geometrical and analytical paper on the "map pentagraph."

"Calcul. of force of engine turned by reaction of two jets of water for mechanical purposes."

"On the vibrations of pendulums whose centers of suspension move." Young's result is obtained that the effect is the same as if the pendulum were prolonged to a fixed centre. (For rolling motion, no correction is required.)

"Concerning the spinning of tops by Mr Mitchell." Doubtless his friend, Rev. John Michell[1] (1724–1793). "This point is what is known by the name of the center of percussion. How I came not to take notice of it I do not know."

"Pendulum." Records of swings on different days in case and out of case. "It is supposed that in this and the following pages the pendulum was compared with father's clock in new building."

"Pendulum with rolling motion by Nairne." A close scrutiny of two pendulums with reference to symmetry and slipping on edge and decrement of free swing. "Hence it should seem that the time was nearly equal to the difference of the logarithms of the 2 arches of vibration multiplied by 225: and therefore the time in which it changes from 2° to 1° should be $67\frac{1}{2}'$: by a mean of the observation in p. 7 it seemed to do the same with the former motion in $63'$."

A long series of experiments on moduli of bend and twist for glass tubes and iron and brass wires under load. The ratios seem to be very concordant. Further "experiments on twisting of wires of silver, iron and other metals tried by the time of a vibration."

In a manuscript "Concerning waves" there are general remarks on the character of wave-motion on water: and interesting guesses of the cause of the discrepancy between the velocity of sound and Newton's theoretical value, considering after Euler particles repelling according to the law r^{-1} or near thereto, finally rejecting all proposed explanations.

"Concerning the vibrations of pendulums suspended from the same horizontal bar." An unsatisfactory attempt at general explanation of the

[1] [See short *Memoir of John Michell*, by Sir Archibald Geikie, Cambridge University Press, 1918.]

fact that "If 2 clocks, the length of whose pendulums never differ by above a certain quantity, are fixed to the same horizontal bar, they will keep moving constantly together: one clock will never be before or behind the other by as much as one vibration." [See Ellicott, *Phil. Trans.* 1739.]

"On centrifugal pendulum" including a form of isochronous check.

A paper on the "Vena Contracta": nothing of note except a careful diagram of form near the orifice.

A series of carefully reduced experiments on flow of water through a glass tube ·2117 inches in diameter and 44·1 inches long: "the pressure required to overcome the friction in velocities of 34 and 19 in. per $''$ = $\frac{1}{368}$ of the length multiplied by the $\frac{5}{3}$ power of the vel. in in. per $''$ but in greater velocities seems to increase in a greater proportion."

A discussion of the mode of action of spiral springs.

"On error in pend. beating dead, supposing the accel. from the action of clockwork to be uniform while weight acts upon it, and that it is uniformly retarded during rest of time with such force as to keep the vibrations of same length." Inconclusive.

"Question about Tower of Babel." Triangular pyramid?

A paper "On the shape of the teeth in rack work": encloses a beautifully written manuscript ending with the following on a separate page:

Dr Young takes the liberty of sending for Mr Cavendish's inspection a copy of what he means, with Mr Cavendish's permission, to insert in his syllabus respecting the teeth of wheels. He believes that the point of contact *G* will seldom if ever fall between *E* and *F*.

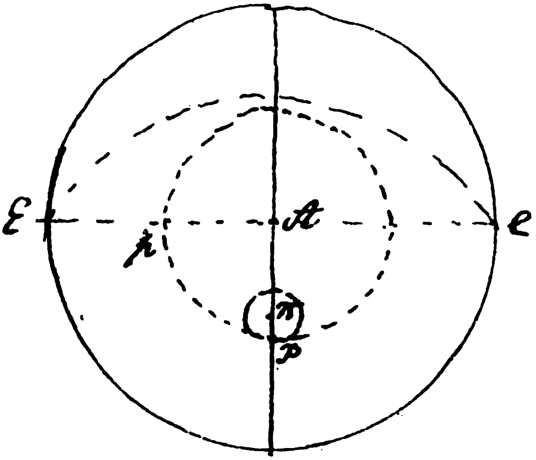
Welbeck St. Thursday 3 Sept 1801.

In "A Syllabus of a course of lectures on Natural and Experimental Philosophy" printed at the press of the Royal Institution in 1802, this memorandum slightly changed occupies §§ 178–181: and in § 179 which contains the argument in small print there is the sentence "For the substance of this demonstration I am indebted to Mr Cavendish."

DYNAMICAL VARIATION OF LATITUDE

Let an oblate spheroid revolve round an axis not coinciding with the axis of the spheroid, and suppose this spheroid to be placed at such a distance from any other matter as not to be influenced by the attraction thereof. Let *A* be the pole of the spheroid, and *P* the pole on which it revolves, *Ee* the equator, the motion being from *E* to *e*; and let the difference of the axes of the spheroid divided by the whole axis be called α. The point *A* endeavours by the centrifugal force to move towards *P*; by this compound motion the point *P*, or the point on which the spheroid actually revolves, is continually shifting its place on the surface of the spheroid, *id est* the spheroid revolves on a different part of its substance from what it did before; the motion of the point *P* being such as to describe a circle round *A* as a center in the direction from *P* to *p* (*id est* in the same direction that the spheroid revolves) in the same time that it makes $\frac{1}{\alpha \times \text{cs} AP}$ revolutions round its axis.

With respect to absolute space the point P describes a small circle round the point π: in the same time that the spheroid makes 1 revolution round its axis, and in the same direction; the radius $P\pi$ being to PA [strictly sin $P\pi$ to sin PA] as $\alpha \times \text{cs}\, PA$ to one.



If the axis on which the earth revolves does not coincide with the axis of the spheroid, I imagine the precession of the equinoxes and nutation of the pole will not be sensibly affected thereby; but the pole will revolve on the surface of the earth round the axis of the spheroid in 230 days (supposing the difference of the axes to be $\frac{1}{230}$ part[1]) being sometimes on the European side thereof and sometimes on the American: whereby the latitudes of all the places on the earth will be continually altering, but the pole will constantly point to the same part of the heavens that it would otherwise do. Suppose for example that the pole of the earth was removed 1″ from the pole of the spheroid, at one time the latitude of London and all the places in the same meridian would be 1″ more northerly than the mean: consequently the sun and all the stars would cross the meridian 1″ more to the south than they would otherwise do: but in about

[1] [As regards this estimate, see p. 405. The correct Eulerian factor is $C/(C-A)$, where C, A are the moments of inertia, being 304 days. This supposes the earth to be rigid: if not, $C-A$ must be the value when the distortion produced by centrifugal force of diurnal rotation is supposed taken off, which should give the observed value 428 days. See *M. N. R. Astron. Soc.* Nov. 1906; *Proc. Cambridge Phil. Soc.* May, 1896. For a homogeneous spheroid the Eulerian period for small amplitude would be α^{-1}, as the text indicates: thus Cavendish was probably acquainted with Euler's general abstract result (*Mechanica*, 1736; *Theoria Motus*, 1765) which he here converts (possibly for the first time) into practical geometrical form, not far removed from the modern one in terms of Poinsot's rolling momental ellipsoid. Cf. *Proc. Camb. Phil. Soc.* 1896. In fact the rotational motion of the Earth is represented precisely by the circle pP of the diagram rolling on the much smaller fixed circle with centre π situated inside it.]

115 days afterwards the latitude of the same places would be 1″ more southerly than the mean, and then all the stars would cross the meridian 1″ too much to the north.

The historical interest of this subject has prompted renewed examination of the lengthy and complex computations in the manuscript, "On the motion of a solid of revolution from its centrifugal force." By powerful but hardly elegant use of spherical geometry he arrives at an expression for the centrifugal effect of the motion, giving a final result of the same type as in this fragment: but it would involve a tedious examination to find whether the value of his coefficient (α) agrees in general with the Eulerian one.

As a geometrical lemma he establishes the law for finding the angular velocity which is the resultant of three given ones round axes OP, Op, Om marked by points P, p, m on a spherical surface.

There is also a separate paper with the object probably of making the problem of free rotation manageable, in the absence of the resources of analysis, for the general form of solid without an axis of symmetry. He establishes for the case of a right solid the principle of kinetic equivalence: "the resistance of the whole solid is the same...as if $\frac{1}{6}$ of the whole quant. matter was placed in the centers of each of the six faces of the parallelepiped," which reduces that problem to one relating to particles.

In Euler, *De Motu*, 1765, Ch. XII there is only a passing reference to the effect on latitudes, which is the application that is of outstanding modern importance, and is Cavendish's main concern in the formulation here reproduced.

EFFICIENCY OF AN UNDERSHOT WATER WHEEL

Calculation of maximum and force of undershot wheels in which the float boards are of such a breadth that the water shall dash over them and in which little or no water can escape between the floats and the sides.

All the water which issues through the sluice loses the excess of its velocity above that of the wheel.

Therefore the pressure upon the wheel is such as would in a given time communicate to all the water which issues through the sluice in that time a velocity equal to the excess of the velocity of the issuing [entering?] water above that of the wheel and therefore (as the quantity of water which issues through the sluice in a given time is given) is proportional to that excess.

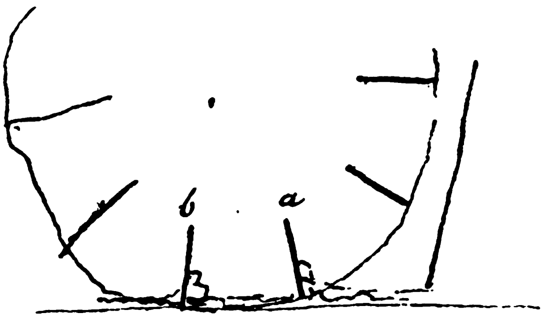
Therefore the quantity of work done by the wheel is proportional to the velocity of the wheel into the excess of velocity of the issuing water above that of the wheel and is a maximum when the velocity of the wheel is $\frac{1}{2}$ that of the issuing water.

The pressure acting upon the wheel when it moves with $\frac{1}{2}$ the velocity of the issuing water is $\frac{1}{2}$ the weight of the water which issues from the sluice in the time in which a heavy body will in falling by its own weight acquire the velocity of the issuing water.

The velocity of the wheel in the same case is such as would in the abovesaid time move through a space equal to the height from which a body must fall to acquire the velocity of the issuing water.

Therefore the greatest effect of an undershot wheel is such as would in a given time raise a weight equal to that of ½ the water which issues from the sluice in that time to the height from which a body must fall to acquire the velocity of the issuing water.

The fallacy of the common answer to this problem consists chiefly in supposing the water to act only upon that float nearest the sluice whereas in reality the



water contained between the float (*a*) and (*b*) continues to act upon the float (*b*) (after the float (*a*) is interposed between it and the sluice) till it comes to move with no greater velocity than the wheel.

In a manuscript "On the engine for raising water by a centrifugal force" it is shown that when the water spurts out from the end of a rotating horizontal pipe "The force [work] requisite to move this engine for any time is such as is sufficient to raise the quantity of water which issues from the horiz. pipe in that time to 2ce the height βD." The difference appears of course chiefly as kinetic energy of the issuing water.

ON THE MOTION OF SOUND

The method of proof in the carefully written manuscript, which he describes as *infra*, is to assume that when the piston vibrates *in any manner* the air along the tube takes up the same type of vibration but after a time represented by the velocity of propagation, and then to verify that this description fits with the motion of a portion of air as determined by the difference of pressure at its two ends, provided however the compression is small. There is a discussion of what may be expected when the amplitude of vibration is so great that this verification holds good only for the turning points of the vibration, remotely after the manner of Rayleigh's *Theory of Sound*, II, § 251. It is noted that the velocity of a wind adds to that of the sound.

The occasion of my writing this paper was as follows Sr I. N. [Newton] proved that if the vibrating body moved with a velocity regulated according to the same law as a pendulum vibrating in a cycloid, the particles of air would vibrate with a velocity regulated according to the same law, but does not prove that they will if the body vibrates in any other manner. Euler proved that Newtons demonstration was equally applicable if the body moved in the same manner as a body vibrating in a parabola and some other figures and if I mistake not supposed it an objection to the demonstration; this made me suspect that the case was that according to whatever law the body vibrated the air would do the same and set about trying to demonstrate it.

Besides making it general there are 2 or 3 other points in which this demonstration differs from Newtons first in reducing his 3 propositions into one which makes it a good deal clearer 2ndly it is drawn from the well known property that the density of the air is in proportion to the compression instead of the property which Newton deduces from it that the particles of air repel with a force inversely as the distance and 3rd that in the 1st part the air is supposed to be confined in a straight uniform canal because Newtons demonstration is in reality applicable only to that case. In the 2nd part however it is extended to the case of a conical canal which comes to much the same thing as if the sound is produced in the open air.

THE DISCHARGE OF A CANNON BALL

Internal ballistics. "Investigation of the loss of force owing to the inertia of the powder."

The density of the inflamed air will be greater at bottom of the barrel than near the bullet as having a greater quantity of matter to put in motion; the proportion which the density at bottom and at bullet near to each other remains pretty nearly the same in whatever part of the barrel the bullet is in after the space through which the bullet has moved bears any considerable proportion to the space occupied by the powder. The force with which the powder actually acts upon the ball is to the force with which it would act upon it if the powder had not inertia as the density of the inflamed air at the bullet to the mean density or the density which it would be of supposing it every where alike.

On this basis a calculation is made by fluxions which allows also for the escape of the inflamed air past the sides of the ball, on the basis "Elasticity of air gener. by powder before ball is moved = 1000 atmospheres": of course the cooling effect of expansion does not appear. Results and data from "Rob" (= Robins) are adopted as follows:

Height bullet discharged with vel. 1700 would rise in vacuo = 44930 feet = 8½ miles therefore its greatest range would be 17 miles its actual range is supposed less than ½ mile.

Vel. of 24l. cannon ball discharged from piece 10 feet long with full charge of powder id est 16 pounds = near 1650 feet per " the greatest range of such a shot in vacuo would be about 16 miles its real range is less than 3 miles its resistance at that vel. is near 23 times its weight.

REMARKS ON THE THEORY OF MOTION

If the force by which a body is accelerated is given the velocity acquired by it will be as the time during which it is accelerated. If the time is given the velocity acquired will be as the accelerating force.

Therefore the velocity acquired by any body is as the accelerating force multiplied into the time during which it acts.

The velocity acquired by a body acted on by a given accelerating force is as the square root of the space through which it is accelerated. If a body accelerated by a given force acquires a certain velocity v in falling through a given space s the same body acted upon by a force which is to the 1st as (a) to (1) will in falling through the same space acquire the velocity $v \times \sqrt{a}$ for the body will in the same time that it described the space s by the 1st force describe the space $s \times a$ by the 2nd and at the end of it acquire the velocity $v \times a$ therefore if in falling through the space $(s \times a)$ it will acquire the velocity $v \times a$ in falling through the space (s) it will acquire the velocity $v \times \sqrt{a}$.

Therefore the velocity acquired by a body falling through a given space is in the subduplicate ratios of the accelerating force.

Therefore the velocity acquired by a body is as the square root of the accelerating force multiplied into the square root of the space passed over.

From hence appears the nature of the dispute concerning the force of bodies in motion; for if you measure this force by the pressure multiplied into the time during which it acts the quantity of force which a moving body will overcome or the force requisite to put a body in motion or in other words the force of a body in motion will be as the velocity multiplied into the quantity of matter, but if you measure it by the pressure multiplied into the space through which it acts upon the body the force of a body in motion will be as the square of velocity multiplied into the quantity of matter. The 1st way of computing the force of bodies in motion is most convenient in most Philosophical enquiries but the other is also very often of use, as the total effect which a body in motion will have in any mechanical purposes is as the quantity of matter multiplied into the square of its velocity; for in all mechanical purposes the force must be measured by the weight or resistance to be overcome multiplied into the height to which it is raised or the space through which it is moved, thus it requires an equal force to raise the weight of one pound 2 yards as it does to raise 2 pound 1 yard for the same force which is employed to raise 1 pound to 2 yards will by a proper machine raise 2 pounds 1 yard height. What I have here said will appear plainer by examples.

If a body moving with 1 degree of velocity is able to compress 1 spring to a certain distance the same body moving with 2 degrees of velocity will compress 4 springs to the same distance.

If in any machine a weight by descending communicates any degree of motion to the parts of the machine as in fig. 1st Pl. 1st [p. 428] where the weight (a) is suspended by the string AB which is wound round the axis BC so that the weight cannot descend without putting in motion the fly dfg, then if the weight in descending any given height can make the fly revolve with any given velocity it will require 4 times that weight descending from the same height or the same weight descending from 4 times that height to make the fly revolve with 2ce that velocity; when it is required to make the fly revolve with 2ce the velocity by 4 times the weight descending from the same height this will not be exactly true, because as part of the weight is employed in accelerating itself there is then a greater weight to be accelerated than in the first case.

For like manner if one man can by applying his strength in the most advantageous [way] communicate a given velocity to any machine it will require 4 men to work during the same time to communicate 2ce that motion to the engine, or it will require the force of one man for 4 times that time.

It must be observed here that as a man or any other cannot move with more than a certain velocity, and as the faster he moves the less force he is able to turn the engine with, therefore when it required to move the engine with 2 degrees of velocity the velocity with which the man moves should be only ½ as great in respect to that of the engine as when it is to be moved with only 1 degree of velocity.

The thickness of wall or timber which a cannon ball will force its way through is as the square of the velocity.

The work which an engine turn[ed] by a stream of water will do is as the quantity of water which strikes the wheel multiplied into the square of the velocity; for the pressure exerted upon the wheel is as the quantity of water × its velocity and the velocity with which the wheel may turn is as the velocity of the stream.

As this way of computing the forces of bodys in motion is very often of service, and as some theorems of considerable use in philosophy may be deduced from it, I would have some name by which to distinguish this way from the other; because it expresses the effect which a moving body will produce in mechanical purposes. I think it would not be amiss if it was called the mechanical force or *mechanical momentum* of bodies in motion.

The 1st Cor. It follows from the known property of the lever, namely that in order to an equilibrium the power and weight must be to each other inversely as their respective velocitys, that in fig. 1st the resistance to the motion of the fly caused by the force spent in accelerating in part of it, as *g*, is the same as would be caused by a body placed in any other part of the fly (for instance in its center of gravity) whose momentum (computed according to the usual manner) multiplied into its velocity is equal to that of the body *g* multiplied into its velocity, that is whose mechanical momentum is the same; and as this property obtains equally in all the other mechanical powers as in the lever we may conclude that in any mechanical engine whatever in which part of the weight is employed in putting the engine in motion, it being supposed to move freely without friction, that the mechanical momentum acquired by the engine whilst its center of gravity descends through a given space is the [same] as it would acquire if the whole matter of the engine was collected on its center of gravity or is the same as the whole matter of the engine would acquire by falling through the space which its centre of gravity descends: in most cases the truth of this is very evident and from what will come after it will appear that it is equally true in the most complex cases.

Lemma fig. 1st, 2nd, 3rd. Plate 2nd. If any force *D* acts at *D* upon the crooked lever *DGC* whose center of motion is *G* in the direction *DB* perpendicular to *DG*, and another force *C* sufficient to keep the lever in equilibrio acts at *C* in the direction *Cc* perpendicular to *CG*, then the force with which the center of motion *G* is pressed in the direction *Gg* or in the same direction in which *D* acts is equal to the force $D +$ that part of *C* which acts in the same direction as *D*; only it must be observed that as in 1st and 3rd figures the force *C* resolves it self into 2 the one acting in a direction perpendicular to that of *D* the other directly opposite to it, this last must be looked upon as negative and that in fig. 3rd the sum of $D +$ the above mentioned part of *C* will be negative because in that case *G* is pressed in central direction to *D*. The force with which *G* is pressed in the direction $GD =$ that part of the force *C* which acts in that direction. The truth of this Lemma is pretty evident by inspecting the figures.

When 2 bodies impinge on each other directly it is pretty evident that the center of gravity of the 2 bodies will move with the same velocity before and after the stroke, or the sum of their momenta taken in a given direction will be the same after the stroke as before, but it is not so plain that the same thing will take place when they impinge obliquely; this however may be shewn to be as constant a law as the other.

To do this no more is required than to prove Fig. 4th Pl. 2nd that the force which must be impressed upon the body *DNQ* whose center of gravity is *C* in the direction *BD* perpendicular to *DC* in order to give the body a given momentum in the line *BD* is the same which would be required to give it the same quantity of momentum provided the force was applied directly on its center of gravity.

The abovementioned force impressed at *D* will be spent partly in giving the center of gravity of the body a motion in the direction *BD* and partly in making the body revolve round its center of gravity; now it must be observed that as the body revolves round its center of gravity at the same time that the above mentioned center moves in the direction *BD* there will be a certain point in it which will be at rest immediately after the impression of the force, whilst all the other parts of the body have exactly the same motion as if they revolved round that point as a field center just as when a wheel rolls along that point which touches the ground stands still during the instant of its touching it. Let *G* taken somewhere in the line *DC* produced represent that point (for it will be somewhere in that line) and suppose the whole quantity of matter in the body to be collected in any number of points *LMH* and *a*. The velocity with which any point *L* moves is as *GL* and the direction of its motion in the line *lL* perpendicular to *GL*, the reaction of the point *L* or the force with which it resists being put into motion by drawing *L* from *L* towards *l* endeavours to make *G* move from *G* towards *O* were it not hinderd by the reaction of the rest of the body. By the preceding Lemma the force with which *G* is drawn towards *O* by the reaction of *L* is equal to that part of the force impressed at *D* which is spent in giving *L* its momentum + that part of the reaction of *L* which acts in the same direction as the force impressed at *D* (or since the reaction of a body put in motion is in a directly contrary direction to the motion of the body) —that force which is required to give the point L its velocity in the same direction as the force impressed at D. The case is the same in respect to any other point of the body. Therefore the force with which G is pressed towards O by the united reaction of all the parts of the body is equal to the whole force impressed at D minus the force which would be spent in giving the whole body its velocity in the direction BD supposing the force applied directly on its center of gravity; but as the point G is at rest this is necessarily nothing, therefore the force impressed at D is the same which would be required to give the body the same quantity of momentum if the force was applied directly on its center of gravity[1].

It appears also from the latter part of the preceding lemma that (as the velocity of M is proportional to MG) the force with which G is pressed by the resistance of M along the line DG either to or from G according to which side of DG the point M lies is proportional to $MG \times \frac{sr}{sm} = Mr$; hence appears the truth of what I before took for granted that G must be somewhere in the line passing through D and the center of gravity (for if it was not it must have a motion in the direction DG) and also that the center of gravity must move in a line parallel to BD.

When 2 perfectly elastick bodies of whatever shape strike each other in any manner whatever the sum of their mechanical momenta, not computed in one given direction but in any direction whatsoever, will be the same after the stroke as before.

When you say that the momenta of 2 bodies computed by the mass multiplied in the velocity are the same after the stroke as before you consider it only as made in a given direction, therefore if a body moves in a contrary direction you look upon its momentum as negative, if at right angles to it as nothing; but in this case whatever is the direction of a bodys motion I still look upon it as positive and the same as if it was made in any other direction.

There is no need here of running into disquisitions concerning the nature of absolute and relative motion; for suppose any given point to be at rest and compute the motion your system of bodies as they are in respect to that point, the truth of the proposition will be the same whether that point is really at rest or moving uniformly forwards in a right line[2].

1st case. Let the 2 bodies impinge directly on each other; suppose the quantity of matter in the 1st body as A and in the 2nd as B and let the velocity of the 1st body before the stroke be equal to $x + B$ and that of the 2nd to

[1] [This involved argument is to prove that the centre of gravity moves as if all the forces were transferred to that point and the mass were all collected there. The general theorem of conservation of the motion of the centre of gravity where there are no extraneous forces is established by Newton in the Introduction to the *Principia*: the present extension is ascribed by Lagrange to d'Alembert, and directly follows from his analytical method. Though Cavendish's method resembles Newton's in its geometrical character, he threshes the subject out for himself as usual, without any dependence on authorities.]

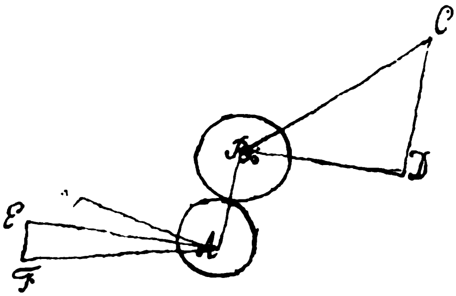
[2] [That is, interchanges of energy in impacts are not affected by a uniform motion of translation of the whole system: and this could not be so, unless the momentum is conserved.]

$x - A$; it is plain that by altering the value of x in respect of A, $x + B$ may be made to bear any assignd proportion to $x - A$ and therefore this is an universal expression for the velocity of the 2 bodies[1]; after the stroke the velocity of the 1st body $= x - B$ and that of the 2nd $x + A$; before the stroke the sum of the mechanical momenta of the bodies

$$= \overline{x + B}^2 \times A + \overline{x - A}^2 \times B = x^2A + x^2B + 2ABx - 2ABx + A^2B + B^2A$$

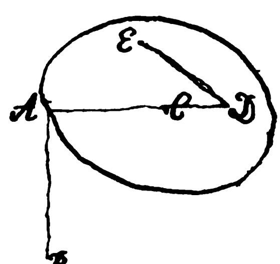
the momenta after the stroke $= \overline{x - B}^2 \times A + \overline{x + A}^2 \times B$ which is likewise equal to $x^2A + x^2B - 2ABx + 2ABx + A^2B + B^2A$ [whatever x may be].

Case 2nd. If any 2 bodies whose centers of gravity are A & B and the directions of whose motion are expressed by the lines FA and BC strike each other obliquely, supposing however that the point in which they strike one another and that they are void of friction by which means they will acquire no revolving



motion by the stroke, they will retain the same quantity of mechanical momentum after the stroke as before; for by resolving the motions FA and BC into FE and EA, CD and DB whereof FE and CD are parallel to the line AB joining their centers of gravity and EA and DB perpendicular to it, by the last proposition the sum of their mechanical momenta along the line FE or CD is the same before and after the stroke whilst their motions along EA and DB are not at all alter'd; therefore as whole mechanical momentum of it is equal to the sum of its mechanical momenta along FE and EA because $FA^2 = FE^2 + EA^2$ the sum of the entire mechanical momenta of A and B are the same after the stroke as before.

To prove that the same thing will take place computing the mechanical momentum of a body by the sum of the mechanical momenta of all its parts, though the bodies acquire a revolving motion from the stroke, we need only prove that supposing a body at rest whose center of gravity is C to be acted upon at A by any force in the direction BA perpendicular to AC, that the body will react at A with the same force as another body acted upon in the direction of its center of gravity of such a bigness that it should receive the same quantity of mechanical momentum.

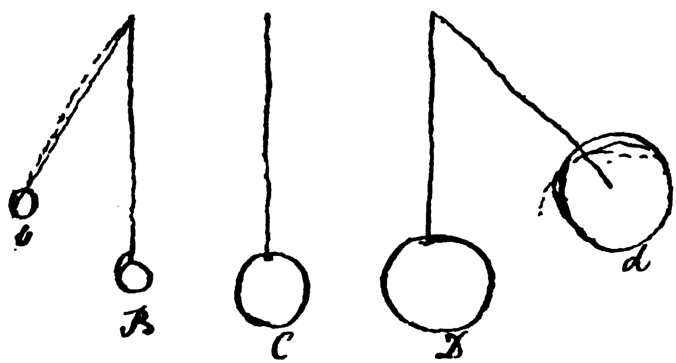


For by the preceding prop. there will be a certain point D which will be at rest immediately after the stroke therefore the velocity communicated to any particle

[1] [It would be simpler to write $x + kB$ and $x - kA$, where k is arbitrary; and so on.]

E will be as DE its distance from that point and the force with which it reacts upon it will be to the force with which another particle of the same size placed at A would react upon it[1] as $DE^2 : DA^2$ therefore the point E reacts with the same force as a point placed at A whose size should be to that of E as DE^2 to DA^2 and whose mechanical momentum would consequently be the same.

Because in the collision of elastick bodies there is very often an encrease of momentum as usually computed some people have thought there might be a perpetual motion made from it, and indeed it at first seems very likely that it might be employed in mechanical performances to encrease the force.



Thus suppose B, C, D to be any number of elastick balls encreasing in size from B to D let the ball B be drawn to any distance b and let fall again it will strike C and then D and the body D will move after the stroke with a much greater momentum than what B acquired by the fall[2]; it should seem therefore that if when D was arrived at its greatest hight d it should be stopt by a catch in such manner that its weight might be employed in moving the engine, that it would move it with more force than what was required to raise the ball B. But on the contrary it appears by the foregoing proposition that if the motion of all the other balls after the stroke could be in like manner applied to move the engine that their effect would be but rarely equal to what was required to raise the ball B.

It appears also from this proposition that whenever 2 nonelastick or imperfectly elastick bodies strike there is a loss of mechanical momentum; upon supposition that the bodies are intirely void of elasticity and strike each other directly the quantity which is lost may thus be calculated. Let the weight of the 2 bodies be A and B and their respective velocities $x + B$ and $x - A$ as before[3]; after the stroke they will move both together with the velocity x and the sum of their mechanical momenta will be $x^2 \times \overline{A + B}$, subtract that from their momenta before the stroke and there will remain

$$A^2B + B^2A = \frac{AB}{A + B} \times \overline{A + B}^2$$

[1] [That is, the acceleration of a particle E is as DE, therefore the *work* absorbed by its kinetic reaction is as DE^2.]

[2] [Because B has now acquired reversed momentum by its rebound.]

[3] [See footnote on previous page.]

equal to the momentum of a body whose weight is equal to the product of the weights of the 2 bodies divided by their sum and whose velocity is equal to that with which the 2 bodies meet.

If one body is supposed to stand still before the stroke the quantity of mechanical momentum lost by the stroke will be to the whole mechanical momentum before the stroke as the weight of the body struck to the sum of the weights of both bodies.

From hence it appears how much force is lost when you drive a nail or wedge &[a] with a hammer unless the weight of the wedge is very small in proportion to the hammer: if the hammer and wedge are of the same weight $\frac{1}{2}$ of the force will be lost.

From hence it follows that if a man holds a board in his hand and a bullet whose weight is small in comparison of that of the board is shot against it so as to pierce through it, the shock which the man will receive will be small in respect of what might be expected from the force requisite to pierce through it. For the same reason though light has a very sensible effect in putting the small particles of bodies in motion id est in causing heat, yet it is only by the nicest experiments that it can be found to have any effect in pushing bodies out of their places.

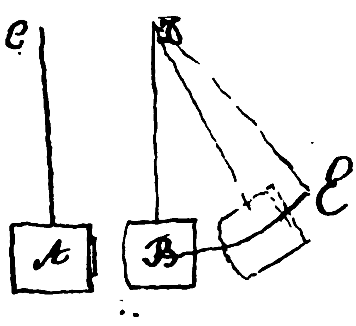
If a bullet moving with a given velocity pierces through the board in a certain time this same bullet moving with 10 times its velocity will pierce through it in less than $\frac{1}{10}$ part of that time; therefore as the force with which the parts of the wood resist being displaced is much the same whether they are struck fast or slow, the momentum communicating to the board in this latter case will be less than $\frac{1}{10}$ of what was communicated to it in the other, and the mechanical momentum less than $\frac{1}{100}$, consequently the mans hand will receive $\frac{1}{100}$ of the shock in this case than it would in the first; for the same reason a bullet moving very briskly will pass through the board without shattering it whereas a bullet which is almost spent will shatter it greatly.

When 2 soft or elastick bodies strike against each other the pressure exerted by one body on the other during the stroke is inversly as the space through which the bodies yield or are compressed by the stroke; therefore if the space through which they are compressed becomes nothing, or which is the same thing if both the bodies are perfectly hard, the force exerted by one body upon the other during the instant of the stroke is infinite. This property I believe was first taken notice of by Borelli.

Therefore in driving a wedge or any such thing by a hammer, if both the hammer and wedge were perfectly hard and the body to which the wedge was to be drove into was perfectly void of elasticity, every stroke would have some effect however small the stroke was; but if either the hammer or wedge yield to above a certain degree it is plain that pressure exerted on the wedge by the stroke of the hammer may be less than what is sufficient to move the wedge; moreover though the stroke be sufficient to move the wedge yet there will still be some force lost in compressing the hammer and wedge before they are overmuch compressed that their resistance shall be sufficient to move the wedge, and as the quantity of mechanical momentum lost this way will be the same whether the stroke is great or small, provided it is not too small as to be unable to move the wedge, there will be less force lost in proportion the greater the stroke is with which the wedge is drove.

This property will also serve to explain an odd experiment upon unannealed glasses related in *Phil. Trans.* No. 477 namely that if you drop into one of those glasses a small piece of flint or diamond or hard temper'd steel though not bigger than a small pea the glasses would brake, though you might drop into it a large piece of iron or lead, wood, or ivory &ª without breaking; unluckily most of the bodies which he dropt into it of those kinds which broke the glasses were rough and sharp such as flints which broke the glasses the easiest of anything, he however says that he dropt a pearl of something less $\frac{1}{12}$ of an inch into a glass which broke it and says that he has often dropt musquet balls into them without breaking. This may pretty easily be accounted for by supposing [un]annealed glass to be extreamly little compressible; thus suppose the degree of compression of unanneald glass or the distance to which it may be compressed by any given force to be as 1 that of the pearl as 2 and lead as 4000, then the pressure exerted on the glass by the pearl is as great as that by a piece of lead of 1300 times its weight which is a greater disproportion than that of the weight of the pearl to the musquet ball, though there is no need of supposing it so great because the musquet ball, by being larger bears upon a greater part of the glass than the pearl; but if the compression of the glass had been 100 instead of 1 then the pressure exerted by the pearl would equal to that by a piece of lead of no more than 40 times its weight. There are some parts of this experiment, particularly the glasses sometimes not breaking till ½ an hour after the stroke, which seem very difficult to account for.

It perhaps may not be impossible to contrive a method upon this principle of finding the degrees in which different bodies are compressible by the same force. Let the body *A* composed of the materials whose compressibility you have a mind to try be suspended as a pendulum by the line *CA* with a thin piece of tin foil or gold leaf stuck against its side, and let another body *B* of the same materials hung by the line *DB* be drawn to any distance from the perpendicular *E* and let fall against it so as to compress the tinfoil; after this has been repeated a sufficient number of times the resistance of the tinfoil will be so much increased that the stroke of the body *B* will not be sufficient to compress it any further, which may easily be seen by observing whether the breadth of it increases; then to compare the compressibility of any other kind of matter with it exchange the bodies *A* and *B* for others of the same size and shape with the former, of the matter required, fix the same piece tinfoil used before to one of them and increase the height from which you let the body fall till you find that it is just able to compress the tinfoil and no more. There are 3 principal causes which disturb the accuracy of this experiment: the 1st is that the tin foil is not perfectly



unelastick, therefore when the resistance of the tinfoil is so great that its thickness cannot be decreased by the stroke it does not follow that it is not at all compressed by the stroke, but only that it restores itself to its former position, consequently the proportion of the compression found by this experiment does not express the real proportion of the compression of the bodies themselves but of the sum of the compressions of the body and tinfoil together—the best way I know of getting over this difficulty is by making the tinfoil so thin that its compression shall be small in respect to that of the bodies: the 2nd is that as the resistance of all bodies increases the more they are compressed we cannot be sure that the law in which it increases is the same in all bodies, but however that will not be of much signification in such an experiment as this; but the most formidable objection I think is this as it is impossible to make the body B constantly to keep exactly the same situation when it strikes A, the inside of it of which strikes A must be made a little convex to avoid striking it with its edge and consequently the part of its surface with which it touches the tinfoil will vary according as the body is more or less compressible; this must be avoided as much as possible either by making the radius of curvature reciprocally as the compression found by experiment, or by observing in how great a space the body and tinfoil touch one another by marking one of them by some colour which will easily come off or by engraving fine strokes in the body and seeing how much of the tinfoil receives the impression; but whatever way you use it can hardly be done with any great exactness.

Fig. 1st Pl. 3rd. Let the body A be attracted by or repelled from the points B, C, D, E, and a severally with forces which are always equal at equal distances however unequal at unequal distances, and let the body A by their united attractions or repulsions be carried to a, take Bb equal to Ba and Cc equal to Ca, Dd equal to Da &ª then the quantity of mechanical momentum which the body A will acquire or lose in falling from A to a will be equal to the sum of the mechanical momenta which it would acquire or lose by falling from A to b by the attraction or repulsion of B singly and from A to c by the attraction or repulsion of C singly and from A to d by the attraction or repulsion of D singly &ª: thus suppose that the body A by falling from A to b by the attraction of B would acquire the mechanical momentum B that in moving from A to c against the repulsion of C it would lose the momentum C and that in moving from A to d against the attraction of D it would lose the momentum D then the mechanical momentum acquired by A in falling through $Aa = B - C - D$.

For assume any 2 points in Aa, a and A infinitely near to each other and assume $B\beta$ equal to BA, and $Bb = B$a and in like manner assume

$$C\kappa = C\mathrm{A} \text{ and } C\mathrm{k} = C\mathrm{a}, \quad D\delta = D\mathrm{A} \quad \text{and} \quad D\mathrm{d} = D\mathrm{a} \ \&^{a}$$

then will the body A supposing it to fall through aA by the attraction of B alone receive the same increase of mechanical momentum as it would by falling through bβ by the same force, by Newt. Prin. Prop. 40th: in like manner it will lose the same quantity of momentum in moving through aA against the repulsion of C as in moving through kκ against the same repulsion, and it will be the same with D, E &ª consequently as the mechanical momentum given to a body by any number of forces acting upon it together is equal to the sum of the momenta which they could give it separately, and as the body will receive the same encrease of Mechanical momentum in falling through aA whatever velocity it enters that space with, it will in falling through aA by the united attractions or repulsions of all the bodies receive an encrease of Mechanical momentum equal to the sum of those it would receive in falling through bβ by the attraction of B through kκ by the repulsion of C &a: therefore the quantity of mechanical momentum acquired in falling through Aa [under] the united attractions or repulsions of all the bodies equals the sum those it would in moving through Ab, Ac, Ad &a by their proper attractions or repulsions singly. Q.E.D.

If any point B instead of being considerd as immoveable is in motion then the encrease of mechanical momentum produced in A by the attraction of B above what it would acquire by the attraction or repulsion of the other bodies added to that produced in B by the same attraction is equal to that produced in a body in falling by the same attraction through the space by which those 2 bodies approach one another.

Therefore if there is a system of bodies A, B, C, D, E &a attracting or repelling each other in the abovemention'd manner compute the mechanical momentum which it could produce in or take away from B in falling from B to A and also the momentum which it could produce in C in falling from C to A as also what it could produce by the same means in all the other bodies D, E &a: in like manner compute the momentum which the attraction of B could produce in all the bodies which it attracts except A (as the momentum produced by their mutual attraction was before computed) compute also the momentum which C could produce in all the bodies it acts upon except A and B and do the same thing by all the other bodies, then the sum of these additional mechanical momenta added to the real momenta[1] with which the bodies are moving will remain constantly the same and will not be alterd by their actions upon one another.

Cor 1st. If any number of perfectly elastick bodies or particles mutually repellent in the manner above described are included in any space and put in motion, the sum of their mechanical momenta though it will not be constantly strictly equal because the sum of those additional momenta which are necessary to be added in order to make it so is not always the same, will yet be nearly so if the number of bodies be very great, but whether the number of bodies be great the sum of their mechanical momenta can never continue either encreasing

[1] [Namely, the *kinetic energy* and the *potential energy*, the latter being here proved to subsist in a field of attractions provided the forces depend only on the distances. Compare the same argument and reservation, expressed more analytically, in Helmholtz's *Erhaltung der Kraft*, 1847. The potential energy of extended springs is explicitly introduced in Cor. 5 *infra*: it is the same as Daniel Bernoulli's *vis viva potentialis* mentioned by Euler, *De Curvis Elasticis*, § 1, 1744. The theorem of *vis viva* was extended to bodies moving under mutual forces of attraction by D. Bernoulli in *Berlin Memoirs*, 1748. In *Hydrodynamica*, Sec. 10 (1738), D. Bernoulli had proposed the air thermometer as the natural standard of "heat"; also he calculates the potential energy of compression of air (at uniform temperature), and speaks of the definite amount of "vis viva quae in carbonum pede cubico latet" being liberated by combustion, and (referring to Amontons) he speaks of its mechanical utilization.]

or diminishing for ever (because the bodies cannot continue either approaching nearer to or receding further from each other for ever) but must sometimes encrease sometimes diminish and their mechanical momenta taken at a medium will remain always the same.

If you suppose them to be elastick bodies there is no need that they should be spherical since the only reason why I supposed the repulsive or attractive force of each body to be equal at equal distances is this, that if I supposed the repulsive or attractive force of the body to be as is represented in fig. 2nd pl. 3rd where A represents the central body and whose central force is supposed equal in each part of the same line BCD, bcd &ª which are driven further distant from each other and from the center at B and b than at the opposite part D then a body moving along $BbecC$ would be more acted upon by the central body in moving from B to e than from e to C. But if you suppose the attractive or repulsive force of the body to be as represented in fig. 3rd Pl. 3rd where D represents the body and where the lines ABC, abc, $\alpha\beta\kappa$ &ª in all parts of which the attraction is supposed of equal strength[1] are drawn in such manner that if you draw lines as $B\beta$ perpendicular to ABC or abc the distances of those lines from one another measured along $B\beta$ shall be everywhere the same, then it will be impossible for a body to come within the action of the body in such a manner but what it must be as much attracted or repelled in descending towards the body as in rising from it and the case will be the same as to preserving the same quantity of mechanical momentum as if the force was everywhere equal at equal distances from the center. In this case as well as that of elastick bodies the bodies will by their actions upon one another acquire a revolving motion[2] but this will make no difference by what has been said before.

The truth of this corollary is greatly confirmed by a preceding passsage where I shewed that the mechanical momentum of 2 perfectly elastick bodies was not alterd by their striking against each other.

Cor 2nd. Heat most likely is the vibrating of the particles of which bodies are composed backwards and forwards amongst themselves; therefore if bodies are composed of particles attracting or repelling one another in the manner above described their heat must remain constantly the same except as far as it is alterd by receiving from or communicating heat to other bodies, and whenever 2 bodies of different heats are mixed together or otherwise placed so that one may receive heat from the other, one will receive as much mechanical momentum or in other words as great an encrease of heat multiplied into its quantity of matter as the other loses so that the sum of their mechanical momenta may remain unalterd. But there is plainly both an encrease and loss

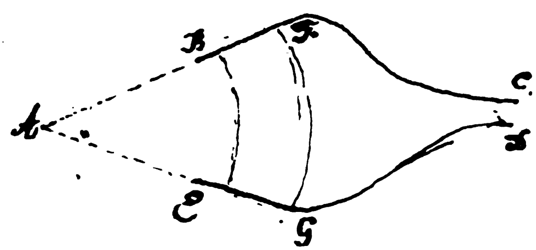
[1] [He is here struggling without success towards the modern idea of curves of constant potential: but the conclusion is correctly established at the top of the preceding page. The figures are on p. 430.]

[2] [Compare in connexion with this and the end of the previous paragraph the modern dynamical theory of gases; but Cavendish is intent mainly on the balance of energy. The nature of pressure and elasticity had been present to Huygens long before. Compare also more especially the theory of heat expounded in the next paragraph, which involves the steady state of internal motion, above deduced, with a definite amount of internal energy, for each temperature or state of "heat."]

of heat without receiving it from or communicating it to other bodies, as appears from the fermentations and dissolutions of various substances in which there is sometimes an encrease sometimes a loss of heat as well as from the burning of bodies in which there is a vast encrease of heat above what can reasoneably be supposed to be produced by the action of emitting light; and as this I think cannot with the least probability be supposed to arise from the attracting or repelling particles approaching nearer or receding further from one another, by which means the sum of those above mentiond additional momenta may be alterd, the particles must either not attract or repel equally at equal distances or must act stronger when placed in some particular situations than others or something else of that nature[1]. One would be apt at first to explain this by supposing them to attract or repel some kind of bodies stronger than others; but then it should seem as if there should always ensue an encrease heat whenever 2 bodies are mixed which mix together with any degree of force whereas there is often produced a great degree of cold thereby as in mixing salt and water. There are other reasons too which seem to shew that this way of explaining it is insufficient.

Cor 3rd. When any number of rays of light strike any body so as to be reflected backwards and forwards within it without ever emerging from it they will communicate their whole momentum to it, but when they are reflected from the body immediately as they will be returned with nearly the same swiftness with which they struck it they will communicate a very small part of their momenta to it; which is the reason why black bodies heat so much faster in the sun than white.

Cor 4th. When a vibrating body causes any pulse or sound in the air supposing it to be a perfectly elastick fluid, as soon as the body has performed a vibration compute the quantity of mechanical momentum communicated to



the air together with the momentum which would be produced by the compressed or rarefyed air restoring itself to its natural situation; that quantity will be neither encreased or diminished as the pulse moves forwards, and if there is any hollow vessel *BCDE* whose length *BC* is greater than the length of

[1] [Alternatively, some source of intrinsic or structural potential energy (e.g. of orbital motions in an electrical molecule) must be drawn upon when chemical changes occur. This well-founded reservation may afford the reason for some remarks of Cavendish in other connexions, which have been interpreted as leaning towards the caloric theory. See also footnote, p. 408.

It is noteworthy that a very significant aphorism of Newton, ***calor est agitatio partium quaquaversum***, heat is uncoordinated internal motion, occurs abruptly in a chemical memorandum in Latin, printed in Horsley's Edition, vol. iv, pp. 397–400.]

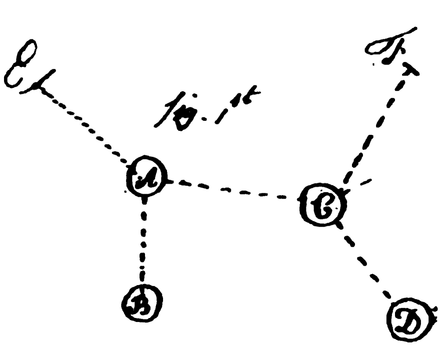
a pulse with its large end *BE* open and turn'd towards the sound as soon as the pulse is intirely within the vessel or which is the same thing when the hindermost part of the pulse is within the line *BE* compute as before the quantity of momentum of the air within the vessel; that quantity will not be alterd as the pulse proceeds on towards the narrower part of the vessel therefore the velocity of the vibrating fluid together with the compression and rarefaction of the air and the length of the pulse will be greater in the narrow part of the vessel than the other. This is the reason why deaf people hear better by applying those kind of funnels which they sometimes make use of to their ears.

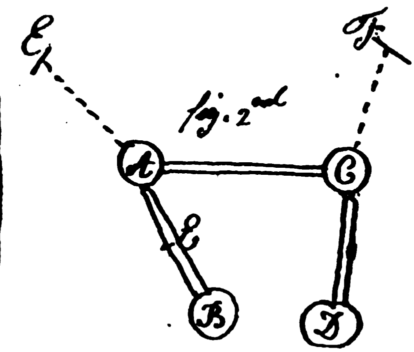
[1][For like manner when the tide runs up any branch arm of the sea which is broader and deeper at the mouth than further in, the quantity of momentum of the water within the arm encreased by that which might be produced by all the particles of water which are raised above their natural level falling down to their level will remain still the same as the tide proceeds to the narrower part of the arm, and consequently the height to which the tide rises together with the velocity of the water and perhaps length of the tide or pulse will be encreased, supposing the water to be perfectly fluid or to yield perfectly easily to any motion impressed upon it and to be intirely void of cohesion and friction, for in that case as no force could be lost friction or the impinging of one body against the other the case would hold equally good as in a perfectly elastick fluid.

N.B. I here consider the motion of tides as exactly analogous to the pulses of air in sound.

Any kind of waves or such like motions either in air or water would upon the same supposing the fluid as before to be void of friction &ª would continue their motions in it for ever.]

Cor. 5th, fig. 1st. Suppose any number of bodies as *A, B, C, D* &ª connected together in any manner by perfectly elastick springs and suspended from one or more fixed points as *E, F* &ª to be put in motion, the sum of their me-





chanical momenta added to the momenta which might be produced by the restoring of the springs to any given degree of tension and the falling of the bodies to their proper level will remain constantly the same.

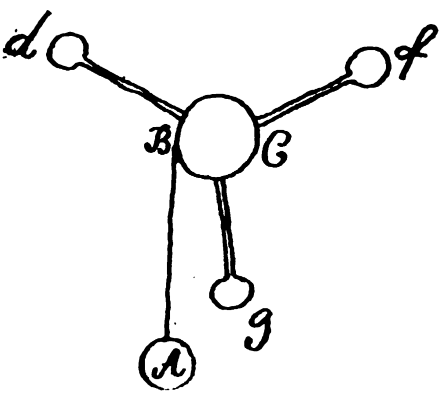
Fig. 2nd. The same thing will hold good if the bodies instead of being connected

[1] [The paragraphs within brackets are marked to be deleted in the manuscript.]

by springs are connected by solid inflexible rods only made so that the joints shall be intirely void of friction or shaking and that the parts of the engine are so constructed that they cannot impinge against one another; for suppose that the rod AB instead of being absolutely inflexible were perfectly elastick but of such a nature that it would require an infinite force to compress or bend it, in that case it would certainly hold good, but as long as this rod is acted upon by only finite powers it is of no signification whether it is inflexible or of such an elasticity here mention'd; but if 2 of these rods were to strike against each other in which case the force will be infinite then there will be a total difference between the 2 cases and there will be a great loss of force if the rod is incompressible; and it is plain from the nature of the lever and all mechanical powers and from what was said concerning bodies striking each other obliquely that the parts of the rod AB not being acted on immediately by the springs but by the mediation of the lever AB can make no alteration.

And in general we may conclude that whenever any system of bodies is in motion in such a manner that there can be no force lost by friction imperfect elasticity or the impinging of unelastick bodies, that then the sum of the mechanical momenta of the moving bodies added to the sum of the abovementioned additional momenta will remain constantly the same[1].

Fig. 1st Plate. 1st.



[1] [This surely is the earliest precise enunciation of the principle of the conservation of energy, kinetic and potential, including enumeration of the causes that lead to its degradation, which on the principles of Cor. 2 would be into heat of precisely equivalent amount.]

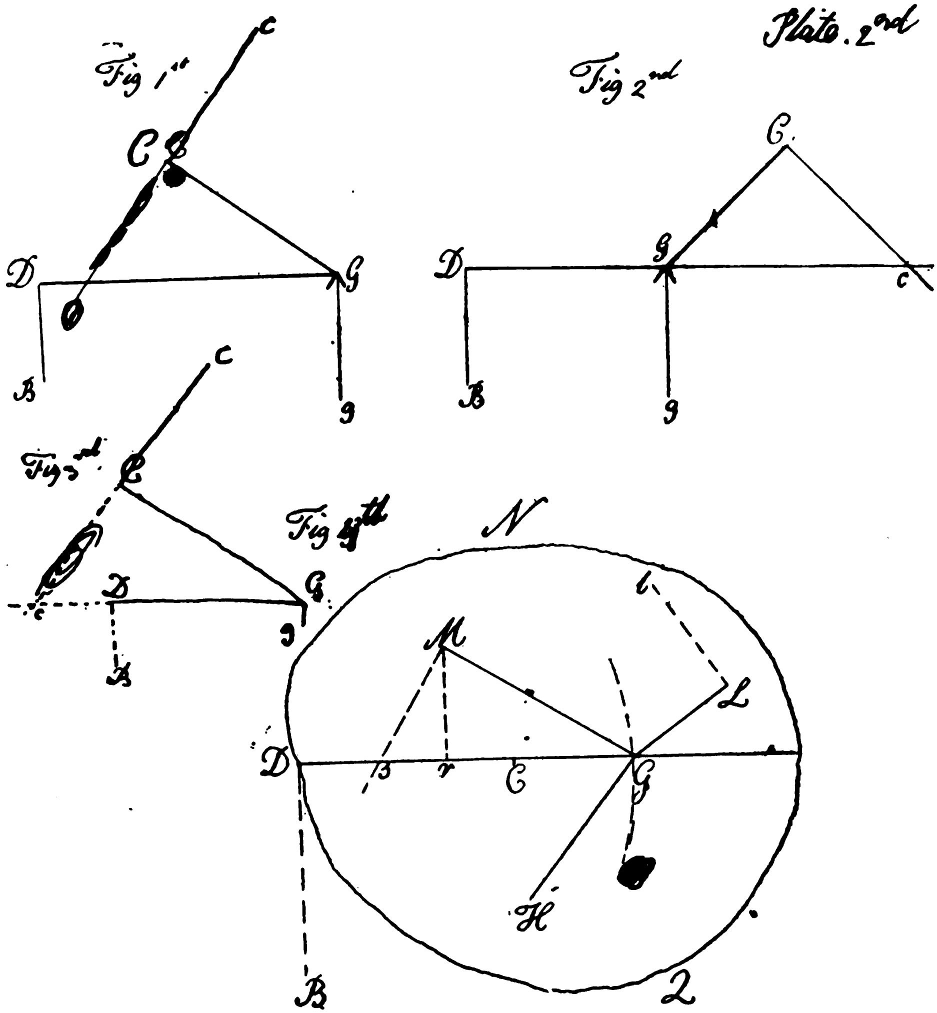


Plate. 2nd
Fig 1st
Fig 2nd
Fig 3rd
Fig 4th
C
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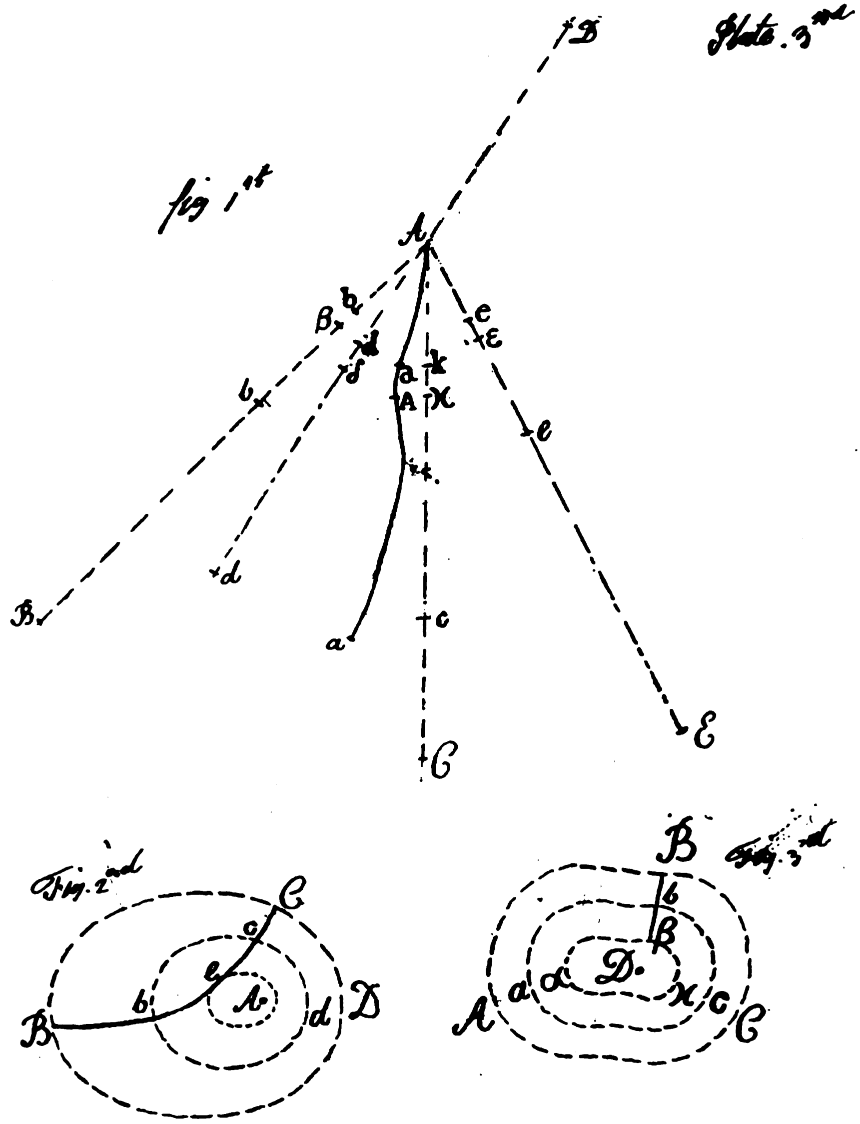
Plate 3rd
fig 1st
Fig. 2nd
Fig. 3rd

CAVENDISH AS A GEOLOGIST

CAVENDISH throughout the course of his long life rarely left London or thought to give himself a holiday. His habits were so fixed and regular that any interruption to the routine of his daily existence was irksome to him and was resented with an almost peevish impatience. Whatever rest he needed he seemed to find in change of occupation. The charms of the country had apparently few attractions for him. But at one period, viz. from 1785 to 1793, he was led to take up the study of Geology, then in its infancy as a science, possibly through the influence of his friend the Rev. John Michell, who occasionally dined with him at the Royal Society Club, and in whose conversation and correspondence he took great interest. In the summer and autumn of 1785, 1786, 1787 and 1793 he made a series of driving tours through portions of central and southern England and Wales in company with Sir Charles Blagden, at one time Secretary of the Royal Society, partly with a view of observing their geological and mineralogical features, and partly to familiarise himself with technical and manufacturing operations depending upon the applications of chemistry and physics. Accounts of these journeys are to be found among the Chatsworth Manuscripts. They are written in part by Cavendish himself, in part, apparently, by Blagden or by an amanuensis. As narratives of travel they have no particular value, nor do they afford any special information concerning such manufacturing processes as he was able to witness, as, for example, dyeing in the west of England, iron manufacture and mechanical engineering at Birmingham and its vicinity when he visited Watt, alum manufacture at Whitby, etc. He nowhere expressed any feeling for natural scenery and he seemed wholly unaffected by it. But, as may be surmised, whatever could be quantitatively ascertained was religiously noted. Thus he regularly observed by means of a way-wiser the distance travelled each day. He constantly read the height of the barometer as indicating the rise and fall of the roads he traversed. He observed the angles which distant hills and ridges subtended and noted the temperatures of the deep wells he came across.

Such scientific interest as these itineraries may possess is mainly confined to the geological observations they record, which are summarised in a special paper in Cavendish's handwriting. The manuscripts were accordingly submitted to Sir Archibald Geikie who was so good as to look through them and to contribute the following statement of his impressions of Cavendish's merits as a geologist.

NOTE ON CAVENDISH AS A GEOLOGIST

By Sir Archibald Geikie, O.M., K.C.B., F.R.S.

Cavendish evidently had a keen interest in tracing out the distribution of some of the more conspicuous geological formations across central and southern England. But he identified them, as he confessed, only by their superficial characters. These characters however are often deceptive. One of them on which he seems to have laid considerable stress was their colour. When he found a *yellow* limestone he concluded that it lay immediately below the Clay [Gault] which underlies the Chalk. Michell pointed out to him that another yellow limestone, below the "Lyas," was extensively developed from Leicestershire to Yorkshire and further north. Cavendish accepted this statement, calling Michell's formation the *Ancient Yellow Limestone*. There can be no doubt that this was what we know as the Magnesian Limestone which overlies the Coal-measures, as Michell pointed out.

Cavendish recognised that a succession of formations could be traced from the Chalk downwards through Clay (Gault) then Sands (Greensand) to his "yellow limestone" which seems to have been any portion of the calcareous zones in the Oolitic series, below which he saw that the "Lyas" lies. He seems to have formed a general notion of the distribution and trend of these formations in the wide extent of country which he had traversed. He perceived that they are less inclined to the horizon than the older rocks which lie to the west of them, and he correctly inferred that "for the most part the farther we go to the W. or N.W., the lower the strata we meet with."

He gives a curious proof that, as he confesses, "a very imperfect knowledge of the strata is to be acquired by merely looking at the surface, as is my case." He took the clay which lies below the gravels of London to be the same as that which lies below the Chalk, and he supposed that the Chalk has been entirely washed away from the London plain. He did not know that this thick formation lies still intact *below* the London Clay, protected from denudation by the mass of overlying Tertiary deposits.

Cavendish entirely missed the meaning of the Derbyshire Toadstone, preferring Michell's idea that it is "clay which was heated in its place," instead of Whitehurst's, who recognised the volcanic origin of the rock.

It is curious to reflect that while Cavendish was perambulating England in geological excursions, William Smith was busy with those observations and inferences among the very same rocks which gave him the true key to the stratigraphical succession, and laid a sound foundation for Stratigraphical Geology.

I do not think that Cavendish's paper should be published. It is of no geological importance, and it would add absolutely nothing of any consequence to his scientific renown.

CAVENDISH'S ASTRONOMICAL MANUSCRIPTS

AMONG the Cavendish Manuscripts preserved at Chatsworth are a number of disconnected papers relating to Astronomical Subjects. Many of them are simply notes or memoranda on matters of passing interest, or connected with the work of Committees of the Royal Society. Others are concerned with the mathematical treatment of observations. A few have been written out in detail as if for preservation or future reference. The great majority are without titles, and in many cases it is very difficult, if not impossible, to discover their meaning. Sir Frank Dyson was so good as to undertake to examine them. He writes as follows: "I have been through the Cavendish papers....They were extremely interesting, and one cannot help regretting that he did not publish more....I have indicated what they are about and made a few extracts."

ON CAVENDISH'S ASTRONOMICAL PAPERS

By SIR FRANK WATSON DYSON, LL.D., F.R.S.,
Astronomer Royal.

These papers, many of them very scrappy and not intended for publication, show the wide range of Cavendish's interests in all subjects connected with Astronomy. This is indicated by the titles:

The Moon's Atmosphere.
Table of Tides at various places.
On the Moon's parallax.
The Attraction of an Elliptic Wedge.
On the light of the full moon.
Alteration of shape in planets' orbits due to resisting Medium.
Method of adjusting the equatorial sector.
Herschel's planet.

In the last paper he concludes "On the supposition of a $\left\{\begin{matrix}\text{parabolic}\\ \text{circular}\end{matrix}\right\}$ orbit its distance from the sun during the time of its having been seen is $\left\{\begin{matrix}20\\ 19\end{matrix}\right\}$ times the distance of the Sun."

In another paper he deals with the difficulty caused by the custom of using the true equinox, and recommends the use of the Mean Equinox (i.e. corrected for nutation). The complete change of astronomical practice in this matter was made later by Bessel and Airy.

Evidently Cavendish was in close touch with Maskelyne, and two of Maskelyne's letters are found in this packet of papers.

Among a number of observations at different places to determine the height of Blanchard's balloon are a series made by Cavendish at Greenwich under the heading "Observations of the Altitude of Blanchard's balloon 16 Oct., 1784 with Bird's Astronomical Quadr. at the Royal Observatory. 4 ft. radius."

He was very much interested in the transit of Venus, and there are computations and correspondence as to the best places for its observation. Among the places mentioned in the letter to Dr Morton, Wardhus was occupied by Father Hell, and Tahiti by Captain Cook. In connection with the transit he considered the effect which would be produced by an atmosphere on Venus, and foresaw to some extent the difficulties which might arise in the observations. Incidentally he made some experiments on the *Minimum visible* under different conditions. He found 45″ for a small notch and 3″ for the diameter of a wire seen against skylight.

Cavendish maintained for a considerable time an interest in the difficult problem of the determination of the orbit of a comet from three observations. Among the papers there is a list of all the Comets whose orbits had been computed, and there are a number of scrappy papers of computation. These are undated. A more finished essay, apparently intended for publication, was sent to Maskelyne, who, in his reply dated April 16, 1788, suggests that Cavendish should compute the orbit of the Comet discovered in that year by Miss Herschel, and supplies observations. There is another letter of Maskelyne's to Cavendish dated Oct. 9, 1799, in which he says that Sir Henry Englefield to whom he had communicated observations of the Comet of 1799 at the same time as to Cavendish, had forwarded him a preliminary orbit. "I send these to save you unnecessary trouble or that you may direct it with more advantage by commencing with the rough elements here given."

The orbit of the Comet of 1799 is "computed by the table of Boscovich's sagitta." Besides using this Newtonian method he also made the computation by "a fluxional process." There are a few short notes on Laplace's method. At the conclusion of one of these notes he remarks

For La Place's method we only find whether the supposition agrees with observation in one respect, but the greatest fault is that if the angle subtended by the Earth and Sun at the Comet at either observation is nearly right, a small alteration in the radius vector makes a great error in the heliocentric place and therefore a small error in that observation will make a great alteration in the parabola.

None of this work appears to have been published, and no reference is made to Cavendish in Sir Henry Englefield's book (1793) which gives an account of Boscovich's and Laplace's methods, or in Ivory's paper in the *Phil. Trans.* 1814.

There are short notes on planetary perturbations of Comets, which probably had their origin in discussions about the near approach of Lexell's Comet to Jupiter in 1779. One of these is drawn up in the form of precepts for a computer and is entitled "Written for person thought of for calculating perturbations of expected Comet."

I have made four short extracts from the papers:

(1) Letter to Dr Morton.

(2) The last page of a paper on the Precession of the Equinoxes.

(3) A part of a very short note on the influence of the tides on the earth's rotation.

(4) An isolated scrap on the bending of a ray of light by gravitation, which is of interest, as the possibility of the bending of a ray of light by a gravitational field is at present engaging attention, though Cavendish was working on a corpuscular theory. This may have been suggested by Query 1 of Newton's *Opticks* "Do not Bodies act upon Light at a distance, and by their action bend its Rays, and is not this action (*coeteris paribus*) strongest at the least distance?"

(1) *Letter to Dr Morton.*

The best way of finding the parallax of the sun from the transit of Venus in 1769 is by a comparison of the duration of the transit in different places for which purpose it should be observed in such places where the difference of duration is the greatest. The duration is greatest about Tornea and Wardhus in Lapland. It is to be hoped that Swedes and Danes will send observers to these places. The place where the duration is least is in some of the islands supposed to be in the South Sea to the South of the Equator, and which would consequently be the best place to compare with Tornea and Wardhus. If observers could be sent there, which I imagine there is no probability of, the next best places are Cape Corrientes in Mexico, where the duration is above 16 minutes less than at Tornea, and California, where the duration is from 16 to 15 minutes less than at Tornea. The Royal Society proposes to send observers to California which is a better place than Cape Corrientes, as at this latter place the transit will end so little before sun set that there is great danger of the sun being hid in clouds.

It is very desirable that the transit should be observed also in some place where the duration is of an intermediate length between that at California and the North as it would serve as a check in case of error in either of other observations and besides that would be particularly useful in case the observation should fail at either of the other places. I know of no place so proper for this purpose as Kamptschatka. In the southern part of the peninsula of Kamptschatka the duration is near 8 minutes less than at Tornea and rather more than 8 minutes greater than at the south point at California, so that the difference is sufficient to deduce the parallax very well by comparing the duration at Kamptschatka with that at either of the foregoing places, supposing the observation to fail at one of them; I know of no other place proper for this purpose except the north western parts of Hudson Bay, where I believe it would be extremely difficult to send observers as ships can hardly get there early enough in the summer to land observers there before the transit, and it would be hardly practicable for observers to winter there. Besides that, if it was practicable to send observers there, it would not be so proper a place as Kamptschatka as the sun is much lower and consequently there is more danger of its being hid in clouds.

(2) *On Precession of the Equinoxes.*

The French Academicians by the mensuration of a degree determine the difference of the axes to be $\frac{1}{178}$ of the whole, but it has been demonstrated that if the earth be supposed to consist of spheroidical strata and is of the same density in each part of the same stratum, however different in different strata, or however different the degree of ellipticity of those strata, that then the difference of the 2 axes divided by the axis + the difference of gravity at the equator and pole divided by gravity = $\frac{5}{2}$ of the centrifugal force at the equator divided by gravity; the Academicians have also determined the difference of gravity to be $\frac{1}{180}$, whence the difference of the axes should come out $\frac{1}{318}$, therefore, as the difference of axes as observed by the Academy can not take place without assuming some very improbable hypothesis of the density of the earth, or by denying the theory (which seems too well founded to be shaken by this) and as the different measures of degrees agree so little with one another, as well as because the irregularity of the surface of the earth (particularly in the high mountains of Peru) may cause an alteration in the direction of gravity, and by that means disturb the accuracy of the experiment, I think we may fairly reject this mensuration and assume that difference of axes which agrees with the difference of gravity or $\frac{1}{318}$; this, if you suppose the annual precession caused by the sun to be 15″·4, answers very well, but if you suppose it to be 12″ or 8″·7 it may be reconciled with experiment by supposing that the degree of ellipticity of the spheroidical strata diminish as they approach nearer to the center. Thus, if you suppose the earth to consist of a spherical nucleus of an uniform density covered with a spheroidical shell of less density, and if you suppose the diameter of the nucleus to be $\frac{10}{11}$ of the diameter of the earth and its density to that of the outer shell as 13 : 9, the difference of axes of the earth and difference of gravity ought to be the same as I have here supposed and the precession caused by the sun would be 12″ or the mean quantity, but I know no way of accounting for a precession 8″·7 unless you suppose the density of the earth at first to increase and then diminish again as you approach towards the center; if you will grant this, you may account both for the difference of axes and precession by supposing the earth to consist of an hollow sphere whose outer diameter is $\frac{18}{20}$ and inner diameter $\frac{1}{10}$ of that of the earth and filled with a matter of less density and covered with a spheroidical shell and that the density of the outer shell hollow sphere and inclosed matter are to each other in the proportion of 1, 6 and $\frac{42}{100}$.

(3) *On the diminution of the diurnal motion of the earth in consequence of the tides.*

If there was no loss of force by friction the sum of the *vis viva* of the ☽ in its orbit round ⊕, the rotatory *vis-viva* of ⊕ considered as one Mass (or the visible) and the *vis viva* of the Water in respect of ⊕ (or the invisible *vis viva*) should remain unaltered except by the attracting parts approaching nearer. But if the invisible *vis viva* is diminished by friction, and the loss is continually supplied by the attract. ☽ then the sum of the *vis viva* of ☽ and of the visible *vis viva* of ⊕ must be as much diminished as to compensate that. But the only way by which the loss of invisible *vis viva* can be compensated is by the water being at a medium raised higher on that side of ⊕ which has left ☽ than on the other, and this will diminish the visible *vis viva* of ⊕ and increase that of ☽, and the increase of *vis viva* of ☽ is to the diminution of visible *vis viva* of ⊕ directly as their angular velocities, or as 1 : 13, and conseq. the diminution of visible *vis viva* of ⊕ is to the diminution of the invisible *vis viva* by friction as 13 to 12, and therefore may be considered as equal. It must be observed, however, that this increase of *vis viva* of ☽ will increase its distance from ⊕, and therefore will actually diminish its *vis viva*, but this does not affect the justness of the foregoing conclusion.

(4) *To find the bending of a ray of light which passes near the surface of any body by the attraction of that body.*

Let s be the centre of body and a a point of surface. Let the velocity of body revolving in a circle at a distance as from the body be to the velocity of light as $1 : u$, then will the sine of half bending of the ray be equal to $\frac{1}{1+u^2}$.
[This deflection is half the amount given by Einstein's law of gravitation.]

CAVENDISH'S MAGNETIC WORK

CAVENDISH'S interest in Magnetism, and especially Terrestrial Magnetism, would appear to have originated through his association, as an investigator, with his father. Lord Charles Cavendish possessed instruments for observing magnetic declination and dip and seems to have occupied himself in making systematic observations in the garden of his house at Great Marlborough Street in which he was assisted by his son. It was probably at their instigation that the Royal Society included measurements of these elements in the scheme of meteorological observations which they instituted at their house in Crane Court, on which Cavendish, by desire of the Society, reported in 1766 (see p. 112).

The Chatsworth Manuscripts contain a considerable number of papers by Cavendish on Magnetism and related subjects. These have been very carefully examined by Dr Charles Chree, F.R.S., the Superintendent of the Kew Observatory, who writes as follows concerning them.

ON THE CAVENDISH MSS. RELATING TO MAGNETISM AND ASSOCIATED SUBJECTS

By CHARLES CHREE, Sc.D., LL.D., F.R.S.,
Superintendent of the Kew Observatory.

The Cavendish MSS. relating to magnetism consist of (1) a bundle of small octavo pages about $6\frac{1}{2}'' \times 4''$, usually two leaves, sometimes only one. Two leaves are usually paged as four pages. The pages run from 2 to 256, but numbers 167, 168, 209, 210, 223, 224, 225, 240, 241, 242, 243, 254, 255 and 256 each include two pages. There is an additional leaf 200 A, numbered on one side only. These all relate to declination observations. The observations on pp. 2—10 seem experimental. The year to which they refer is not given. On pp. 2, 3, 4 there are no dates, only the days of the week. On pp. 5—10 there are dates ranging from July 3 to December 30, but no year. Pp. 11, 12, 13 relating to observations in May and June 1782 seem similarly preliminary. The observations recorded on pp. 14 *et seq.* are mostly systematic and cover declination observations taken at Hampstead and Clapham between 1782 and 1809. The usual particulars given are the date, the hours of observation, the readings of the two ends of the needle and their mean. In 1805 and subsequent years, as a rule one reading only is given with a correction. Also some explanation is given, especially in p. 200 A, of how the corrections were arrived at. In addition to the actual declination observations, there are for each year the calculations from which the several mean declinations were derived.

Six pages (unnumbered) of the same size as above, and two slips summarise the mean declinations from 1782 to 1803. The pages and slips to a considerable extent are duplicates of one another.

The above form the basis of the tables giving declinations, mean values, ranges, etc. at Hampstead and Clapham.

A separate group of pages of similar size numbered 1 to 23, having subject "Effect of heat on magnets" indicated on one page. The results have been summarized.

(2) A bundle of small octavo pages about $6\frac{1}{2}'' \times 4''$ marked "Horizontal needle" on outside page. These are variously paged and include dip as well as declination observations. The first part, numbered pp. 1 to 61, deals with declination and dip observations made in 1773, 1774 and 1775. A good many of the observations are experimental. Others compare different magnets or aim at determining errors due to peculiarities of instruments, presence of magnetic matter, etc. A good many refer to observations made at the Royal Society House, or with the Royal Society needles. The observational data referring to the Royal Society House have been set out subsequently.

(3) There follow pages numbered 1 to 6, the first headed "Trial of dipping needles for R.S.," and five unnumbered leaves with a scrap of paper relating apparently to needles by Ramsden and Nairne.

Then pages numbered 1 to 4, the first headed "Sissons dipping needle."

Then four leaves describing some dipping needle.

Then four leaves unnumbered headed "springing of needles" giving some values of Young's modulus. These have been made use of in what follows.

Then four leaves, two blank, headed "Trial of long bars by compass."

Then eleven leaves, some blank, numbered 1 to 15, describing results of experiments on strength of variously shaped needles which are subsequently dealt with.

Then a series of pages numbered 1 to 58 dealing with dip observations. Some of the observations seem experimental. There is a synopsis of the experiments recorded on pp. 11—16, but their object is not explained. On pp. 17, 18, 19, 25, 26, 27, 28, 33, 36, 37, 38, 39, 40, 41, 42, 43 there are results of dip observations taken several times a day throughout a number of months, presumably during several years of which 1775 was certainly one (cf. p. 39). [There seems no attempt to deduce from them the nature of the diurnal variation, but it is difficult to imagine what other purpose Cavendish can have had in view. Presumably when he started observing the probable extent of the diurnal range was unknown to him *v. infra.*] Pp. 45—54 deal with dip observations made in August 1778 in London and various places in England, the results of which have been reproduced. Pp. 55—58 deal with dip observations made in 1791 which have also been utilised. In the same bundle are two sheets with dip results obtained with Nairne's needle inside a house, and also with its plane out of the magnetic meridian. There follow eight pages, some of them blank, giving some declination results. The place of observation is not stated. One page containing a summary of results for 1788, 1789, 1790 is headed "Gilpin's observing needle." The declinations it gives are fully 10′ in excess of those obtained in the same years at Clapham. A second set of data for 1789 give lower values for the declination than those at Clapham. At the end of the packet are a few pages having apparently nothing to do with magnetism. One contains some information as to radii of gyration; a second refers to an elasticity experiment in which a modulus of elasticity was derived for crown glass; a third refers to some astronomical calculation.

There are finally a bundle of miscellaneous papers with sheets of various sizes. These comprise: a paper, paged 1 to 13, entitled "Bending of tapering needle by its weight." This includes the mathematical solution of the problem and some numerical results which are referred to subsequently.

A MS. of two sheets containing declination results obtained in Cecil Street and Pall Mall between 1759 and 1775 (*v. infra*).

Three sheets dealing with declination and dip results obtained in June and July 1776 and 1778 at the Royal Society's House (see p. 465 *et seq.*).

One sheet dealing apparently with error in (Royal Society?) compass in 1777.

One sheet and small scrap of paper dealing with error of (Royal Society's?) compass in 1779 and 1780.

A sheet giving some declination results in Pall Mall in May 1787.

A sheet giving results of hourly declination observations with "Mr Gilpin's observing needle" in June 1788 (see p. 472 *et seq.*).

A sheet summarising declination results in Pall Mall from 1782 to 1791.

A sheet containing apparently the results of half hourly observations of declination from 6 a.m. to 7 p.m. on Oct. 24th (?) of an unspecified year, at an unspecified place. The range shown is 12′.

A sheet giving some comparative declination results for Clapham and Pall Mall, and some yearly means at Hampstead and Clapham.

A sheet giving "Variation at Royal Society" in June, July and September 1802 and 1803.

Two sheets (six pages) describing some experiments with different suspensions in a declination needle, referring apparently to effects of moisture.

Eight pages (three blank) apparently notes from some work on Terrestrial Magnetism, including dip results obtained at a number of places in England in 1720.

Sheet referring to old declination observations, copied apparently from Gellibrand's work on Magnetism.

Sheet containing some unexplained data headed 1774 and 1775.

Large sheet folded in four dealing with some unexplained experiments apparently with dip needles near a disturbing magnet in 1778 and 1779.

Sheet folded in two giving results of deflection experiments made in 1776 with a poker and the cast iron "cheeks" of a stove.

Sheet folded in two giving results of deflection experiments made in 1776 with an iron and a steel bar supplied by Elwell.

Three large sheets folded in four dealing with deflection experiments made with blistered steel, cast iron and forged iron bars supplied by Elwell (see p. 444).

Sheet with some unexplained diagrams of parallelograms having apparently something to do with prospects obstructed and not obstructed by trees at some unspecified place.

One sheet folded in two, one side being the solution of a problem in spherical trigonometry, the other side inscribed "Computation how great the dip must be that the error caused by moving needle a given distance from magnet(ic) meridian shall be a maximum and how great the error is in that case."

Ten large and ten small pages (some blank) giving calculations in spherical trigonometry. One page is inscribed "Examination whether direction of horizontal needle (i.e. declination needle) is in the small circle passing through the two points of surface in which dip = 90°, the two magnetic poles being supposed to be at a great and equal depth below surface and the distance of the two above mentioned points being near 180°."

MS. eight large pages inscribed "The method of balancing the needle after it is made magnetical." This refers to dip needles furnished with balancing screws.

MS. paged 1 to 5 inscribed "To find the true dip from observations made in the four different ways when the difference between those ways is considerable." This refers to the case when there are considerable differences between the dips obtained with the circle facing east and west and with the two ends dipping.

Four pages, three blank, inscribed "On the different forms of constructing a dipping needle," refers to cases where the axle rolls on horizontal planes and on friction wheels.

Four pages dealing with effect of ship's iron on compass (see p. 463).

MS. paged 1 to 26, and sheet with two figures, inscribed "On the different construction of dipping needles." This deals with various sources of error including some numerical results for bending of different shaped needles (see p. 453).

A single sheet, folded in two, inscribed "For Captain Pickersgill."

A MS. paged 1 to 7 inscribed "For Cook and Bayley."

A MS. paged 1 to 17 inscribed "Directions for using the dipping needle for Dalrymple."

These three MSS. contain instructions to travellers and are dealt with subsequently (see p. 462).

Three scraps of paper unintelligible by themselves.

The substance of these papers may be conveniently arranged and dealt with as follows:

§ 1. Introduction.

§ 2. Experiments on "fixed" (permanent) and "moveable" (temporary) magnetism.

§ 3. Effect of Heat on Magnets.

§ 4. Strengths of Magnets of various cross-sections.

§ 5. "Springing" (Elastic Bending) of Needles.

§ 6. Errors in Observed Dip due to bending of Dip Needles.

§ 7. Sources of error in Dip Observations.

§ 8. Instructions to Observers and General Notes.

§ 9. Dip Observations.

§ 10. Diurnal Variation of Dip.

§ 11. Declination Observations.

§ 12. Secular Change of Declination.

§ 13. Diurnal Variation of Declination.

§ 14. Disturbed Days.

Introduction

§ 1. To facilitate the comprehension of the experimental work on magnets which Cavendish executed, it is desirable to consider first how he measured the strength of magnets.

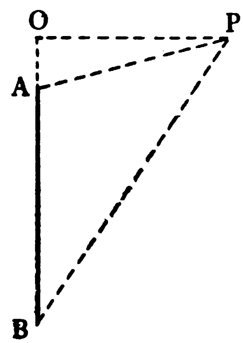

Fig. 1.

Suppose A and B to represent the poles of a magnet in a vertical position, and P a point at a horizontal distance $PO = d$ from the vertical line BA. Let $+\mu$ and $-\mu$ represent the equal strengths of the two poles. Let l denote the length AB, and let ψ_1 and ψ_2 denote the angles APO and BPO. If $m \equiv \mu l$ be the magnetic moment of the magnet, it is easily proved that the horizontal component F of the resultant magnetic force at P is directed along OP and is given by

$$F = (m/ld^2)(\cos^3\psi_1 - \cos^3\psi_2) \text{ ....(1).}$$

Suppose now that a small compass needle NS, capable of motion only in a horizontal plane, has its centre at the point P, and that Fig. 2 represents this needle as deflected out of the magnetic meridian MM'. We may suppose the needle so short compared with the horizontal distance d of its centre from the deflecting magnet that the magnetic force may be regarded as the same at the positions occupied by the two poles N and S. Suppose this force F to make an angle α with the perpendicular to the magnetic meridian, and ϕ to be the inclination of the deflected needle NS to the magnetic meridian. Then we obviously have

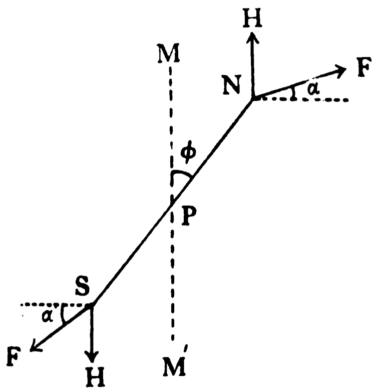

Fig. 2.

$$F \cos \alpha = (H + F \sin \alpha) \tan \phi.$$

If α be small, and ϕ not too large, a close approximation is

$$F = H \tan \phi \quad \text{..............................(2).}$$

If the same magnet in different magnetic conditions—or a series of magnets in succession, identical in pole distance—occupies the fixed position AB, we get for the magnetic moment by combining (1) and (2)

$$m = CH \tan \phi \quad \text{..............................(3),}$$

where C is a constant determined by the values of l and d and the consequent values of ψ_1 and ψ_2.

For very exact work (2) would have to be replaced by a more complicated expression which allowed for the finite length of the deflected compass needle, and allowance would have to be made for variations in H during the time of the observations. There were no magnetographs in Cavendish's time, and he naturally was obliged to treat H as a constant. Also, instead of taking changes in m as measured by changes in $\tan \phi$, he regarded them as measured by the changes in ϕ itself. As ϕ was not in all cases small, this cannot be regarded as altogether satisfactory. Still the results suffice to give a good general idea of the nature of the phenomena, and this seems to have been all Cavendish really aimed at.

The objects he seemed to have had principally in view were to ascertain what kinds of iron or steel were most suitable for temporary and permanent magnets; how magnets were affected by changes of temperature; and what shapes should be given to magnets to secure high strength for a minimum of weight.

In his experiments Cavendish seems invariably to have got one end of the deflecting magnet—either the upper or the lower end—level with the auxiliary compass needle. He may have supposed the poles of bar magnets to be situated at the extreme ends, but there is nothing to show that he did. The position is obviously one which could be recovered with considerable accuracy without any elaborate machinery.

EXPERIMENTS ON "FIXED" (PERMANENT) AND "MOVEABLE" (TEMPORARY) MAGNETISM

§ 2. The first experiments we shall consider were made in 1776. They seem to have been led up to by some experiments, to which there are references in several notes, which employed various iron objects that happened to be at hand, including part of a stove, a poker and an iron bar from an electrical machine. After this preliminary stage, to quote Cavendish's own words,

> Some bars were got from Elwell 31¾ inch long, 2·1 (inch) broad and about 0·5 (inch) thick. On May 29, 1776, one of each (kind) was made magnetical, the marked end being the South pole. In trying the experiment the bars were placed perpendicularly (i.e. in the vertical position) against a wall 25 inches distant from the centre of the (auxiliary compass) needle, 91°¼ to west of usual magnetic north, either the top or bottom of the bar being always on a level with the needle. They were kept constantly with the marked end upwards till after the observation of June 30, after which they were kept with the mark downwards.

These remarks apply to four only of the Elwell bars, including an unnumbered "blistered steel" bar, and three others, all called No. 1, distinguished as "blistered steel hammered," "cast iron" and "forged iron" respectively. It is obvious that what the statement means is that, except for the short time when experiments were in actual operation, the magnets up to June 30 were kept with the south (marked) pole upwards, and so in the position in which the earth's vertical field tended to strengthen them; but after June 30 they were kept south-pole down, and so with the earth's field tending to demagnetise them.

The plan of the experiments was as follows:

With the marked, i.e. south, pole up, the lower or north pole was brought level with the compass needle, the deflection of whose north pole ϕ_1 to the east of the magnetic meridian was noted; then, still with south pole uppermost, the upper or south pole was brought level with the compass and the deflection ϕ_2 to west of magnetic north was noted. The algebraic difference of the two angles, i.e. $\phi_1 + \phi_2$ taken numerically, was called A. The magnet was then inverted so that the marked pole was downmost, and readings were again taken of the compass needle, ϕ_3, counted + to east, when the lower, i.e. marked pole, was level with the compass, and ϕ_4, counted + to west, when the upper pole was level with the compass. The numerical sum $\phi_3 + \phi_4$ of these two angles was called B. To see the significance of these angles, let m_p represent the magnetic moment arising from the permanent, or as Cavendish called it the "fixed" magnetism, and m_t the magnetic moment arising from the temporary magnetism, induced by the earth's vertical field, or as Cavendish calls it the "moveable" magnetism. When the marked pole was up, the magnetic moment was $m_t + m_p$; when it was down—supposing for mathematical convenience the temporary magnetism to be the stronger—the magnetic moment was $m_t - m_p$.

If now we suppose with Cavendish that (3) is replaceable by

$$m = (1/C')\phi \quad \text{.............(4)},$$

where C' is a constant, we have

$$\left.\begin{aligned} A &= 2C'(m_t + m_p) \\ B &= 2C'(m_t - m_p) \end{aligned}\right\} \quad \text{.............(5)},$$

whence

$$\left.\begin{aligned} m_p &= (A - B)/4C' \\ m_t &= (A + B)/4C' \end{aligned}\right\} \quad \text{.............(6)}.$$

Thus the variations in $A - B$ and in $A + B$ tell us the changes in the permanent and temporary magnetic moments. These variations are shown in the following table.

TABLE I. "Fixed" and "moveable" magnetism.

Date		Blistered Steel		Blistered Steel (hammered), No. 1		Cast Iron, No. 1		Forged Iron, No. 1	
		$A - B$	$A + B$	$A - B$	$A + B$	$A - B$	$A + B$	$A - B$	$A + B$
1776		° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
May	29	27 30	8 30	—	—	—	—	—	—
,,	30	27 20	8 24	90 42	4 8	68 32	3 48	26 18	12 42
,,	31	—	—	89 50	3 50	68 14	3 26	24 12	11 22
June	2	27 32	8 28	90 42	3 52	69 0	3 40	24 19	11 35
,,	6	27 26	8 40	90 48	4 4	69 11	3 35	24 1	11 35
,,	19	27 21	8 41	90 48	3 52	69 13	3 37	23 46	11 28
,,	22	27 5	8 39	90 51	3 45	68 55	3 41	23 40	11 24
,,	30	27 11	8 35	90 54	4 0	69 12	3 32	23 27	11 49
July	2	26 51	8 39	90 43	4 3	69 1	3 37	22 51	11 35
,,	29	26 40	8 46	90 15	3 55	68 55	3 49	22 53	11 33
August	10	26 32	8 36	89 30	3 56	68 15	3 41	22 40	11 36
October	9	25 30	8 34	89 38	3 42	68 14	3 36	22 25	11 19
,,	27	25 34	8 34	89 34	3 50	68 2	3 32	22 28	11 22
1777									
June	2	24 20	8 44	86 43	4 3	66 2	3 48	21 30	11 34
,,	13	24 25	8 31	86 35	3 51	66 0	3 36	—	—

In considering the significance of these figures it must be remembered that C' in (4) is not an absolute constant, but varies with H, that the induced magnetism necessarily varies with changes in the vertical force, and that the magnetic moment of a magnet is a quantity having a temperature coefficient. Thus some irregular fluctuation was inevitable in the figures in Table I, even if the apparatus had possessed every modern refinement. That the fluctuations should be so regular as they are, considering the rough character of the apparatus, is a remarkable tribute to Cavendish's care and skill as an observer.

When we look at the large size of $A - B$ in the case of the second and third bars, the criticism that the use of the deflection angles instead of their tangents was under the circumstances very inexact naturally suggests itself. But it should be remembered that $A - B$ has to be divided by 4 to get the real size of the angle. If we take as an example $22°{\cdot}5$, i.e. $\pi/8$, we have

$$(\tan 22°{\cdot}5) \div (\pi/8) = {\cdot}414/{\cdot}393 = 1{\cdot}05,$$

so that even in this case the departure from unity is not very serious.

The first observation with the forged iron bar gives an outstandingly large value for both $A + B$ and $A - B$ which it is difficult to account for satisfactorily. With this exception the fluctuations in the values in the several $A + B$ columns appear to be fortuitous. They supply no evidence of any change in the induction coefficient in the earth's vertical field. With the $A - B$ columns, i.e. the permanent magnetic moment, it is quite otherwise. In the case of the blistered steel bars, whether hammered or not, there was evidently little if any change in the permanent moment so long as the bars were kept south pole up; but subsequent to June 30, 1776, when the earth's vertical field tended to demagnetise the bars, the loss of moment is clearly apparent. The cast iron bar seemed if anything to increase in moment up to June 30, 1776, but subsequently there was an undoubted fall. The forged iron bar seems to have lost moment from the start. Its percentage loss of moment subsequent to June 30, 1776, was similar to that in the other bars.

The differences between the individual bars—which, it will be remembered, were identical in size—are very striking. If the two blistered steel bars were of the same material, differing only in the hammering to which the one was subjected, the effect of the treatment was surprisingly large. Roughly speaking, it halved the temporary, and trebled the permanent magnetism, both excellent things for a horizontal force collimator magnet. The cast iron bar had apparently a slightly lower temporary moment than the hammered steel, and a permanent moment some 25 per cent. less. The forged iron bar was evidently the softest, its temporary induction being three or four times as large as that of the hammered steel and cast iron bars.

Of the remaining bars obtained from Elwell, Cavendish writes:

The three bars marked No. 2 were kept constantly with the mark upwards. In the first trials with them the compass was placed in the same position as with the other bars (i.e. the horizontal distance of the compass from the wall against which the magnets stood was 25 inches); in the others (i.e. in the later experiments) it was placed....17·8 (inches) from the wall.

The three No. 2 bars represented, like the No. 1 bars, blistered steel hammered, cast iron and forged iron. Apparently these bars were not stroked, as the No. 1 bars were, but had acquired a small amount of permanent magnetism before the readings commenced, presumably simply from continued exposure in one position to the earth's vertical field. The observations on them commenced on May 30, 1776. On May 31 after readings had been taken with the compass 25 inches from the wall, they were repeated with the distance reduced to 17·8 inches, and all the subsequent readings were taken at this reduced distance. As to the subsequent treatment of the bars, Cavendish writes of the blistered steel No. 2 bar that after the readings taken on June 10, 1777, it was "struck 100 times on anvil, falling 1·6 inches by its weight. This was repeated thrice, giving 300 blows in all." After the reading on June 11, 100 blows were repeated twice. Then 100 blows were repeated each day until June 26. It was then "struck almost every day" until July 12, "struck several times" between July 12 and August 21, and finally "struck

TABLE II. "Fixed" and "moveable" magnetism.

Date		Blistered Steel (hammered), No. 2		Cast Iron, No. 2		Forged Iron, No. 2	
		$A-B$	$A+B$	$A-B$	$A+B$	$A-B$	$A+B$
1776		° ′	° ′	° ′	° ′	° ′	° ′
May	30	1 3	5 3	1 30	3 44	0 45	10 15
,,	31	0 52	5 2	1 35	3 41	0 18	10 12
,,	31	2 20	11 0	3 15	8 15	1 4	22 10
June	2	2 25	11 15	3 12	8 18	1 29	22 45
,,	19	4 10	11 20	3 24	8 30	1 25	22 25
,,	25	3 57	11 13	3 24	8 20	1 40	22 34
July	28	4 38	11 18	3 33	8 37	1 2	22 58
August	10	4 38	11 18	3 35	8 29	1 30	22 50
October	10	5 35	11 15	3 40	8 34	2 0	23 6
,,	29	5 2	10 56	3 37	8 23	1 47	22 37
1777							
June	8	6 53	10 47	4 5	8 15	1 57	22 37
,,	10	6 52	10 52	—	—	—	—
,,	11	—	—	4 14	8 30	1 51	22 5
,,	11	8 4	11 10	—	—	—	—
,,	12	—	—	4 32	8 22	9 57	23 49
,,	26	8 35	11 5	4 44	8 30	10 37	23 59
July	12	8 38	10 58	4 49	8 23	10 50	23 38
August	21	8 50	11 4	4 43	8 17	10 52	23 32
October	1	9 22	11 2	5 2	8 32	11 30	22 54

sometimes" between August 21 and October 1. The cast iron No. 2 bar was struck 250 times on June 11, 1777, and 100 times on June 12 before the readings taken on that day. The forged iron No. 2 bar was struck 300 times on June 11, and 200 times on June 12 before the reading was taken. Apparently after July 12 the cast iron and forged iron bars were treated identically with the blistered steel bar. The results of the observations appear in Table II, the significance of $A - B$ and $A + B$ being the same as in Table I.

The first of the horizontal lines in the table marks the reduction in the distance between the magnet and the compass. Only the readings above this line are immediately comparable with those in Table I. The second horizontal line marks the introduction of the experiments intended to show the effect of mechanical agitation on the magnetic moment.

Comparing the values of $A + B$, i.e. the temporary magnetic moments, observed on May 30 and 31, 1776 before the reduction of distance, with those given for the corresponding No. 1 bars in Table I, we see that the differences are small. This means that the presence or absence of a large permanent magnetic moment makes little difference to the temporary magnetic moment.

The reduction effected on May 31, 1776, in the distance of the compass needle was doubtless intended to secure greater sensitiveness, the permanent magnetic moments being so small. Between May 31, 1776 and June 11, 1777 any change in the permanent moments must be regarded as arising from simple exposure to the earth's vertical field, acting steadily in the direction to increase the magnetism. The bars all show some increase of moment, but it is decidedly larger in the blistered steel bar than in the others. All this time, it will be observed, the temporary magnetic moment remained the same, or very nearly so, just as in the case of the No. 1 bars in Table I.

The introduction of mechanical agitation of the bars on June 11, 1777, produced at once a remarkable increase in the permanent magnetism of the forged iron bar. It had also a considerable effect on the blistered steel bar, but not much on the cast iron bar. The successive applications of the treatment tended apparently to further increase the permanent moment, but only to a comparatively minor extent. There is nothing to suggest that the mechanical agitation had any effect on the induction coefficient for temporary magnetism of the blistered steel or cast iron bars, but there is at least a suspicion of an increased induction coefficient in the forged iron bar.

If we take the formula (1) and simplify it by assuming the poles at the ends of the magnets—the distance being probably about $\frac{1}{12}$ of the magnet's length—we find that increasing the distance d from 17·8 to 25 inches would reduce the force F to about 0·43 of its value at the shorter distance. If we compare the sum of the $A + B$ deflection angles on May 31 before and after the increase of distance we obtain a ratio of 0·46. We shall thus certainly not be far wrong if we regard the angles observed on October 1, 1777, in Table II when multiplied by 0·45 as fairly comparable with the corresponding angles in Table I. We thus infer that mechanical agitation alone sufficed in the case of the forged iron to secure a permanent magnetic moment about a quarter the size of that obtained by stroking in the usual way. This agrees with the conclusions we should derive if we assumed the temporary magnetic moments equal in the bars Nos. 1 and 2.

Effect of Heat on Magnets

§ 3. A small packet of papers entitled "Effect of heat on magnets" deals with experiments on the effects produced by immersing magnets in water at about 115° F., the previous temperature being that of the air, about 65° F. usually. The experiments were made in July, but the year is not stated.

The effects of a sudden rise of temperature on a magnet or on an unmagnetised bar of iron or steel are somewhat complex. There is first of all a shock effect, apparently similar to that caused by mechanical agitation. If an ordinary magnet, especially one recently magnetised, has a sudden rise of temperature, such as occurred in Cavendish's experiments where the bar was lowered into a glass of hot water, an immediate loss of magnetic moment occurs. The loss may not be altogether permanent, i.e. if the magnet is kept for some days after the incident at its original temperature, a partial recovery of moment may take place. But a considerable part at least of the loss seems to be permanent. If the experiment be repeated a second time, there is usually still further loss, but the loss is less. After some repetitions there is at least an approach to a cyclic state of matters, in which the lower magnetic moment is associated with the higher temperature. The cyclic change, which alone represents the effect of a true temperature coefficient, is usually small, and unless there are a very large number of observations accidental changes in the earth's field affecting the readings of the auxiliary compass may introduce an undesirably large element of uncertainty, which can be eliminated only when magnetograph records are available. This will I hope explain why only a short summary of the results is given.

The experiments were confined to three bars. One of these, No. 4, had apparently been magnetised for some time. It was 10 inches long; its other dimensions are not recorded. Its marked end was a north pole. The observations were made exactly in the same way as in the case of the experiments summarised in Tables I and II. The double deflection angle, i.e. the algebraic difference of the readings of the auxiliary compass when level first with the one then with the other of the two poles of No. 4, is recorded in all cases, the marked end of No. 4 being sometimes up sometimes down. The differences between the results with mark up and down were very small, showing that No. 4 had a very small temporary induction coefficient. Its temperature coefficient was apparently also small. The shock effects are however apparent. On the first occasion the marked, i.e. north, pole was down when the sudden rise of temperature, amounting to 63·5° F., occurred. The double deflection angle fell from 44° 0′ to 43° 14′. On the next occasion, with the marked end up, a rise of 56° F. was accompanied by a fall of 48′ in the deflection angle. After this experiment No. 4 was kept for about a fortnight marked pole up, and so suffering the demagnetising effect of the earth's vertical field. This reduced the double deflection angle by about $2°\frac{3}{4}$. Subsequently two more sudden changes of temperature similar to the two previous were made. The first took place with the marked end up, the double deflection angle falling 36′. The second also took place with the marked end up, the double deflection angle falling only 8′. Some hours thereafter observations were made at normal temperature and a rise of 5′ was observed, as compared with the reading taken at the high temperature. This suggests that a close approach had been made to the cyclic state.

The two other bars had not been stroked prior to the commencement of the experiments, and possessed originally only a small amount of permanent magnetism, acquired under exposure to the earth's vertical field. They were both $19\frac{1}{2}$ inches long, the cross-section being a square of side 0·75 inch. No. 1 was of steel, No. 2 of iron.

When experiments began on No. 1 the lower end, whether the marked or the unmarked end, was a north pole, but the moment was somewhat larger when the marked pole was up. Thus the marked end was the south pole of the permanent magnetism, but the temporary considerably exceeded the permanent magnetism. When the first sudden rise of temperature, amounting to 41° F., was applied, the marked pole was up, and the double deflection angle rose 34′. Thus the heating acted apparently in the main as hammering would have done, facilitating the action of the earth's vertical field. On the second occasion a sudden rise of 48° F. of temperature occurred with the marked pole down. There was now a small fall in the double deflection angle with marked pole up, but a considerable increase in the angle with marked pole down. Also the deflection angle was now greater with the marked end down than with it up. Thus apparently the shock enabled the earth's field to reverse the magnetism. After this the bar was stroked so as to make the marked the north pole, and in the intervals between the experiments it was kept marked pole up, and so suffering demagnetisation from the earth's field. The cycle: normal, hot, normal temperature was applied on each of two days, one day intervening, the range of temperature being 47° F. on the first occasion and 52° F. on the second. On the first occasion the double deflection angle on return to the normal temperature showed a fall of 2° 58′; on the second occasion there was again a fall, but only of 25′.

The change from the intermediate hot to the final normal temperature was accompanied on both occasions by a rise in the double deflection angle, amounting to 41′ on the first occasion and 67′ on the second. The double deflection angle was about 37° on the second occasion. The passage from the high to the normal temperature on that occasion would certainly not be accompanied by more than a small fraction, if any, of the permanent loss sustained during the cycle. Taking the moment as proportional to the deflection angle, we get as a rough measure of the temperature coefficient per 1° F:

$$q = 67 \div (37 \times 60 \times 52) = \cdot 0006.$$

This is at least of the right order, but high for good magnet steel.

The No. 2 or iron bar was treated in a similar way to No. 1. Originally it showed little if any permanent moment, the double deflection angle being mark up 10° 7′, mark down 9° 50′. But on being suddenly raised 44° F. in temperature, the marked end being down, the readings were: mark down 11° 35′, mark up 9° 55′. Thus heating had the effect of a mechanical shock, the bar tending to have the pole that was down at the time made the north pole of a permanent magnet. Two days later the effect seemed to have largely disappeared, the bar appearing again to be nearly neutral. The temperature was again suddenly raised, the mark being this time up, and again the bar assumed a small permanent moment, the marked end being this time the south pole. The bar was then stroked, converting it into a permanent magnet with the marked end a south pole. The permanent and temporary moments in this bar were now so nearly equal that when the marked pole was down the deflection angle was almost nil. The temperature cycle: normal, hot, normal was applied on two days, one day intervening. On the first occasion the deflection angle fell both with the rise and the subsequent fall of temperature, the combined effect being a reduction in the double deflection angle from 19° 38′ to 18° 54′. On the second occasion the apparent differences between the readings were too small to possess any significance.

Strengths of Magnets of various cross-sections

§ 4. During May of some unspecified year Cavendish made a number of experiments on two needles made by Nairne and nine made by Elwell. Of the latter five were old and four new. He exposed them to a variety of treatment and tested their strengths, employing an auxiliary compass in the way already described. The deflection angles were of the order of 10°, so that replacing the angle by its tangent would have made little difference to the numerical results. After concluding the experiments Cavendish tried whether any formula could be found which represented satisfactorily the relation between the magnetic moment and the dimensions of the cross-section. The only calculations carried to a conclusion related to five magnets all by Elwell, but supplied at two different dates.

The information given as to their weights and dimensions in different places is not in all respects consistent, but the following data accord closely with the values employed by Cavendish for log bt and log $(b + t)$, where b denotes the breadth and t the thickness, and they also fit fairly with the recorded values of the weights. The dimensions are in inches. The descriptive terms and letters are those applied by Cavendish.

TABLE III. Dimensions of various magnets.

Bar	Length	Breadth	Thickness
"straight"	12·03	0·154	0·096
"square"	11·95	0·164	0·164
"flat"	12·03	0·600	0·041
B	12·03	0·50	0·073
E	11·90	0·26	0·067

The first three of these represented the earlier, the two last the later consignment. A remark by Cavendish seems to imply that the two lots were supposed to be of the same steel, but possibly it was intended to apply not to the bars B and E but only to one bar A of the later set, the results for which are not included. In any case the treatment of the older and newer bars as regards annealing may have been quite different. It will be seen that for practical purposes the lengths were all the same, 12 inches.

Cavendish tried a formula of the type

$$m = C\,(b + t)^p\,(bt)^q \qquad \text{.........................(7)},$$

where C was regarded as constant for the bars of the same consignment. After trying various values of p and q he drew the following conclusion: "Therefore if we suppose the force of the bars to draw (the compass) needle aside to be as $(b + t)^{\frac{1}{4}} \times (bt)^{\frac{5}{8}}$ it will agree as well with observation as any proportion." In other words the best values of p and q in (7) are $p = \frac{1}{4}$, $q = \frac{5}{8}$.

The formula makes the results for bars "straight," "square" and "flat" agree pretty closely, and likewise the results for B and E, but the values of C in the two cases differ sensibly.

Cavendish also writes the formula he approved in the form

$$\{(b + t)^{0\cdot25}/(bt)^{0\cdot125}\} \times (bt)^{0\cdot75} \qquad \text{.......................(8)},$$

presumably with the object of showing that in bars of the same length and similar form of section the magnetic moment varies as $D^{1\cdot5}$, where D represents the linear dimension of the cross-section. The weight of course varies as D^2, so that the magnetic moment increases less rapidly with the area of the cross-section than does the weight. Another way of regarding the result is that it gives moment $\propto$ (weight)$^{\frac{3}{4}}$.

It will be noticed that the formulæ tried were of the type

$$C's^pS^q \qquad \text{..................................(9)},$$

where s denotes the perimeter, S the area of the cross-section, and C' is a constant.

Cavendish proceeded to try whether any results could be obtained calculated to throw light on the best shape to give to dip needles, whether they should be of uniform width throughout, a common shape apparently in older needles, or should taper towards the ends. He thus carried out experiments in the way already described on the magnetic moments of three needles B, C, D of the same length and approximately the same thickness, B being of uniform width, C tapering from 0·5 inch at the middle of the length to about $\frac{1}{4}$ inch at the ends, and D still more tapering. The needles were apparently of the same steel. Particulars of the experimental results appear in the following table. The dimensions are in inches, but I have converted the weights, w, from pennyweights to grammes. The values in needle B of the quantities in the two last lines have been taken as unity for comparative purposes.

TABLE IV. Strength of dip needles of various shapes.

Needle	*B*	*C*	*D*
w	56·7	42·2	33·4
Length	12·03	12·03	12·03
Breadth at mid-length	0·5	0·5	0·5
Breadth at ends	0·5	0·24	0·06
Thickness (approx.)	0·07	0·07	0·07
Moment/w	1	1·14	1·19
Moment/$w^{\frac{3}{4}}$	1	1·07	1·04

One would rather infer that Cavendish regarded the formula

$$\text{moment} \propto w^{\frac{3}{4}}$$

as at least approximately satisfied by the results. From the remarks he made later in connection with the shape of dip needles, he evidently decided that the desirability of a taper in the needles was proved. We learn incidently that when fitted as dip needles the above would have an axle weighing about 11·7 grammes.

"SPRINGING" (ELASTIC BENDING) OF NEEDLES

§ 5. Cavendish also made experiments on what he calls the "springing of needles," meaning the elastic deflection under a load. In this case "needle" meant a rectangle of uniform breadth and small thickness. It was supported at the ends, in the flat position, and loaded at the middle. The deflection immediately under the load was measured, exactly how is not stated, but evidently with high precision. The "needles" seem to have been in general about 12 inches long, varying in width from 0·60 to 0·16 inches. A few particulars are given in each case.

For instance, weights of 2 oz., 8 oz. and 16 oz. applied successively to one of the "needles" caused it to "spring" 0·04, 0·15 and 0·305 inches. This presumably was intended partly as a check on the application of Hooke's law, and partly as some guidance to the choice of suitable weights. If we denote the length, breadth and thickness of the "needle" by $2a$, b and t, the observed "spring" or deflection by y, and the applied weight by w, the formula applied by Cavendish for the determination of Young's modulus E may be written

$$E = 2wa^3/ybt^3 \quad \text{.............................(10)}.$$

Cavendish himself uses A as the symbol for Young's modulus, and he expresses it in what is now rather an unusual way, viz. as a length modulus, with the inch as unit of length. In other words, E is to be regarded as the length in inches of a bar of the material which if attached to the end of a sample bar of the same section would double its length, supposing, what of course is not the case, that Hooke's law applied in that extreme case.

The following values are quoted by Cavendish for certain "needles" to which his descriptive titles are assigned. For comparison with modern data I have added a column showing the equivalent values of E in grammes weight per cm.^{2}, taking with Cavendish 7·8 as the specific gravity in each case.

TABLE V. Young's modulus in iron and steel needles.

"Needle"	Thickness in inches	E: Length modulus in inches	E: Grammes weight per cm.^{2}
Flat	0·0417	106·9 × 10^6	21·2 × 10^8
Straight	0·0953	84·7	16·8
Iron	0·0667	106·2	21·0
Iron hammered	0·0566	104·0	20·6
B	0·0732	103·6	20·5
C	0·0669	109·3	21·6

In another place Cavendish comments on the fact that the difference in elastic properties between iron and steel is not to any great extent due to the size of their elastic moduli, but to the weight which they can stand without permanent deformation. The values obtained above accord well with modern results. The thicknesses assigned to the bars called "flat," "straight" and "*B*" are sufficiently close to those given for the bars similarly designated in the experiments made in connection with formula (7) to render it almost certain that they were the same bars.

ERROR IN OBSERVED DIP DUE TO BENDING OF DIP NEEDLES

§ 6. Cavendish's experiments on the elasticity of needles were presumably suggested by his interest in a subject to which there are many references in his papers, the bending of dip needles. That the bending of a dip needle will introduce an error into the dip observed with it is one of those propositions which though really true are apt to be accepted for erroneous reasons. If one's attention is exclusively directed to the dipping end, the fact that bending of the needle under its own weight will bring the end nearer the vertical than it otherwise would be, and so increase the dip, appears sufficiently obvious; while if one's attention happens to be concentrated on the other end, the exactly opposite conclusion is acceptable. On reflection one sees that when one reads both ends of the needle, as is invariably done, the two effects neutralise one another, at least to a first approximation. It is true notwithstanding that the bending of needles does introduce an error, and that for a reason which Cavendish correctly apprehended. But if his views on the subject ever reached the notice of his contemporaries, they do not seem to have produced any permanent impression, and it was left for Sir Arthur Schuster[1], less than 30 years ago, to call attention to the fact in a convincing way.

In the figure suppose AB to represent diagrammatically the dip needle as it would be if weightless, and $A'B'$ to represent its actual position when supported at its centre C. If AB makes an angle ϕ with the horizontal, gravity g may be resolved into $g \sin \phi$ along and $g \cos \phi$ perpendicular to AB. Only the latter component tends to bend the needle. Thus G the centre of gravity of the bent needle is on the perpendicular to AB

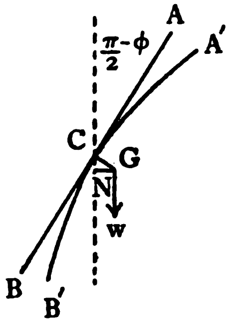

Fig. 3.

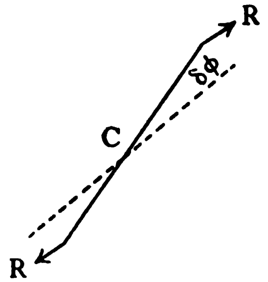

Fig. 4.

through C, and its distance GC from $AB = \bar{y} \cos \phi$, where $\bar{y}$ represents the reduction in the height of the centre of gravity due to bending when the needle is horizontal. If $GN = GC \sin \phi$ be the perpendicular from G on the vertical through C, the gravitational couple tending to turn the needle is wGN, where w is the weight of the needle, or substituting for GN in terms of $\bar{y}$ and ϕ, $w\bar{y} \sin \phi \cos \phi$. If the consequent deflection from the position, which the needle if weightless would assume be $\delta\phi$, the magnetic couple is obviously $mR \sin \delta\phi$, where m is the magnetic moment of the needle and R the total magnetic force.

Thus $$mR \sin \delta\phi = w\bar{y} \sin \phi \cos \phi,$$

or $$\sin \delta\phi = w\bar{y} \sin \phi \cos \phi / mR \quad \text{.......................(11)}.$$

[1] *Philosophical Magazine*, March 1891, p. 275.

Cavendish did not actually demonstrate this formula, but contented himself with announcing it in the following terms:

Error caused in dipping needle by its bending is ang(le) whose sine is to the rad(ius) as mot(ion) cent(re) grav(ity) × weight needle × sine × cosine of dip to $\frac{1}{2}$ length needle × force applied at end needle sufficient to draw it 90 degrees out of true direction.

A figure shows at once that the somewhat circuitous phrase "$\frac{1}{2}$ length needle × force applied at end sufficient to draw it 90 degrees out of true direction" merely represents mR. The angle $\delta\phi$ is so small that $\sin\delta\phi$ may be replaced by $\delta\phi$. It represents in all cases a reduction in the angle of dip. There is no doubt as to what "motion centre gravity" means because it is the title Cavendish actually attaches to $\bar{y}$ as defined above.

Cavendish calculated the value of $\bar{y}$ in three cases, viz. first when the needle is of uniform width throughout, second when it tapers uniformly from the centre to the sharp ends, and third when it tapers uniformly from a width $2b$ at the centre to a width $2f$ at either end. The last case (cf. Fig. 5, showing the half needle $BFF'B'$) is more general than and includes the first two as particular cases. The particular cases are the identical cases treated by Sir Arthur Schuster, and the results obtained for them are identical with his.

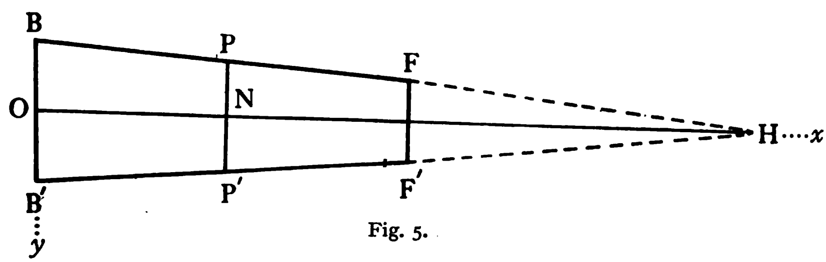

Fig. 5.

The more general case is considerably more difficult mathematically, but an ingenious substitution adopted by Cavendish enabled him to surmount the difficulties. His analysis, slightly modernised, is substantially as follows: If Ox and Oy be taken along and perpendicular to the central line ON of the needle, the ordinary elastic equation on the Bernoulli-Euler theory is $E\omega\kappa^2 \frac{d^2y}{dx^2}$ = bending moment arising from the weight to the right of the section PP', at distance $ON = x$. Here $\omega\kappa^2$ represents the moment of inertia of the cross-section through N about the perpendicular to PP' through N. In Fig. 5, $BFF'B'$ is to be regarded as the outline of the half needle in the mid-plane of its thickness, this mid-plane being vertical and containing the horizontal axis Ox and the vertical axis Oy, the positive direction of y being downwards. If t be the thickness and η represents PN, then for the cross-section through N

$$\omega\kappa^2 = 2t\eta^3/3.$$

Suppose the two edges BF, $B'F'$ of the needle to be prolonged and meet in a point H, which lies by symmetry on the median line ON, and let α denote the angle BHO. Then by regarding the portion $PFF'P'$ of the needle as the difference of the two triangles PHP' and FHF', it is easily seen that the bending moment of the mass to the right of the section PNP' is

$$\eta . \eta \cot \alpha . \tfrac{1}{3}\eta \cot \alpha . g\rho t - f . f \cot \alpha \, (\eta \cot \alpha - \tfrac{2}{3} f \cot \alpha) \, g\rho t,$$

where ρ is the density of the material and g gravity.

This reduces to $\quad g\rho t \cot^2 \alpha \, (\tfrac{1}{3}\eta^3 - f^2\eta + \tfrac{2}{3}f^3).$

Again $\quad x = ON = OH - HN = \cot \alpha \, (b - \eta),$

and so $$\frac{dx}{d\eta} = -\cot \alpha,$$

and $$\frac{d^2y}{dx^2} = \tan^2 \alpha \frac{d^2y}{d\eta^2}.$$

Thus the bending equation becomes

$$E \left(\tfrac{2}{3}\right) t\eta^3 \tan^2 \alpha \frac{d^2y}{d\eta^2} = g\rho t \cot^2 \alpha \, (\tfrac{1}{3}\eta^3 - f^2\eta + \tfrac{2}{3}f^3),$$

or $$\frac{d^2y}{d\eta^2} = \frac{3g\rho \cot^4 \alpha}{2E} \left(\frac{1}{3} - \frac{f^2}{\eta^2} + \frac{2}{3}\frac{f^3}{\eta^3}\right) \qquad \text{.....................(12).}$$

Integrating this in two steps, noticing that $\dfrac{dy}{d\eta}$ and y both vanish when $\eta = b$, we find

$$y = \frac{3}{2}\frac{g\rho \cot^4 \alpha}{E} \left\{\frac{1}{6}b^2 + f^2 - \frac{2}{3}\frac{f^3}{b} - f^2 \log b - \frac{1}{3}b\eta - \frac{f^2}{b}\eta \right.$$
$$\left. + \frac{1}{3}\frac{f^3}{b^2}\eta + \frac{1}{6}\eta^2 + \frac{f^3}{3\eta} + f^2 \log \eta \right\} \text{...(13).}$$

The logarithm is to the Napierian base.

If $\bar{y}$ denote the distance of the centre of gravity of the bent needle below ON, a being the half length of the needle, we have

$$\bar{y} = \int_0^a y \, 2\eta \, dx \div \text{area of } BFF'B'.$$

Noticing that area $BFF'B' = (b^2 - f^2) \cot \alpha$, and changing from x to η as variable, we have

$$\bar{y} = \frac{3g\rho \cot^4 \alpha}{E(b^2 - f^2)} \int_f^b \{\eta \, (\tfrac{1}{6}b^2 + f^2 - \tfrac{2}{3}f^3 b^{-1} - f^2 \log b)$$
$$+ \eta^2 (-\tfrac{1}{3}b - f^2 b^{-1} + \tfrac{1}{3}f^3 b^{-2}) + \tfrac{1}{6}\eta^3 + \tfrac{1}{3}f^3 + f^2 \eta \log \eta\} \, d\eta.$$

Carrying out the integration, and replacing $\cot \alpha$ by $a/(b - f)$, we find eventually, in exact agreement with Cavendish,

$$\bar{y} = \frac{g\rho a^4}{E(b-f)^5 (b+f)} \left\{\tfrac{1}{24}b^4 - \tfrac{1}{2}b^2 f^2 + \tfrac{2}{3}bf^3 - \tfrac{15}{8}f^4 + 2b^{-1}f^5 - \tfrac{1}{3}b^{-2}f^6 + \tfrac{3}{2}f^4 \log \frac{b}{f}\right\}$$
$$\text{......(14).}$$

For the special case of the pointed needle putting $f = 0$ we find

$$\bar{y} = g\rho a^4/24Eb^2 \quad\text{.............................}(15),$$

which is identical with Prof. Schuster's equation (3), allowing for the difference of notation, Prof. Schuster's l, a, Y answering to our a, $2b$, E respectively.

The other special case treated by Prof. Schuster, $f = b$, does not follow conveniently from the above, because the method of proof tacitly assumes that α and *therefore* $(b - f)/a$ is finite, and the expression (14) assumes the form $0/0$ if we put $f = b$. It was treated independently by Cavendish, who found

$$\bar{y} = 3g\rho a^4/20Eb^2 \quad\text{..........................}(16),$$

which is in agreement with Prof. Schuster's equation (4).

Cavendish evaluated (14) for a series of values of b/f, finding for

$$\begin{array}{rcccccc} b/f & = 1 & 1{\cdot}5 & 2 & 3 & \infty \\ \bar{y} \div (g\rho a^4/Eb^2) & = 0{\cdot}150 & 0{\cdot}124 & 0{\cdot}108 & 0{\cdot}090 & 0{\cdot}047. \end{array}$$

The needle he was particularly interested in had

$$a = 6,\ \ b = \tfrac{1}{4},\ \ f = \tfrac{1}{8}, \text{ in inches, and so } b/f = 2.$$

For its length modulus he took 107×10^6 inches, in other words he replaced E by $g\rho \times 107 \times 10^6$. Thus he had

$$\bar{y} = \frac{6^4 \times 4^2 \times 0{\cdot}108}{107 \times 10^6} = 0{\cdot}0000209 \text{ inches.}$$

Cavendish writes down, however, 0·0000265 inches.

The numerical factor given for the case $b/f = 2$ is at least very approximately correct. I find 0·1085, while to bring the result for $\bar{y}$ up to Cavendish's value we want the factor to be 0·136 approximately. A possible explanation is suggested by the series of values quoted in § 5 for Young's modulus. The value which appears above, 107×10^6, obviously answers to that of the first "needle" on the list described as "flat." If, however, we replace it in the calculation by 85×10^6, the value of the next "needle," which is described as "straight," employing the more exact value 0·1085 of the factor we agree exactly with Cavendish's figure 0·0000265 inches.

He proceeds

I find also that the force which must be applied to the end of the needle in order to draw it aside 90 degrees is 1/600 of the weight of the needle, consequently the error caused by the bending of the needle in this climate where the dip is 72°½ is about 2½ minutes, but in a place where the dip was 45° would be about 4½ minutes.

This is easily shown by means of equation (11) if we notice that in modern language Cavendish's statement is equivalent to $mR = \frac{1}{600}wa$, where a is 6 inches.

Substituting in (11) we have

$$\sin \delta\phi = 100 \times 10^{-7} \times 265 \times 0{\cdot}5 \sin (145^\circ) = 0{\cdot}00076,$$

or $\delta\phi = 2'{\cdot}6$ approx.

For $\phi = 45^\circ$, $\sin \delta\phi = 100 \times 10^{-5} \times 265 \times 0{\cdot}5 = 0{\cdot}001325,$

or $\delta\phi = 4'{\cdot}6$ approx.

The phraseology adopted by Cavendish naturally suggests that the magnetic moment was actually found by balancing the magnetic couple against a gravitational couple. This would be quite practicable. One might, for instance, determine the weight required to be suspended from one end to bring the needle into the horizontal position, or into the position at right angles to the natural dip. But, as a matter of fact, a different procedure seems to have been adopted. This is what Cavendish writes on the subject:

Nairne's needle is 12 inches long $\frac{1}{2}$ inch broad in middle and $\frac{1}{4}$ at end, therefore force applied at end sufficient to turn it on centre is 10/36 of what it would be if all the matter was collected at end, therefore if the needle vibrates in 5″, force required to draw it aside $90^\circ = \dfrac{6 \times 10}{39{\cdot}12 \times 25 \times 36} = \dfrac{1}{600}$ of weight nearly.

From a small calculation on the same page, deducing the ratio 10/36 as quoted above, it is clear that what Cavendish means by "force applied at end sufficient to turn it on centre" is simply the moment of inertia I of the needle about its axis of rotation. He is obviously employing the ordinary formula for the time T of swing from rest to rest, viz.

$$T = \pi\,(I/mR)^{\frac{1}{2}}.$$

This gives $mR = \pi^2 I/T^2.$

His first statement really means

$$I = \frac{10}{36}\frac{w}{g}a^2,$$

whence

$$mR = a\left\{\frac{10}{36}\frac{wa}{(g/\pi^2)}\frac{I}{T^2}\right\}.$$

Taking the inch as unit of length,

$$\begin{aligned} a &= 6,\\ g &= 32{\cdot}2 \times 12 = 386{\cdot}4,\\ g/\pi^2 &= 39{\cdot}12 \text{ very nearly.} \end{aligned}$$

Thus

$$\begin{aligned} mR &= a \times 10 \times 6w \div (39{\cdot}12 \times 36 \times 25)\\ &= aw/600 \text{ very nearly.} \end{aligned}$$

The ratio 10/36 is deduced as follows:

force necessary to turn this needle on its centre is to that necessary to move the same quantity of matter placed at the end

$$\begin{aligned} &:: \textstyle\int x^2 (b - x\tan\alpha)\,dx \;:\; \frac{1}{2}(b+f)\,a^3\\ &:: \tfrac{1}{3}ba^3 - \tfrac{1}{4}a^4\tan\alpha \;:\; \tfrac{1}{2}(b+f)\,a^3\\ &:: 4b - 3a\tan\alpha\,[\equiv (4b - 3b + 3f)/12] \;:\; \tfrac{1}{2}(b+f)\\ &:: (b+3f)/6 \;:\; b+f. \end{aligned}$$

As $b = 2f$ the ratio is 5 : 18, or 10 : 36 as stated.

The introduction of $\frac{1}{2}(b+f)\,a^3$, i.e. $\frac{1}{2}(b+f)\,a \times a^2$, is alone sufficient to make it quite clear that what Cavendish had in view was the moment of inertia. The integral given, however, is not an absolutely complete expression for the moment of inertia of the needle—or rather the quarter needle. The complete expression is

$$\int_0^a \{x^2(b - x\tan\alpha) + \tfrac{1}{3}(b - x\tan\alpha)^3\}\,dx.$$

For the present case in which $b = 2f = a/12$, the ratio of the term neglected to that retained proves to be $3\,(f/a)^2 : 1$, or $1 : 192$. Thus the neglect of the term, whether intentional or otherwise, is quite immaterial.

Sources of error in Dip Observations

§ 7. Cavendish discusses several other sources of error in dip observations. If the needle be in a vertical plane inclined at an angle α to the magnetic meridian, the observed dip is necessarily too great. If ϕ be the true and $\phi + \delta\phi$ the observed dip, we have obviously

$$\tan(\phi + \delta\phi) = \tan\phi\sec\alpha \quad \text{.....................(17)}.$$

This is the most convenient form for exact logarithmic determination of $\phi + \delta\phi$, and so of $\delta\phi$, when ϕ and α are known.

When α is not large the approximate formula

$$\delta\phi = \tfrac{1}{4}\,\alpha^2\sin 2\phi \quad \text{..............................(18)}$$

is convenient. It shows at once, as remarked by Cavendish, that for a given error in the meridian setting of the dip circle, the consequent error in the dip is greatest when the true dip is 45°.

In Cavendish's time, in London, ϕ was about $72°\frac{1}{2}$, and so $\sin 2\phi = 0{\cdot}574$. This reduction factor had consequently to be applied to deduce from the errors calculated for $\phi = 45°$ the corresponding errors in the case of observations made in London near the end of the eighteenth century. For $\phi = 45°$ Cavendish quotes for $\alpha = 2°$, and for $\alpha = 5°\frac{5}{8}$ the respective values $1'$ and $7'$. In the latter case, I suspect, he inadvertently took $5°\frac{1}{8}$ instead of $5°\frac{5}{8}$ (i.e. half a point). For 2° and $5°\frac{5}{8}$ more exact values are $1'{\cdot}05$ and $8'{\cdot}35$. Consequently in Cavendish's time an error of half a point in the setting of the dip circle in London would have entailed an error of only $4'{\cdot}8$ in the observed dip.

Another source of error considered by Cavendish is the existence of a protuberance on the axle of a dip needle, resulting from imperfect polishing. In his time dip needles were either carried on friction wheels or with their ends rolling on flat planes, so that the conditions were not the same as now exist. He concludes that if the height of the protuberance be regarded as constant, the consequent maximum error in the dip will vary as the square root of the diameter of the axle, while if the height of the protuberance be supposed to vary as the diameter of the axle, the error also will vary as this diameter. He uses these results as an argument for having the axles of as small diameter as is compatible with their being sufficiently strong to bear the weight of the needle satisfactorily. I do not on this point follow Cavendish's reasoning, which must, I think, make some assumptions not fully disclosed. Something will depend in practice on how the observations are actually taken. In modern practice the difficulty presents itself usually from the occurrence of rust on the axle. It is now usual to set the microscope wire to the estimated mid position of the point of a vibrating needle, and the error consequent on the presence of a speck of rust depends a good deal on whether one observes with a large or small arc of vibration. Cavendish seems to assume the needle to be at rest when read. The calculations he made as to the effect of a protuberance, of what he considered a probable size, evidently somewhat startled him. He remarks

it seems surprising how it should be possible (to make) the axis so true as that the needle should not be liable to a greater error, and indeed the only way by which I can account for it is by supposing that the axis (axle) and plane on which it rolls do not actually touch but are kept from one (another) by a repulsive force.

This remark is taken from a MS. inscribed "On the different construction of dipping needles." In it Cavendish enumerated the following four principal sources of error: (i) imperfections of the axle, (ii) departure of the axle from horizontality, (iii) observing out of the magnetic meridian, (iv) bending of the needle. As regards (i) his opinion that the axle should be as fine as the weight of the needle allows has been already mentioned. No sensible error he says should arise from (ii) assuming ordinary care is exercised in levelling the dip circle, provided readings be always taken as he advocates with the instrument facing both east and west. As regards error (iii) the figure he obtained, viz. 1′, as the maximum error of dip for an error of 2° in the setting of the circle, shows that with reasonable care in determining the magnetic meridian no sensible error should arise from this cause in observations on land. At sea (iii) is a more serious source of error, but Cavendish had been assured that unless it is very rough it is rare for a ship to deviate as much as half a point from the direction it is intended to steer in, so that the average departure from the magnetic meridian during the taking of a dip observation ought with proper care to be but a small fraction of half a point, and the consequent error in the dip should thus be much under 7′ (or more exactly 8′). As regards (iv) he had found in the way already explained that with a needle such as he himself used the effect of bending was to reduce the dip observed in London about 2′½.

The following is a summary of the practical conclusions reached:

1°. The less the diameter of the ends of the axle (where it rolls), supposing them to be equally well ground, the less the error arising from irregularities of shape.

2°. The slenderer the needle, the less will be the error arising from defects in the axle, because the magnetic moment is larger in proportion to the weight in a slender needle. Also the more slender the needle, the less need be the diameter of the axle. A very slender needle, however, is liable to increased error from bending.

3°. The longer the needle, the greater the errors to which it is liable. The longer the needle the greater the error due to bending, also the greater the weight, and so necessarily the thicker the axle. There is, however, a practical limit to the reduction in length because the pointing of a long needle can be read more accurately than that of a short one.

4°. It is better that the needle should taper than have a uniform breadth from centre to ends (as seems to have been the case with many of the older needles). Experiments he had made showed the ratio of the magnetic moment to the weight to be greater in a tapering needle than in one of uniform width. By tapering the needle the weight is reduced, and so the necessary thickness of axle. The time of vibration is also reduced, which is advantageous from the observational point of view. Finally he ventures on the following anticipation of the shape now generally adopted in dip needles: "If the weight of the axis (axle) is very small in proportion to that of the needle, I should think it would not be worse if it was made still more tapering, or even brought almost to a point." A note indicates that he thought the ratio of the weight of the blade of the needle to that of the axle should not be less than 3 : 1.

It is interesting to find that Cavendish anticipated the modern idea that small magnets have many theoretical advantages. In others of his MSS. he remarks on the difficulties and errors associated with the use of the large dip needles—some of them about four feet in length—employed by certain of the old English observers.

Instructions to Observers and General Notes

§ 8. Several short manuscripts deal with observations at sea. They are entitled "For Cook and Bayley. Directions for the use of the dipping needle," "Directions for using the dipping needle for Dalrymple," "On the different forms of constructing a dipping needle," etc. Mr Dalrymple, it seems, was to sail to Madras round the Cape of Good Hope, while Captain Cook was about to sail presumably on one of his three great voyages of exploration which began respectively in 1768, 1772 and 1776. There is naturally a good deal that is common in the several MSS., and much of what is said is by way of instruction as to the use of instruments now obsolete. There are, however, various remarks of interest. Referring to the subject of ships magnetism, he writes:

If there are no large iron bars in the ship except such as stand upright then these bars will be equally magnetised and the direction of magnetism in them will be the same whatever tack the ship goes on. Consequently if they draw the (compass) needle out of its true direction towards the west when the ship sails on one course, they will draw it as much to the east when the ship sails on the direct contrary course. But if there are any large horizontal bars in the ship this will not be the case, for the direction of magnetism in these bars will be reversed by the ship's turning round. The guns are large horizontal bars, but as they are of cast iron I believe they will not easily acquire magnetism, and when they have acquired it, its direction will not easily be changed.

Cavendish wrote of course in the days of wooden ships, when such comparatively small amount of iron as there was, except in the shape of cannon, was likely to be soft iron. His remarks on cast iron were presumably based on the experiments summarised in Tables I and II, which show it to have a small induction coefficient for temporary magnetism. He proceeds:

It would be of great use if a way could be discovered of finding by an easy experiment at sea how much the needle is drawn out of its true direction in different positions of the ship. I say of doing it at sea, because in all probability the quantity by which it is affected will be very different in different parts of the world. If there are no horizontal bars in the ship this may be done in this manner. First find the variation in the usual way with the ship's head to the north by the compass, then turn round 4 points and observe as before, and proceed in that manner till you have got all round the compass. The mean of these 8 observations will be the true variation, whence you may find how much the variation is affected by the iron work in each position of the ship.

After commenting on the practical difficulties, he proceeds

If either from the quantity of horizontal iron or from other causes this method of finding the error of the compass is impracticable, it still might be possible doing it in a harbour in this manner: Let the ship be brought in a line between 2 objects on shore, and take the bearing of those objects by the compass, with the ship's head in different directions, while another person places himself on shore, also in a line between those 2 objects and takes their bearing by another compass.

A note attached to this adds

An easier way will be for the person on shore to place himself in any situation and to take the bearing by the compass of the observer on board the ship, at the same time that the observer on board the ship takes the bearing of the person on shore.

In another place the suggestion is made that the observers on shore and aboard should interchange compasses and repeat the observations.

"Perhaps," Cavendish adds, "by making experiments in different parts of the world rules might be found out by which a person who knows how much his compass is affected by iron work in one part of the world may find how much it would be in another."

These remarks, it must be remembered, were written long before the days of Poisson and Archibald Smith.

In the instructions to Captain Cook and Dalrymple, Cavendish emphasises the fact that not only must dips be taken with the circle facing both east and west, but further that the result is imperfect unless the poles be reversed. He allows, however, that time for taking the full experiment with the poles reversed may not be always available. In this event he suggests that on the occasions when time allows, the process of reversing the poles should be gone through several times, so as to get a reliable value for the difference between the readings with marked and unmarked poles dipping, and so for the correction to be applied when readings are taken with only one pole dipping. He thought it specially important that this should be done before crossing the magnetic equator. He mentions how best allowance may be made for the error resulting when readings are taken with one pole only dipping. Suppose, for example, that the marked pole gives the larger dip. Suppose the excess on one occasion to be found to be $\delta\phi$, the true dip—i.e. the mean of the dips obtained on that occasion first with the marked end and then with the unmarked end dipping—being ϕ_1; and suppose on the next similar occasion the excess to be $\delta\phi_2$, the true dip being ϕ_2. Then on any intermediate occasion, when ϕ was the observed dip from observations with the marked pole only dipping, the true dip may be taken as

$$\phi - \tfrac{1}{4}(\sec\phi_1\delta\phi_1 + \sec\phi_2\delta\phi_2)\cos\phi.$$

Cavendish merely states the result, but the proof is easily supplied. The difference between the two ends arises from the centre of gravity being on one side or the other of the centre of the axle. It is obviously nearer the end which when dipping gives the bigger dip, and unless the needle is lop sided may be supposed to be in the median line at a distance c from the axis. With the marked end dipping, the gravity couple $wc\cos\phi$, where w is the weight of the needle, pulls the needle through the angle $\tfrac{1}{2}\delta\phi$ out of the direction of the true dip. Thus if m be the magnetic moment and R the total force

$$mR\sin(\delta\phi/2) = wc\cos\phi.$$

The angle $\delta\phi$ in any reasonably well made needle is so small that the sine may be replaced by the angle, and so

$$\delta\phi = 2\,(wc/mR)\cos\phi.$$

Thus we have

$$\tfrac{1}{4}(\sec\phi_1\,\delta\phi_1 + \sec\phi_2\,\delta\phi_2) = \frac{wc}{2}\left(\frac{1}{m_1R_1} + \frac{1}{m_2R_2}\right).$$

And if we may suppose $\dfrac{1}{m_1R_1} + \dfrac{1}{m_2R_2} = \dfrac{2}{mR}$, we obviously have

$$\phi - \tfrac{1}{4}(\sec\phi_1\delta\phi_1 + \sec\phi_2\delta\phi_2) = \phi - \tfrac{1}{2}\delta\phi,$$

as it ought to be.

If a needle is always magnetised in a uniform way, with the same bar magnets, the moment acquired will naturally be nearly uniform, and the variations of total force with latitude or longitude are much less rapid than those of vertical or horizontal force. The correction of course is put forward not as a perfect one, but only as the best that is forthcoming. w really varies slightly with latitude, but any such variation would be negligible in view of the several uncertainties. Under the circumstances supposed, the needle was presumably freshly magnetised only when the poles were reversed. If this were the case, m would naturally tend to fall, and $\delta\phi$ would correspondingly increase. The possibility of an indirect effect of m upon the dip—the moment not being exactly the same when the marked and unmarked poles dip—is in fact one of the weak points in dip observations.

It should be added that Cavendish himself seems generally if not always to have observed the period of oscillation of the dip needle, or rather the time occupied by a given number of oscillations. At a fixed station this should afford a very good check on the uniformity of the results obtained when stroking the needle on different occasions. Unless a needle is unusually thick or of exceptionally hard steel, it does not require very powerful bar magnets to practically "saturate" it. Thus if any of Cavendish's original needles whose period he has recorded remains in working order, and could be identified, it might possibly serve to give an approximate estimate of the intensity of the total (i.e. resultant) force in his day.

Amongst the instructions to explorers are two which have nothing to do with magnetism, but may perhaps be mentioned for their general interest. Cavendish expresses a wish that observations should be made at frequent intervals on sea temperatures, the water being apparently secured with a bucket lowered overboard. An officer who had made previous observations of the kind had concluded that in all cases the sea temperature closely approached that of the air, but Cavendish thought confirmation desirable. His second suggestion was that in any land explorations measurements should be made of the temperature of any deep well, or natural spring having a considerable flow. He thought in this way, judging by his experience in England, that a good guess might be made at the mean annual temperature of the place.

Dip Observations

§ 9. There are amongst the magnetic papers a good many notes of dip observations. They mostly refer, however, to comparisons made of different needles—e.g. "Royal Society's needle" and "father's (Lord Charles Cavendish's) inverting needle," which are hardly of general interest. Certain of the comparisons seem to have been made at the "Royal Society's House," which as a place of observation seems to have suffered from the presence of "Dr Knight's magnets[1]." One of the series of comparisons seems worth recording, as it was on an elaborate scale, and was carried out in the garden of Lord Charles Cavendish's house in Great Marlborough Street, a presumably undisturbed place. The date was 1775. The details are of interest as showing the accuracy reached at the time in the construction of dip needles.

TABLE VI. Dip observations in London in 1775, with various needles.

Description of needle	Mark upwards, Instrument facing East	Mark upwards, Instrument facing West	Mark dipping, Instrument facing East	Mark dipping, Instrument facing West	True dip
	° ′	° ′	° ′	° ′	° ′
Royal Society's	72 46	71 59	72 8	72 40	72 23
Nairne's	72 54	72 28	71 45	73 21	72 37
"My new needle"	72 34	72 20	71 41	73 23	72 30
"My old needle"	71 40	73 53	72 19	72 19	72 33
Nairne's new needle	73 8	72 0	73 15	71 57	72 35
Sisson's	73 1	71 49	71 57	73 0	72 27
Means	72 40	72 25	72 11	72 47	72 31

In most cases several observations seem to have been taken with each needle, the days of observation including October 10, 11, 13 and 14, 1775. But the last needle, by Sisson, was tried at a different time, April 15, 1775, and only the final means are given in its case.

The observations being in the garden were doubtless taken by daylight, and so presumably, considering the season of the year, not very far from noon. The total range of the regular diurnal variation in October is, however, on the average only about $1'\frac{1}{2}$, so the precise hour is of minor importance. At the same time there is always the possibility of magnetic disturbance. Thus the differences between the different needles shown by the table are more likely to be over-estimates than under-estimates. There is no explicit statement as to whether the needles were all tried in the same dip circle. If they were tried in the same circle, the differences between the results obtained with the circle facing east and west were in considerable measure due to the needles themselves. They are larger, but still not so very much larger, than the differences one gets with modern instruments.

The MSS. include particulars of observations made apparently in the Royal Society's House in 1775, 1776 and 1778. Observations in all three cases were taken at 7 a.m., noon, 2 p.m. and 10 or 11 p.m., on a number of successive days. On each occasion the needle was read only with the one end dipping and with the circle in only one position. But on different occasions the position of the circle was varied, and the pole which dipped

[1] Mr Harrison, when Assistant Secretary Royal Society, informed me that these magnets after being kept in the Royal Society's custody for about 100 years were eventually transferred to the South Kensington Museum.

was altered, so that the earlier and later observations of the series were made with different poles dipping. It was thus possible to obtain from the series as a whole a final mean value for the dip fairly representative of what would have been got if complete observations with both poles dipping and with the circle facing both east and west had been made on each occasion. The observational mean thus found in 1775, 72° 30′·2, was brought up to 72° 31′½ by the application of a correction of + 1′·3 to allow for the disturbing effect apparently of Dr Knight's magnets. The observations extended from June 19 to July 4, and included in all 64 separate readings, 16 at each hour. The mean from the four hours combined ought to be nearly free from accidental results, but the means for the separate hours seem affected by considerable probable errors. A mean derived from the four hours of the day selected might be expected to exceed the true mean for the day, but only by about 0′·1.

The data for 1776 were similar in character, the only difference being that the last hour of observation was 11 p.m. instead of 10 p.m. The observations in this case extended from June 21 to July 7. There were at least two observers, one of whom described as "Young Rob(erton)" was apparently regarded as the more reliable. The final mean derived from his observations alone was 72° 30′. The mean when all the observations were included seems to be the same. Cavendish says 72° 31′, but the difference was apparently due to an arithmetical error which he subsequently corrected.

In 1778 the last hour of observation was sometimes 10 and sometimes 11 p.m. In other respects the procedure was apparently the same as in 1775 and 1776. The sheet itself gives only the four mean results derived from the east and west positions of the circle and the two poles dipping. The final mean from the four combined is 72° 26′. In the case of the 1776 and 1778 observations nothing is said as to any correction.

About this time it is known from other sources that Cavendish was in general charge of the magnetic and meteorological observations carried on at the Royal Society's House. The results of these observations were published from time to time in the *Philosophical Transactions*, from which I have extracted the following particulars, including all I could find for the inclination:

TABLE VII. Dip in London at Royal Society's House.

Year	Mean dip ° ′
1775	72 30
1776	72 30
1777	72 25
1778	72 26
1779	72 21
1780	72 17

The results given here for 1776 and 1778 are identical with those given above, and the same is also true of 1775 if the correction made on account of Dr Knight's magnets is omitted. It will be noticed that the result obtained for 1775 in the Royal Society's House agrees closely with the mean which Cavendish himself obtained from observations with six needles in the garden of the house in Great Marlborough Street during the same year. This encourages the hope that the results obtained at the Royal Society's House give a fairly reliable indication of the true dip in London, and of its secular change from 1775 to 1780. It would seem to have been decreasing at the rate of about $2'\frac{1}{2}$ per annum.

The only later systematic results which I have observed in the MSS. were obtained in 1791 with two needles by Nairne, described as "thick" and "thin" respectively, and one by Sisson. The observations with the two Nairne needles were made on August 4 and 7, those with the Sisson needle on October 2. Judging by the differences in the several positions, the Sisson needle was the same as that similarly described in Table VI. The Nairne needles may also have been the same as figured in Table VI, but that seems more doubtful. The mean dips given by the three needles were respectively $71° \, 45'\frac{1}{2}$, $71° \, 43'$ and $71° \, 50'$, the final mean from the three being $71° \, 46'$. If we combine this with the mean derived for 1775 from all the needles in Table VI we deduce a fall of $45'$ in 16 years, or an average fall of $2'{\cdot}8$ per annum.

Other dip observations possessing a present day interest are some taken on a limited magnetic survey which Cavendish conducted during 1778. He observed in London before and after his journey, in the course of which he took dip observations at Oxford, Birmingham, Towcester, St Ives and Ely. The observations in London were taken on August 8, 10, 19 and 22, in a garden (probably that of the house in Great Marlborough Street). At Oxford the observations were made on August 14 "in garden of Observatory." The observations in Birmingham followed on the 15th "in bowling green." The observations at Towcester and St Ives were apparently made both on the 17th, the former in a garden, the latter in a room. The observations at Ely were made on the 18th in a garden. The place of observation at St Ives was not altogether satisfactory, owing to the proximity of iron. Observations made, however, in two positions, one considerably nearer the disturbing object than the other, differed by only $2'$, so the disturbing effect was presumably very small. Two needles described respectively as "new" and "old" were employed; but on return to London "the balancing screws were found to be loose" in the "old" needle, and results obtained with it were discarded. Cavendish gives the dip observed at each station, its difference from the London dip, and the difference of the geographical coordinates of the place from London. His figures, with information as to the apparent change in dip between 1778 and 1891, obtained by comparing Cavendish's figures with those obtained by Rücker and Thorpe, appear in the following table:

TABLE VIII. Observations of dip in London and elsewhere in 1778.

Place	Dip	Difference from London: Dip	N. Latitude	W. Longitude	Excess above dip in 1891
	° ′	′	′	′	° ′
London	72 19	0	0	0	4 52
Oxford	72 48	29	13	72	4 56
Towcester	73 1	42	36	60	4 58
St Ives	72 35	16	48	4	—
Ely	72 41	22	54	15	4 43
Birmingham	73 5	46	58	112	4 50

In comparing the results for 1778 and 1891 it must be remembered that the actual sites occupied by Cavendish and by the observers in Rücker and Thorpe's survey were not the same. Even a small distance between the sites occupied might mean several minutes in the dip. The differences between the latitudes and those of London given by Cavendish agree fairly well with those assigned by Rücker and Thorpe to the places of the same name, if we suppose "London" to be at about 51° 31′, or about 3′ north of the mean latitude of Greenwich and Kew. Thus in taking a mean value from Kew and Greenwich as the "London" value for 1891 we should do fairly well. The longitude differences however given by Cavendish would seem to require "London" to be very nearly in the meridian of Greenwich, whereas the supposed site was about 10′ west.

Taking a mean from the five places for which secular change data are available we find 4° 52′, a result identical with that obtained from London alone. This gives 2′·6 for the average annual fall from 1778 to 1891, but we know that between 1860 and 1891 the average rate in London was only about two-thirds of this, so for part of the time it must have been considerably more rapid.

Cavendish did not content himself with merely obtaining the observational results, but drew the following inference as to the direction of the isoclinal lines (i.e. lines of equal dip):

By comparing London, Ely and Oxford dip should increase

30′ by going 1° to north,
18′ ,, ,, ,, ,, west;

according to which supposition

dip at Birmingham should be 62′½ greater than at London,
,, ,, Towcester ,, ,, 36′ ,, ,, ,, ,,

Therefore lines of equal dip should seem to run about 44° to south of west and dip should increase about 42′ by going 1° to N.W.

Accepting Cavendish's figures for the longitude and latitude differences, and employing the method of least squares, I get the following results:

Employing all the data, change of 24′ per 1° Lat. and of 16′ per 1° Long.
Omitting St Ives, change of 27′ per 1° Lat. and of 15′ per 1° Long.

If, confining ourselves to London, Ely and Oxford, we suppose the longitude of London 10′ in excess of the value assigned by Cavendish we obtain larger changes, viz. 34′ per 1° Lat. and 21′ per 1° Long. As it so happens, however, the differences between the directions deduced for the isoclinals from these several sets of figures amount to only a few degrees. All make the isoclinals run approximately N.E. and S.W.

If we take Rücker and Thorpe's figures for their districts III and IV, which seem the nearest comparable, we find their isoclinals running only about 16° north of east.

The number of Cavendish's stations would not warrant any great confidence in the accuracy of his result. The dip and declination results, however, appropriate to 1778 are at least consistent with a considerably more southern position of the north magnetic pole than that obtaining in 1891, so we should expect a difference between the isoclinals at the two epochs in the direction which Cavendish's figures show.

Diurnal Variation of Dip

§ 10. According to E. Walker's *Terrestrial and Cosmical Magnetism*, p. 170, the existence of a regular diurnal variation in the dip was first announced by Arago in 1827. Using a modern dip circle, an ordinary observation of dip with a single needle occupies some 20 minutes, and consists usually of 16 or more readings of each end. The accuracy expected is at the best of the order ± 1′. With a circle and needle in one position, two independent readings taken in immediate succession differ more often than not, the difference not infrequently exceeding 3′. Unless an observer were to remain almost steadily at work, the determination of the diurnal variation by complete sets of observations would hardly be feasible. Single readings taken in one position of the circle, with one end only of the needle dipping, will not give the true dip; but the differences between them are obviously capable of giving the diurnal variation, if a sufficiently high standard of accuracy is attainable. Accident, however, is likely to play too large a part, unless the observations extend over a large number of days.

It is difficult to imagine any object in taking a large number of observations at stated hours of the day, other than the investigation of the diurnal variation. A good many pages of Cavendish's manuscripts contain such observations, and it is thus reasonable to suppose that he definitely wished to find out whether a diurnal variation did or did not exist. We know that the subject had been mooted in his day, because

Walker, *loc. cit.*, p. 169, tells us that Gilpin, a contemporary of Cavendish's, had definitely pronounced against a diurnal variation.

In some of the earlier pages of the MS. there are notes of dip observations, sometimes as many as eight or nine, made at intervals throughout one and the same day. At first these showed differences amongst themselves of 15′ or 20′, the differences occurring in too erratic a way to be natural phenomena. Some improvements of instrument or method must have been introduced, because in the later pages of the MS. the differences are less, and the latest observations of the kind, which alone are allotted to a definite year, 1775, show remarkably small differences. The observations were taken with a "new" needle in the months of August, September, October and November. There was usually one morning observation between 9½ h. and 11½ h., and an evening observation between 19 h. and 20 h., or between 23 h. and 24 h. Sometimes there were evening observations at both these times, and more than one morning observation. There were also occasional observations between 14½ h. and 16½ h. The choice of hours varied from day to day.

We know now that in London the dip in the months August to October is at a maximum about 10 h., and the change between 9 h. and 11 h. is small. Again the minimum occurs in the late evening, and there is little variation between 19 h. and 24 h. Thus the hours selected by Cavendish for his observations suggest that he had recognised the existence of a diurnal variation, and knew approximately the usual times of maximum and minimum. There is, however, no direct evidence of this, and all we can now do is to show that his observations of 1775 constitute practically a demonstration of the existence of a sensible diurnal variation.

Taking all the days in which there were both morning and evening observations, and forming the difference between each morning and evening observation of the same day, I got 14 such differences in August, 13 in September, 6 in October and 2 in November. The sum of the excesses of the morning over the evening reading in these 35 cases amounted to + 66′, giving a mean excess of + 1′·89. The difference was negative in only four cases. The largest single difference was only + 7′, and only three other differences exceeded + 4′. If we take the ordinary day dip results for Kew, from the mean of the 11 years 1890 to 1900, and calculate a morning-afternoon difference, or as we shall call it a "range," from

$$\tfrac{1}{2}\{(10) + (11)\} - \tfrac{1}{4}\{(19) + (20) + (23) + (24)\},$$

where (10), for instance, signifies the dip at 10 h., we get + 1′·92 for August, + 1′·73 for September, + 1′·48 for October and + 0′·68 for November. If we derive a final mean range from these four results, by weighting the August, September, October and November values in the ratio 14 : 13 : 6 : 2, corresponding with the numbers of Cavendish's observations, we get + 1′·71. Results vary in reality from year to year, according to sun-spot frequency, the differences between successive years being often much larger than 0′·18. Thus the range given by Cavendish's observations has not merely the right sign, but is also of the right order of magnitude. His observations in October and November, when the true range is reduced, gave very small differences, which practically neutralised one another. If we had confined ourselves to his August and September observations, we should have got for the mean excess of the morning reading + 2′·56, and in no single case would the evening reading have exceeded the morning reading on the same day.

OBSERVATIONS OF MAGNETIC DECLINATION

§ 11. Cavendish's work on magnetic declination includes a certain amount of discussion of instrumental details, and a certain amount of instrumental comparison, but it is mainly concerned with the results of systematic observations. With regard to instruments, it seems only necessary to remark on the importance he attached to inverting the needle, so as to eliminate any collimation error (i.e. error arising from non-coincidence of the magnetic axis and the sighted line). Several detached sheets of MS. give particulars of mean annual values of declination deduced from the observations taken. For 1782 and later years there are in addition full particulars of the individual observations. There do not seem, however, to be any such particulars for the earliest series of observations included in the following table:

TABLE IX. Magnetic Declination in Cecil Street.

Year	Period of observation	Declination (West) ° ′
1759	June 25 to August 5	18 53·9
1766	August	20 0·0
1767	October	20 22·0
1768	August	20 34·5
1769	October	20 44·0

In the sheet containing these results Cavendish says with reference to the year 1759:

The variation (i.e. declination) by a mean of about 300 observations made in Cecil Street from June 25 to August 5 was 19° 20′·9. By two observations made at the same hours on different days by Dr Bradley at Greenwich and myself in Cecil Street it appeared that my meridian was not correct, and that I made the variation 27′ too westwardly. The true variation therefore was 18° 53′·9.

This latter is the value given in Cavendish's table from which Table IX is copied. It is added that the result for 1766 was "by a correct meridian." Cavendish further mentions that for 1767 and 1768 Mr Canton got 20° 38′ and 20° 50′ respectively—values in excess of Cavendish's by about 16′—

but, referring apparently to 1769, he adds "Mr Canton told me that he had just found that his meridian had been incorrect ever since the year 1765, and that he had now corrected it, and that he made the variation exactly the same this year as mine."

One cannot but feel some doubt whether at this early date Cavendish fully realised that declination may be expected to vary somewhat at places only a few miles apart. He leaves it uncertain whether the simultaneous observations taken by himself and Dr Bradley were astronomical or magnetic. It is only in the former case that reliance could be placed on the deduction he drew as to an error in his assumed meridian. If the difference between the declination at Cecil Street and Greenwich was half as large in 1759 as that between Kew and Greenwich now is, it would amount to about 12′. Without knowing what manner of man Mr Canton was, one cannot be certain that his discovery that his meridian was in error by the precise amount which brought his results into agreement with Cavendish's, was wholly uninfluenced by his recognition of Cavendish's ability[1]. In any case, presumably, we may assume that Cavendish adhered throughout to the meridian he finally adopted in 1759, so that the result his figures give for the secular change, viz. $+ 1^\circ\ 50'{\cdot}1$ in ten years, or an average of $+ 11'{\cdot}0$ per annum, is probably pretty exact. This, it need hardly be said, is an unusually high value.

Judging by the number of observations, some 300, made in 1759 in about six weeks, Cavendish must then have observed much more frequently throughout the day than he did in later years. His earlier work probably disclosed to him the general character of the diurnal variation, and guided him in the selection subsequently made of observation hours.

The same sheet includes certain later data for Pall Mall, viz. $21^\circ\ 13'$ for August and September 1774 and $21^\circ\ 24'{\cdot}5$ for July 1775. These were presumably taken not by Cavendish himself, but probably by Dr Heberden,

[1] Mr John Canton, the son of a broad-cloth weaver, was born at Stroud in 1718. Coming up to London in 1737, he became an assistant in the well-known Academy in Spital Square of which he ultimately became head-master. He died in 1772 in the 54th year of his age. He is described as "a man of very genteel and modest behaviour" who "gained the respect and acquaintance of the most eminent philosophers of his time." He was elected into the Royal Society in 1751, and received the Copley Medal the same year for his paper (*Phil. Trans.* Vol. 47, p. 31) on "A Method of making Artificial Magnets." He afterwards served on the Council for which he was again chosen on two subsequent occasions. He was the first in England to repeat, in 1752, Franklin's experiment of "drawing electric fire from the clouds during a thunder-storm." In 1762 he was again awarded the Copley Medal for his work on the Compressibility of Water (*Phil. Trans.* Vol. 52, p. 640). He contributed several papers on Electricity and Magnetism to the *Phil. Trans.* He was a member of a Committee appointed by the Royal Society to consider the best means of protecting St Paul's Cathedral from lightning, and his name is associated with the phosphorescent substance he prepared by calcining oyster shells with sulphur. [EDITOR.]

to whom Cavendish later refers as observing in Pall Mall[1]. One would also infer that Cavendish was inclined to apply a correction of + 15′·5 to the Pall Mall results.

The sheets already referred to in § 9 containing particulars of dip results during 1775, 1776 and 1778 in the Royal Society House also contain particulars of corresponding declination observations. These were taken at the same hours as the dip observations, viz. at 7 a.m., noon, 2 p.m. and 10 or 11 p.m. The mean from these four hours in the months of June and July would at the present epoch in the average year be about 1′·4 in excess of (i.e. more westerly than) the true mean declination for the day.

In 1775 observations were made on all days from June 18 to July 4 inclusive. The mean resulting declination was 21° 42′·9. It is added that the instrument had no error, but that a correction of about 2′·8 was necessary to allow for the effect of Dr Knight's magnets, bringing the declination up to 21° 45′·7.

In 1776 the observations extended from June 21 to July 7, and the resulting mean was 21° 47′. Nothing is said as to the necessity for any correction.

In 1778 the observations extended from June 29 to July 13. The mean for the uncorrected readings was 22° 20′, but on a separate sheet a correction of − 9′ on account of instrumental error is applied, bringing the value down to 22° 11′.

The *Philosophical Transactions* gives the mean values observed at the Royal Society's House for the years 1774 to 1780. The values it gives for 1775 and 1776 are identical with the above, if we omit the correction of 2′·8 on account of Dr Knight's magnets in 1775. But the value assigned to 1778 is only 21° 55′·5, while the values assigned to 1777, 1779 and 1780 are respectively 22° 12′, 22° 4′·5 and 22° 41′. These fluctuations are such as to preclude the possibility of high accuracy in the results.

From 1782 to 1809 Cavendish made a practice of taking daily declination observations throughout one or more of the summer months. During the earlier years 1782 to 1785 these observations were taken at his residence in Hampstead, but in 1786 and later years at Clapham, where he had gone to live. In the earlier years the number of the daily observations varied considerably, being sometimes only two, but frequently from five to eight. One of the observations was usually at an hour nor far from 8 a.m., and

[1] Dr William Heberden, eminent as a physician, was a Fellow of St John's College, Cambridge. He practised in the University for some years and subsequently repaired to London when he was elected into the Royal Society. He is best known as a medical writer, and was the first to give "a clear and satisfactory account of that painful thoracic disease called Angina pectoris." He possessed "a liberal and enlightened mind, a refined and classical taste, and an uniform complacency of disposition." He died in 1801 at the age of 91. [EDITOR.]

another between noon and 2 p.m., and there was frequently an observation late at night near 10 or 11 p.m. In the later years there were usually only two observations, and not infrequently only one each day, and the hours became much more uniform. At the same time observations tended to be slightly earlier in the day in some years than in others.

In the earlier years Cavendish took the largest and least of the daily readings, irrespective of when these occurred, and formed them into two separate groups. From the mean of the greatest and the mean of the least he derived a mean which he regarded as a measure of the declination, except for instrumental error. During part of the time he observed with the marked face of his needle up, and during the remainder of the time with the marked face down. Combining the mean reading mark up (representing the mean of the corresponding greatest and least readings) with the mean mark down, he obtained his final value for the declination. The greatest reading practically always was a reading taken between noon and 2 p.m., and the least reading was in the great majority of instances observed between 7 a.m. and 9 a.m. Again in the earlier years the needle was inverted at a purely arbitrary date, or dates, no distinction being drawn between days in different months. For instance in 1782 observations commenced on June 26 with mark upwards. The needle was inverted on July 4, and again on July 16. The observations from June 26 to July 4, and from July 16 to 26, were combined in one group representing mark up, while those from intermediate days represented mark down. One final mean was then obtained representing the whole series of observations.

After 1788 it was usual to form more than one mean, but still during a good many years without regard to calendar months. Thus in 1789 the days of observation formed three groups, the first including April 23 to May 15, the second May 15 to June 5 and the third June 5 to June 27. By that time Cavendish seemed to have assured himself that in his own instrument the difference between mark up and mark down was insignificant, and whilst in every year the needle was inverted at least once, he did not apparently consider it essential to have mark up and mark down readings represented in every mean. Thus in 1789 all the readings between April 23 and May 15, and again all those between June 5 and June 27 were taken with mark down, while those taken between May 15 and June 5 were taken with mark up. Thus two of his three groups represented mark down exclusively and only one mark up, but in forming a mean for the year he gave equal weight to each of the three groups.

For 1800 and later years Cavendish in forming his groups recognised the calendar months, and it is not clear whether for a considerable number of years previously he had made a practice of inverting his needle or not.

In the latter part of 1803 Cavendish got a new instrument, and later he seems to have come to the conclusion that the results he obtained with it in 1804 and 1805 were unreliable. At all events he does not include mean results from those years in his tables. From 1806–9 he adopted a considerably more elaborate method of reduction, which experiments made on his new compass had shown to be expedient. This involved readings with the mark—or as he then called it "knob"—both up and down.

The ordinary procedure became eventually a reading about 8 a.m. called a "morning" reading, and a reading about 1 p.m. called an "afternoon" or "evening" reading. Means were derived from morning and evening readings grouped separately, and then a final mean from the morning and evening means. The change of terminology from "greatest" and "least" to "morning" and "evening" seems to have occurred in 1799, but the approach to a uniform practice of morning and afternoon readings was made gradually, anterior to that date.

The time of the "least" or "morning" reading tended on the whole to become later, but fluctuated from year to year. It averaged in 1782 about 7 h. 45 m., in 1786 about 8 h. 10 m., in 1790 about 8 h. 40 m., in 1795 about 8 h. 10 m., in 1800 about 8 h. 20 m., and in 1805 about 8 h. 30 m. During the subsequent years it was about 8 h. 30 m., but in 1809 was 8 h. 50 m.

The time of the "greatest" or "evening" reading varied a good deal on individual days in the earlier years, but the average time did not fluctuate very widely. It was in 1782 about 1.30 p.m., the same in 1786, in 1790 about 1 p.m., in 1795 about 1.30 p.m., in 1800 about 1.40 p.m., but in 1801 and 1802 about 1 p.m. In 1805 and later years the time was very uniform at about 1 p.m.

The maximum, i.e. extreme westerly value, in the regular diurnal variation of declination occurs in London between 1 and 2 p.m. G.M.T., the whole year round. Near the hour of maximum the rate of change is very small, and thus the fluctuation in the time of Cavendish's afternoon reading would have little effect.

The time of the minimum in the diurnal variation is more variable. Near midwinter the forenoon minimum is as late as 9 a.m., but from May to September it occurs at 7 a.m., or at least nearer 7 than 8 a.m. Thus even in the years when Cavendish's morning observations were earliest the minimum was already past on the average day, and in the years when his observations were latest the rise since the minimum was quite appreciable.

Another feature of the diurnal variation is that it is not symmetrical. The maximum departs more from the mean for the day than does the minimum. The consequence of this will be more clearly apparent on considering the following results representing an average from Kew data of the eleven years 1890 to 1900. They may be regarded as representative of the average year in London at the end of last century, and there is no reason to suspect any very large change in such a phenomenon as the general character of the regular diurnal variation in the course of a single century.

TABLE X. Influence of hours of observation on value of Declination.

	Excess above true mean for the day				
	May	June	July	August	September
	′	′	′	′	′
Mean from 7 a.m. and 1 p.m.	0·9	0·5	0·7	1·3	1·7
„ „ 8 „ „ „	1·0	0·6	0·9	1·5	1·7
„ „ 9 „ „ „	1·7	1·2	1·4	2·2	2·3

It is clear that the mean obtained by Cavendish may be expected to be in every case slightly in excess of the true mean, i.e. the mean that would be obtained from hourly readings extending over the whole day. Also the excess will naturally be larger the later the average time of the morning reading. It will also be noticed that the excess may be expected to be a minimum in June and to increase from July to September. It is greater, it may be added, in years of many sun-spots, when the diurnal range is specially large, than in years of few sun-spots.

Supposing declination to be increasing at the time, as was the case during the years of Cavendish's observations, the true value would naturally increase from June to September. We should thus have the natural effect and the observational effect conspiring to raise the apparent value of the declination from one month to the next.

One peculiarity of Cavendish's treatment remains to be mentioned. In a minority, but still not a negligible minority of cases, at least in some months and years, he had only one observation, either morning or evening, in the day. These single observations were used when forming the morning and evening means, i.e. certain days were represented only in one of the two categories. It may seem at first sight that this is immaterial. It would be so if every day of the month were identical with every other day, but this is not the case. In the first place the type of the regular diurnal variation is constantly varying. Thus if we had a majority of morning observations from say the first half of the month, and a majority of evening observations from the second half, it would clearly be unsatisfactory. In the second place, there is the uncertain influence of disturbance. Only five hours intervene between an 8 a.m. and a 1 p.m. observation taken on the same day, as compared with 19 hours between a 1 p.m. and an 8 a.m. observation on consecutive days. The influence of disturbance is more to be apprehended in the second case than in the first.

These remarks will, I hope, explain why it seemed expedient to derive a new set of means from Cavendish's observations, treating the calendar months separately, and including only those days when there were both morning and afternoon readings. Table XI gives Cavendish's own results, and Table XII the new results. The figures in Table XI are not in every case absolutely identical with those Cavendish himself gives. In some cases I came across arithmetical slips, which it seemed well to correct. But these were all comparatively trifling, and the differences from Cavendish's figures were seldom as large as 0'·2.

In Table XI, when Cavendish treated all the data of the year together, obtaining only one mean, that mean appears only in the column devoted to the mean for the year. In all other cases the mean for the year is simply the arithmetic mean of the means for the two or more sub-periods under which Cavendish grouped his observations. These yearly means appeared in most years in a MS. table which Cavendish had drawn up. When they were lacking I have taken the arithmetic mean of the mean values for the sub-periods, as being the method approved by Cavendish. The tendency for the means for the later periods to exceed those from the earlier periods of the same year, which we were led to anticipate, will be readily recognised. There are in fact few years where it does not manifest itself.

TABLE XI. Declination Results at Hampstead and Clapham.

Year	Date				Mean declination from: Groups or months ° '	Mean declination from: Years ° '
1782	June	26 to	July	24		22 36·8
1783	June	24 ,,	July	15		46·9
1784	May	16 ,,	June	23		54·1
1785	May	21 ,,	July	10		23 3·3
1786	July	10 ,,	August	6		12·5
1787	May	4 ,,	June	6		15·3
1788	May	2 ,,	May	29		23·1
1789	April	23 ,,	May	15	23 28·5	
,,	May	15 ,,	June	5	31·5	
,,	June	5 ,,	June	27	30·3	30·1
1790	May	18 ,,	June	28		36·6
1791	May	20 ,,	May	31	39·3	
,,	June	2 ,,	June	11	39·1	
,,	July	2 ,,	August	3	41·0	39·8
1792	June	16 ,,	July	8	48·4	
,,	July	8 ,,	August	3	49·7	
,,	August	3 ,,	August	29	50·3	49·5
1793	July	5 ,,	July	21	52·1	
,,	July	21 ,,	August	9	52·8	52·5
1794	June	18 ,,	July	9	55·8	
,,	July	10 ,,	August	12	57·7	
,,	August	12 ,,	August	27	58·6	57·4

TABLE XI. Declination Results at Hampstead and Clapham—*contd.*

Year	Date					Mean declination from Groups or months		Years	
						°	′	°	′
1795	July	7	to July	25			56·2		
,,	July	26	,, August	26			59·0		
,,	August	27	,, October	1		24	0·7		58·6
1796	June	28	,, July	27		23	59·3		
,,	July	27	,, August	20		24	0·6		
,,	August	20	,, September	21			2·2	24	0·7
1797	July	11	,, August	12			0·8		
,,	August	12	,, September	12		24	1·6	24	1·2
1798	May	29	,, June	23		23	59·3		
,,	June	24	,, July	31		24	1·0		
,,	July	31	,, August	31			2·6		
,,	September	1	,, October	1			3·7	24	1·6
1799	July	12	,, August	24					0·6
1800	July						2·6		
,,	August						4·1		
,,	September						4·5		3·7
1801	June						4·5		
,,	July						6·6		
,,	August						7·7		
,,	September						8·6		6·8
1802	June	23	to July	31			7·0		
,,	August						9·0		
,,	September						10·1		8·7
1803	June						8·1		
,,	July						9·2		
,,	August						12·0		
,,	September						11·6		10·2
1806	August						15·3		
,,	September						16·1		15·7
1807	June						16·0		
,,	July						16·5		
,,	August						17·3		16·6
1808	June					24	15·4		
,,	July						16·2		
,,	August						17·6		
,,	September						18·7	24	17·0
1809	June						16·1		
,,	July						19·4		
,,	August					24	19·6	24	18·4

TABLE XII. Declination Results at Hampstead and Clapham.

Year	Month	Number of days of observation	Mean declination For month	Mean declination For year
			° ′	° ′
1782	June	4	22 37·3	
,,	July	20	36·7	22 36·8
1783	June	6	46·9	
,,	July	13	46·8	46·9
1784	May	4	54·3	
,,	June	5	54·8	54·6
1785	May	5	23 2·8	
,,	June	3	4·2	23 3·3
1786	July	7	12·5	
,,	August	2	17·2	13·5
1787	May	23	15·4	
,,	June	3	16·0	15·5
1788	May	22		22·7
1789	May	23	30·0	
,,	June	19	30·3	30·0
1790	May	9	37·1	
,,	June	21	36·5	36·7
1791	May	8	39·5	
,,	June	3	39·0	
,,	July	8	40·5	40·0
1792	June	8	48·3	
,,	July	9	49·4	
,,	August	3	50·2	49·1
1793	July	18	52·1	
,,	August	8	54·0	52·7
1794	June	5	55·4	
,,	July	18	56·3	
,,	August	8	57·9	56·9
1795	July	16	56·7	
,,	August	14	59·8	
,,	September	25	24 0·7	59·3
1796	June	2	23 57·8	
,,	July	20	59·6	
,,	August	21	24 1·3	
,,	September	12	2·0	24 0·7
1797	July	7	0·5	
,,	August	18	1·8	
,,	September	6	24 1·2	24 1·3
1798	June	24	23 59·6	
,,	July	18	24 1·2	

TABLE XII. Declination Results at Hampstead and Clapham—*contd*

Year	Month	Number of days of observation	Mean declination For month		Mean declination For year	
			°	′	°	
1798	August	26	24	2·7		
,,	September	19		3·5	24	1·7
1799	July	6		0·3		
,,	August	4		0·7		0·5
1800	July	22		2·8		
,,	August	21		4·2		
,,	September	10		4·8		3·7
1801	June	16		4·4		
,,	July	17		6·4		
,,	August	22		7·6		
,,	September	24	24	8·5	24	7·0
1802	June	7	24	5·5		
,,	July	20		7·3		
,,	August	27		9·1		
,,	September	21		10·2	24	8·7
1803	June	13		8·4		
,,	July	22		9·0		
,,	August	25		11·9		
,,	September	24		11·5		10·5
1804	September	26				12·6
1805	June	21		9·9		
,,	July	19		10·5		
,,	August	23		9·9		
,,	September	23		8·6		9·7
1806	August	24		15·3		
,,	September	24		16·1		15·7
1807	June	22		16·0		
,,	July	24		16·5		
,,	August	24		17·3		16·7
1808	June	28		15·4		
,,	July	18		16·2		
,,	August	24		17·6		
,,	September	25		18·7		17·0
1809	June	22		16·1		
,,	July	15		19·4		
,,	August	21	24	19·6	24	18·2

Table XII gives the number of days on which each monthly mean is based. In it the mean value for the year was arrived at by allowing equal weight to each individual day. The days, in fact, were put in a single group irrespective of the months they belonged to. In some years, e.g. 1789, there were months containing only one or two days of observation, from which satisfactory monthly means could not have been derived. These solitary days were used, however, in forming the annual means, so that these means were based in some cases on a slightly larger number of days than contributed to the monthly means. As already explained, days with only one observation were excluded, so that the number of days on which Cavendish observed exceeded that shown in all years. In most cases, especially in the later years, the differences between the yearly means in Tables XI and XII are very small; in a good many cases there is absolute identity. The absolutely largest difference, occurring in 1795, is only 0′·7. In that year Cavendish's third group included about twice as many days as his first, and the mean values he obtained from the two differed by 4′·5. Thus it naturally makes a considerable difference whether one allots equal weight to individual groups or to individual days.

As already stated, the place of observation was changed from Hampstead to Clapham between the 1785 and 1786 observations, and the change of site might of course have entailed a very decided discontinuity in the figures. The mean annual values, whether in Table XI or Table XII, do in fact at first sight rather suggest a small discontinuity. Taking Table XI, it will be seen that the apparent secular change from 1785 to 1786 was 9′·2, while the mean annual change from 1782 to 1785 was only 8′·8. The secular change, however, from 1786 to 1787 was only 2′·8 as compared with 7′·8 in the following year. Thus the most probable explanation seems to be an unduly high value for 1786 unconnected with the change of site, or else an abnormally low secular change between 1786 and 1787. It happens fortunately that some additional light on this point is derivable from data given in one of Cavendish's MS. They represent the mean annual values obtained by an independent observer, Dr Heberden, who observed from 1782 to 1791 in Pall Mall, presumably on one spot.

In the event of any substantial difference between the values of the

TABLE XIII. Variation (Declination) in Pall Mall by Dr Heberden.

Year	From	To	Heberden	Cavendish less Heberden
			° ′	′
1782	May 20	June 24	22 22·5	14·3
1783	June 10	June 25	34·7	12·2
1784	May 12	May 28	40·4	13·7
1785	May 7	May 23	49·2	14·1
1786	May 18	May 27	23 0·1	12·4
1787	May 3	May 21	2·0	13·3
1788	April 22	May 7	7·0	16·1
1789	May 8	May 15	19·8	10·3
1790	May 12	May 22	32·1	4·5
1791	May 12	May 26	25·5	14·3

declination at Cavendish's places of observation at Hampstead and Clapham, the difference between his annual means and Heberden's should have exhibited a decided change after 1785. On the contrary, if we compare the mean difference from the preceding years with that from the succeeding years, we reach different conclusions as to the sign of the difference between Hampstead and Clapham according as we derive means in each case from three or from four years. Heberden's results, equally with Cavendish's, show an exceptionally large change between 1785 and 1786, and an exceptionally small one between 1786 and 1787. Considering the nature of the observations, the uniformity of the difference between Cavendish and Heberden, with the exception of the one year 1790, is really surprising. In that year presumably some error entered into Heberden's observations, as the reversion in an isolated year, 1790–1791, of the secular change is a highly improbable event at an epoch when that change is substantial.

Reference has already been made to declination data from Pall Mall for 1774 and 1775, and to the fact that Cavendish seemed to regard them as requiring a correction of $+ 15'{\cdot}5$ to make them comparable with his own earlier observations in Cecil Street. Unless Heberden changed his instrument or methods between 1775 and 1782, the natural inference would be that the Cecil Street, Hampstead and Clapham results were all fairly comparable, the difference of site representing not more than $2'$ or $3'$ of declination.

Secular Change of Declination

§ 12. Table XIV presents the secular change data derivable from Tables XI and XII. The figures opposite any year represent the increase of declination during the previous twelve months. A minus sign indicates a fall. The first five columns of secular change data give the results obtained by comparing the mean values of the declination for months of the same name in consecutive years as given in Table XII. The sixth column gives the mean obtained by allowing equal weight to each of the monthly results. The seventh column gives the secular change derived from the mean results for the year in Table XII, and the last column the corresponding results derived from Cavendish's yearly values in Table XI.

A comparison of the corresponding figures in the last three columns of Table XIV will give an idea of the order of the uncertainties arising from variations in the method of handling the observational results.

Cavendish himself formed no annual means for 1804 or 1805. Thus no figure is entered under either of these years in the last column of Table XIV and the figure assigned to 1806 really represents ⅓ of the apparent rise between 1803 and 1806.

While I have given in Table XII the values which Cavendish's observations, treated in the same manner as in other years, give for the declination

TABLE XIV. Secular change of Declination in London, 1782–1809.

Year	From monthly mean values						From all observations of year	From Cavendish's yearly means
	May	June	July	Aug.	Sept.	Mean		
	′	′	′	′	′	′	′	′
1783		9·6	10·1			9·9	10·1	10·1
1784		7·9				7·9	7·7	7·2
1785	8·5	9·4				9·0	8·7	9·2
1786							10·2	9·2
1787							2·0	2·8
1788	7·3					7·3	7·2	7·8
1789	7·3					7·3	7·3	7·0
1790	7·1	6·2				6·6	6·7	6·5
1791	2·4	2·5				2·4	3·3	3·2
1792		9·3	8·9			9·1	9·1	9·7
1793			2·7	3·8		3·2	3·6	3·0
1794			4·2	3·9		4·1	4·2	4·9
1795			0·4	1·9		1·2	2·4	1·2
1796			2·9	1·5	1·3	1·9	1·4	2·1
1797			0·9	0·5	− 0·8	0·2	0·6	0·5
1798			0·7	0·9	2·3	1·3	0·4	0·4
1799			− 0·9	− 2·0		− 1·4	− 1·2	− 1·0
1800			2·5	3·5		3·0	3·2	3·1
1801			3·6	3·4	3·7	3·6	3·3	3·1
1802		1·1	0·9	1·5	1·7	1·3	1·7	1·9
1803		2·9	1·7	2·8	1·3	2·2	1·8	1·5
1804					1·1	1·1		
1806				1·1	1·5	1·3	1·7	1·8
1807				2·0		2·0	1·0	0·9
1808		− 0·6	− 0·3	0·3		− 0·2	0·3	0·4
1809		0·7	3·2	2·0		2·0	1·2	1·4
1782 to 1791		6·9	7·1				7·0	7·0
1791 to 1800			2·5				2·6	2·7
1800 to 1809			1·8	1·7			1·6	1·6

in 1804 and 1805, I am inclined to regard the data for 1805 as untrustworthy. The data for 1804 have been used in obtaining the secular change assigned to that year in Table XIV, but in deducing from Table XII the secular change for 1806 I discarded the results for 1804, employing as in the case of Cavendish's own results the data for 1803.

The last three lines give mean results for the three 9-year periods into which the 27 years, 1782 to 1809, can be subdivided.

Even at a modern first-rate observatory it is undoubtedly an optimistic view to regard mean annual values of declination as reliable to 0′·1. Thus to look for that degree of accuracy in the secular change data derivable from Cavendish's observations would be utterly unreasonable.

It will be remembered (see p. 473) that Cavendish's observations in Cecil Street gave + 11′·0 as the mean value of the secular change between 1759 and 1769. If we combine his mean annual values for 1769 in Cecil Street and for 1782 in Hampstead we obtain as the mean secular change during these 13 years + 8′·7. Heberden's results in Pall Mall from 1782 to 1791 give the same mean value + 7′·0 as Cavendish's corresponding figures given in Table XIV. Thus all the results available point to a decline, comparatively slow at first and then more rapid, in the secular change. The rate of decline however fell off after 1800, and we know from other sources that the actual turning point when the declination reached its extreme westerly value was not attained near London until about 1818.

Diurnal Variation of Declination

§ 13. Table XV gives particulars of the ranges obtained from Cavendish's "greatest" and "least" or "morning" and "evening" readings, making use only of those days in which both morning and evening readings were available. The results for the five months May to September are given separately. The column headed "all days" gives the results obtained by combining all the observation days of the year in one group. In the average year there is not very much difference between the ranges of the regular diurnal variation in May, June, July and August. There is a decided though not very large falling off in September. The last column gives the sun-spot frequency for the year taken from Wolfer's great table.

In the later years when the observation hours tended to become stereotyped the range in Table XV should represent pretty nearly that obtained from the values assigned to the two hours 8 a.m. and 1 p.m. or 2 p.m. in the regular diurnal inequality, and should thus be just a shade under the range of the regular diurnal inequality itself. In the earlier years the larger number of daily observations would tend somewhat to increase the range, because the hours at which the extreme values appear fluctuate from day to day. On the other hand, the hours at which the extreme values were most likely to occur were often not the actual hours of observation chosen.

For comparison, I have given in the last three lines the ranges of the diurnal inequality at Kew in May and June, and the means derived from these two months, for the three years 1890, 1893 and 1870. Of these 1890 represents, like 1784, 1798 and 1809, sun-spot minimum, while 1893 and 1870 represent, like 1787 and 1804, sun-spot maximum.

As is well known, the fact that the range of the regular diurnal variation tends to be small in years of few sun-spots, and large in years of many sun-spots, was discovered about the middle of last century by Lamont and Wolf. It is obvious from Table XV that if he had had sun-spot data before him, Cavendish might well have discovered the result prior to the end of the eighteenth century. It would be very interesting to know what

TABLE XV. Daily Ranges from observations at Hampstead and Clapham.

Year	May	June	July	August	September	All days	Sun-spot frequency
	′	′	′	′	′	′	
1782		13·5	13·5			13·5	38·5
1783		13·2	14·0			13·7	22·8
1784	13·5	11·6				12·4	10·2
1785	14·0	12·3				13·4	24·1
1786			18·3	10·5		16·6	82·9
1787	20·1	15·5				19·6	132·0
1788	19·8					19·8	130·9
1789	14·5	12·9				13·8	118·1
1790	13·3	12·0				12·4	89·9
1791	12·8	13·0	12·4			12·8	66·6
1792		12·1	13·8	12·0		12·8	60·0
1793			12·4	12·4		12·4	46·9
1794		9·5	11·5	10·1		10·8	41·0
1795			7·8	9·0	9·1	8·7	21·3
1796		5·5	7·7	8·6	10·0	8·5	16·0
1797			7·5	8·9	9·2	8·6	6·4
1798		8·7	8·9	9·3	9·8	9·1	4·1
1799			9·3	8·8		9·1	6·8
1800			8·7	9·4	6·7	8·6	14·5
1801		8·5	9·0	11·3	10·5	10·0	34·0
1802		8·7	10·2	10·4	10·0	10·1	45·0
1803*		11·0	10·8	10·8	10·1	10·6	43·1
1804					10·1	10·1	47·5
1805		9·9	10·5	9·9	8·6	9·7	42·2
1806		11·4		10·7	9·2	10·5	28·1
1807		8·9	7·4	9·6		8·6	10·1
1808		9·6	9·7	10·3	9·4	9·7	8·1
1809		7·4	8·0	6·2		7·1	2·5
1890	8·6	9·1				8·8	7·1
1893	13·3	13·9				13·6	84·9
1870	16·7	15·1				15·9	139·1

* The mean range from 17 October days in 1803 was 8′·0.

impression the enormous size of the ranges which he was recording near sun-spot maximum in 1787 and 1788 made upon his mind.

It will be generally admitted that if Wolfer's table can be at all relied on very large ranges were to be expected in 1787 and 1788, but the fact that the ranges then observed by Cavendish are so largely in excess of those for 1870, a year apparently of even larger sun-spot frequency, may rouse suspicion. According to observations of recent years the range of the diurnal inequality of declination bears a linear relationship to sun-spot frequency, so that for equal increments in the one we expect at least approximately equal increments in the other. This refers, however, to the mean diurnal inequality for the whole year. Sun-spot frequency varies not merely from month to month but from day to day. The relation between the magnetic diurnal range and sun-spot frequency is doubtfully shown on individual days, and imperfectly shown in individual months. In a year of sun-spot maximum we confidently expect that the range of the diurnal magnetic inequality will be well above the average in every month of the year, but we expect this excess to be considerably larger in some months than in others. This is apparent even in the few recent data in Table XV which are based on magnetograms. If we calculated the range for 1870 from those for 1890 and 1893 on the simple basis of sun-spot frequency we should get a range fully 1′ in excess of that actually found. Still the excesses of the ranges in 1787 and 1788 are undoubtedly difficult to account for in this way. There is, however, a very obvious explanation of the difficulty, though it cannot claim to be more than a probability. The relation established between declination range and sun-spot frequency is based on a comparison of years of many and few sun-spots having approximately the same mean epoch. It thus relates to years which are closely alike if not identical so far as the absolute values of the magnetic elements at the place are concerned. But it is a very different matter when we come to comparing years of sun-spot maximum such as 1893 and 1787 separated by over a century. The most generally accepted theory as to the ultimate cause of the regular diurnal magnetic variation is that it consists of electrical currents in the upper atmosphere, assisted probably by earth currents. But if we suppose the diurnal variation to be due to currents situated either in the air or the earth, then if the currents remain the same, the range will vary inversely as the value of H, the horizontal force. The accurate measurement of horizontal force dates only from the epoch of Gauss and Lamont, so we have no direct knowledge of the value of H in London in, say, 1787. If, however, we consult the Kew records we find that the average yearly rise in H from 1860 to 1865 was identical with the average yearly rise from 1860 to 1900. If we suppose the force to have had the same mean annual change since 1787, we find that while the value of H in 1893 was 0·18238 C.G.S. units, in 1787 it was only 0·15906. Even this would seem too high an estimate, for if we combine it with a dip of 72°—the approximate value deduced for 1787 from Cavendish's observations of 1791 and the secular change given by it and his other observations—we obtain for the vertical force in 1787 a value which would require its mean annual fall from 1787 to 1860 to have been about three times that observed for the average year between 1860 and 1865. If, on the other hand, we assume that the rate of fall of the vertical force was the same from 1787 to 1860 as it was from 1860–1865, we find for the vertical force in 1787 a value which combined with a dip of 72° makes H as low as 0·147. As the rate of fall of V diminished very decidedly after 1865, this latter estimate of H deserves considerably less weight than the first. If we suppose the value of H to have been 0·155 C.G.S. units in 1787 we shall probably not be very far wrong. But 0·155 is only 0·85 of the value of H in 1893. Thus if the declination range varies inversely as the value of H at the time, a range of 19′·6 in 1787 would, under the conditions as to force existing in 1893, have been only 19′·6 × 0·85, or 16′·7. This seems quite reasonable for a sun-spot frequency of 132.

According to the Schuster-Balfour Stewart theory the intensity of the electrical currents in the upper atmosphere varies as the value of the vertical force. If this be true, it would naturally mean larger currents in 1787 than in 1893, and so tend equally with the reduced value of H to enhanced ranges at the earlier date.

As the criticism was most likely to originate with theorists, I have thought it well to treat this aspect of the case in some detail. Probably, however, the plain man will derive more confidence from the very strong confirmatory evidence of the accuracy of Cavendish's range figures which is fortunately derivable from data recorded in his MSS. He gives in the first place the morning and afternoon declination readings taken in Pall Mall, presumably by Dr Heberden, from May 4 to May 21, 1787. Taking all Dr Heberden's 18 days we obtain for the mean range 19′·7, Cavendish's own mean result for the same month as given in Table XV being 20′·1. Fifteen of the 18 days on which Heberden observed were also days on which Cavendish had both morning and afternoon readings. The mean ranges for these 15 days were Heberden 20′·1, Cavendish 21′·7. Cavendish's observations were not at absolutely fixed hours, and very probably the same was true of Heberden's, so absolute identity is not to be expected. Considering the nature of the instruments we should not expect any very high degree of accuracy in individual readings, and high precision is not to be expected in the ranges for individual days. But errors in defect would be as likely as errors in excess, thus some conclusions can be drawn from the individual data. A large mean range might signify that the majority of individual ranges were large, or that there were a few very large ranges. The latter case would mean the existence of days of large disturbance, the former that the regular diurnal variation was specially developed. Of the 18 ranges observed by Heberden the least was 13′

and the largest 29′, while ten lay between 17′ and 23′. Of the 23 ranges observed by Cavendish, in the same month, the least was 13′·5 and the largest 27′·5, and 13 lay between 16′·5 and 23′·5. Thus the phenomenon was clearly not due to the occurrence of a small number of highly disturbed days, but to a persistently large range in the regular diurnal variation.

Further evidence tending in the same direction is derivable from the readings taken four times a day at the Royal Society's House. One of the years included, 1778, is the year of absolutely largest sun-spot frequency in the whole of Wolfer's list. Again Cavendish's MSS. include particulars of observations taken in 1788, a year nearly equal to 1778 in sun-spot frequency, during 12 hours on every single day of June. Where and by whom these observations were taken is not stated; the only information respecting them is the remark "Mr Gilpin's obs(erving) needle" on the back of the single sheet of paper on which they are entered. The presumption is that they were taken by Cavendish himself, or under his supervision, somewhere in London. Table XVI embodies the information derivable from these various sources as to the nature of the regular diurnal variation. The results are given as excesses above the value at 7 a.m., the natural hour of the daily minimum in summer. For comparison, corresponding data are given for 1890 and 1893 at Kew, and the mean sun-spot frequencies given in Wolfer's table for the several years are added. The results for 1775, 1776 and 1778 include days from July as well as June, the results for the other years are from June only.

TABLE XVI. Diurnal Variation of Declination in London.

	Forenoon				Afternoon							
Year	7	8	10	Noon	1	2	4	6	8	10	11	Sun-spot frequency
	′		′	′	′	′	′	′	′	′	′	
1775	0·0			9·1		10·6				2·0		7·0
1776	0·0			7·5		8·0					2·0	19·8
1778	0·0			16·7		19·0				8·5		154·4
1788	0·0	0·6	8·3	17·3	19·2	19·2	13·3	7·4	6·2	5·8	5·7	130·9
1890	0·0	0·3	3·3	8·0	8·9	9·1	7·1	5·1	4·2	3·8	3·7	7·1
1893	0·0	0·4	5·0	12·2	13·4	13·9	10·8	7·5	6·0	5·5	5·2	84·9

In 1776 individual readings at the Royal Society's House were more than usually erratic. The diurnal inequality got out by Cavendish from all the observations comes

7 a.m	Noon	2 p.m.	11 p.m.
0	4′	4′	− 6′

Evidently Cavendish was not satisfied, as he got out a second set of figures, confining himself to observations made by "Young Rob(erton)." It is from these that the results in Table XVI were derived. There were however only five such readings at 7 a.m., and one of them must either have been in error or taken at a considerably disturbed time, as it made the declination 10′ *higher* than at noon and 9′ *higher* than at 2 p.m. If this reading were omitted—it is 19′ higher than any other reading taken by Roberton at the same hour—the figures for 1776 at noon, 2 p.m. and 11 p.m. would each be increased by 4′. This would obviously fit in much better with the other results. If this emendation is accepted, the phenomena exhibited in the older years are so similar to those derived from magnetographs in recent years that their substantial accuracy can hardly be questioned. Whatever the true explanation may be, the large size of the regular diurnal variation in 1778 and 1788 can hardly be doubted.

The hours of observation in 1788 included 6 a.m., and the mean value for that hour was 0′·1 lower than that for 7 a.m., so that the range of the regular diurnal inequality in June 1788 was actually 19′·3. The mean absolute range in that month, i.e. the mean of the ranges derived from the highest and lowest reading of each day, irrespective of the hour of occurrence was 20′·4. It was again unquestionably a case not of a few highly disturbed days with abnormally large ranges, but of persistently large regular diurnal variation throughout the month. The 30 ranges varied only from 14′ to 27′, and 23 of them lay between 17′ and 23′.

Disturbed Days

§ 14. In England, 4 a.m. to 2 p.m. is the time of the day when magnetic disturbance is least common, thus when daily observations are confined to 8 a.m. and 1 p.m., or hours approximating thereto, as was the case in many years with Cavendish's observations, many days may appear quiet which were in reality considerably disturbed. But when observations are taken at 10 or 11 p.m. as well, as was the case in 1776 and 1778, and still more so when they are taken twelve times a day as in June 1788, the chance of any considerable disturbance failing to show itself is not very large. It is a pretty common belief, though not a very well-grounded one—1893 for instance was a conspicuous example to the contrary—that years of exceptionally large sun-spot frequency are years when magnetic disturbances are specially large and numerous. Thus the presence or absence of specially disturbed conditions in years of such abnormal sun-spot development as 1778, 1787 and 1788 is of considerable interest. As regards 1788, the observations with Gilpin's needle establish beyond a doubt that the month of June was a distinctly quiet month, and there are no special indications of disturbance in the observations available for 1778 and 1787. It cannot of course be safely inferred from the character of one or two months what was the character of the whole year. But it can at least be said that while there is conclusive evidence that the regular diurnal variation was abnormally large in 1778, 1787 and 1788, the evidence so far as it goes is against any special development of magnetic disturbance. This does not mean of course that disturbance was non-existent. That is practically never the case, a certain amount of disturbance being the rule rather than the exception. There were undoubtedly some days of considerable disturbance in these years, and one such case, May 24, 1788, is clearly indicated by Cavendish's observations. He observed five times that day. The readings he got at 11.20 a.m. and 1.10 p.m. were identical—in itself a sign of disturbance—and they each exceeded the reading at 7.50 a.m. by 40′. It was a time of large regular diurnal variation, still this represents a considerable disturbance. Again, on the 13th of the same month declination was lower at 5.20 p.m. and at 7.50 p.m. than at 8.25 a.m.—by 5′ and 10′ respectively—whereas on a normal day the difference should have been considerably the other way.

The following are the principal other occasions of disturbance which I have noticed:

On July 20, 1782 declination fell 24′ between 2.45 p.m. and 10.20 p.m. and rose 6′ between 10.20 p.m. and 11 p.m. The observer enters "auror(a)" against the 10.20 and 11 p.m. observations. Such casual notes are very rare, but whether Cavendish associated the disturbance with the aurora, or merely recorded the presence of aurora as a very rare event in London in July, we cannot be certain. The disturbance continued until the 21st, declination *falling* 4′ between 7.40 a.m. and 9.20 a.m., and being 6′ higher at 7.40 a.m. than at 7.30 p.m.

On May 23, 1789, the reading at 9 a.m. was 3′ *higher* than at 4.45 p.m., and only 4′ lower than at 1.30 p.m.

On June 21, 1790, the reading at 12.40 p.m. was only 2′ higher than at 8.50 a.m. instead of 12′ as usual.

On July 5, 1794, the readings at 8.10 a.m. and 1.40 p.m. differed by only 0′·5, instead of 11′½ as usual.

On August 30, 1795, the readings at 8 a.m. and 1.10 p.m. differed by only 1′·5, instead of 9′ as usual.

On August 20, 1796, the reading at 12.30 p.m. was about 10′ higher than usual, making the range for the day fully double the normal.

On July 19, 1800, the reading at 8.30 a.m. exceeded that at 12.40 p.m. by 4′·5, but Cavendish has drawn his pen through it, so it may have been considered doubtful.

On August 23, 1800, the reading at 2.30 p.m. was only 2′ higher than that at 8.30 a.m., instead of 9′½ as usual.

On July 3, 1801, the reading at 1 p.m. was only 1′ higher than that at 8.30 a.m., instead of 9′ as usual.

In 1804 the excesses of the afternoon over the morning readings on September 6 and 20 were respectively only 0′·6 and 2′·3, instead of 10′ as usual.

On September 21, 1805, the afternoon reading was only 0′·5 higher than the morning reading, instead of 9′ as usual.

On August 23, 1806, declination was 6′·8 *lower* at 1 p.m. than at 8.10 a.m., and on July 5, 1808, it was 0′·2 *lower* at 1 p.m. than at 8.50 a.m.

On August 25, 1809, declination was only 1′·4 higher at 1 p.m. than at 9 a.m., instead of 6′ as usual.

The above includes all the cases where I observed the range for the day to fall below 3′, but ranges under 6′ in summer months may certainly be regarded as abnormal, and the number of ranges between 3′ and 6′ was considerably greater than that of ranges below 3′. Allowance must, however, be made for the fact that errors of 1′ or more must frequently have occurred with an instrument such as Cavendish used, even in the most skilled hands, so that the possibility of not infrequent errors of ± 2′ in the daily range must be borne in mind.

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